ABSTRACT

HISTORICAL EXPLANATION

AND VALUE - NEUTRALITY

by James J. Leach

recent emergence of philosophy of history as a criti-
e, attention focuses on the notion of objective his-
nation. Three problems are central: the meaning of
xplanation,' the relationship between scientific and
planation, and the kind of objectivity appropriate to
ically, the issues turn on the covering law theory of
We defend this theory, propounded by K. Popper and C.
st the diverse challenges of Weber, Collingwood, Lavine,
Donagan and Scriven. Briefly characterized, the theory
ntific and historical explanations on the model of log-
ion: deductive or inductive. Rationally acceptable
must incorporate, as essential premises, empirically
true (or highly confirmed) general laws.
t the issues, we critically explore three interrelated
ed by Max Weber and pivotal to the controversy: that
quiry must be objectively value-free (Value-Neutrality
objective cultural explanation, nevertheless, requires
terpretative understanding beyond mere subsumption
nderstanding thesis); and finally that such understand-
tes a peculiarly cultural method of concept formation
hesis). Each faction to the covering law controversy

accepts at least one of these theses. In rejecting all three, we attempt to resolve the issue. The covering law theory, though defensible against the latter two propositions, is shown to embody unnecessarily and unjustifiably the thesis of value-neutrality. Here it can be successfully revised.

Subjective judgment, as espoused by 'empathy' theorists, thus proves important but mislocated. Its significance lies not in the explanatory force of arguments but in the rational acceptability of the relevant hypotheses. Taking our cue from the substantial insight of Verstehen theorists (Lavine, Dray and Scriven) on the historian's need to make value judgments, we argue, against Hempel, for the essential role of such judgments in any philosophic analysis of rationally acceptable explanation. We take this insight to be an additional pragmatic condition to the covering law theory, rather than a fatal weakness. Hence the denial of value-neutrality does not support the thesis of subjective understanding.

The case against value-neutrality, accordingly, seems best argued on the basis of recent work in statistical analysis of rational decision making in the face of uncertainty. In particular, from the fact that the scientist accepts or rejects corrigible hypotheses, and thus decides when the evidence warrants his acceptance, it follows that he cannot escape making value judgments. This argument we unpack and defend against covering law theorists. But in such a way as to avoid both a behavioralist reduction of belief to action and a pragmatic reduction of truth to utility. Sufficient evidence is shown to be a function of such pragmatic factors

as the cost associated with the importance of making a mistake when acting on beliefs. The goal of science and history thus appears not merely as truth for its own sake, but as truth modified by other criteria, *viz.* epistemic and pragmatic utilities.

The import of this argument forces a reconsideration of the meaning of scientific and historical objectivity and of the relationships between theoretical, technological and policy making aspects of rational inquiry. The humanistic orientation of history, stressed by varied 'empathy' theorists, can be preserved. Yet not at the price of abandoning history as a branch of the science of society. This, then, constitutes the main thrust of our thesis that the covering law theory survives the varied logical criticisms of 'empathy' theorists, but only on condition that pragmatic and purposive elements be included essentially in a logical reconstruction of explanations, and hence that the value-neutrality thesis be surrendered.

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CONTENTS

CHAPTER I

INTRODUCTION

The Recent Controversy

In recent years philosophy of history has again become a live and controversial subject among empirically oriented philosophers in England and this country. But it has become so only as a critical rather than a speculative discipline.[1] After long neglect, in fact generally since Descartes' expulsion of history from the domain of knowledge proper in Part I of the *Discourse*, during which time the subject became a veritable obsession with speculative continental philosophers, attention has again been directed to questions of explanation, prediction, interpretation and objectivity in historical inquiries. No doubt the two major exceptions among empirical philosophers to this by-pass of history are Karl Popper (in his series of essays dating from 1936 and now published as *The Poverty of Historicism* and his later *Open Society and Its Enemies*) and Carl Hempel (in his now classic essay "The Function of General Laws in History").

However, these works were not at all sufficient to arouse recent philosophers from their complacent slumbers. This seems due in part to their lucid and potent argument producing general acceptance among empiricists, and in part to a lack of clarity and persuasiveness in the counter-argument of the idealists causing their position to go by default. Only when recent empiricists of

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[1] W. H. Walsh, *Philosophy of History* (N.Y.: Harper and Brothers, 1951). Chapter I.

the analytical variety, attempting to revive some of the idealist doctrines by reconstructing them in linguistic and pragmatic guise, took issue with and more lucidly challenged the position of these essays did the subject emerge once again as the stage of a major philosophic controversy.

This general lack of empirically-oriented philosophic interest in history, strange though it be in itself, becomes even more perplexing in light of the frequent charges by historians that philosophers have imposed their own methodological problems and rigorous scientific ideals of inquiry on the historian's practice, and hence have forced this practice onto some Procrustian bed. It seems, instead, that historians themselves, not philosophers, have fostered these problems. And they have done so largely because of the extensive variability in their interpretations of historical events, actions and processes, a variability often embarassingly difficult to reconcile with claims of historical objectivity. In the phrase of one theoretically-inclined historian, Cushing Strout, "A specter haunts American historians--the concept of causality. After nearly a hundred years of passionate and dispassionate inquiry into 'the causes of the Civil War' the debate is still inconclusive."[2] Moreover, the specter arose in the context of developments which, by erecting a barrier between the historian's practice and his theory, prevented him from even attempting a systematic appraisal of his explanatory concepts and hypotheses. So

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[2] C. Strout, "Causation and the American Civil War," in History and Theory, Vol I (1961), p. 175.

ce has the situation become that many historians take
scrupulously avoiding generalizations and theoretical
tions. Yet in practice, of course, these same historians
to transcend mere chronicling of facts by offering ex-
s and interpretations of what they take to be significant
y suggesting causes, motives and reasons for historical
d actions.

developments largely responsible for this gap between
ice and theory of historians has been instructively at-
by David Potter[3] among others, to the dilemma produced
rlier cult of scientific history and the relativist re-
it. During the "scientific" era of history, under the
of Ranke's anti-propaganda campaign, historians strove
hard, neutral descriptions of fact and disclaimed any
nterpret or explain or find meaning in the facts. Though
med to know the truth of these events, it was a truth
e only by the purity of descriptive inquiry. Any attempt
n or interpret would be to underwrite purity with bloody
judice and personal value committments.

when it became evident that this strategy failed to lessen
bility of historian's conclusions, that the data apparent-
in varied tongues to even more varied investigators, his-
inally discarded this creed and partly regained their in-
l sanity by recognizing the necessity of going beyond the

Potter, "Explicit Data and Implicit Assumptions in Histori-
," in L. Gottschalk (ed.), Generalization in the Writing of
Chicago: University of Chicago Press, 1963), pp. 178-83.

nterpret and explain it, to organize and unify the facts
o understand them. And when this strategy produced only
usion and variability, they embraced, as a last resort,
relativism. Since one must interpret and explain, and
produces unwanted variability, they sacrificed in theory
ivity of their inquiries. The assumption common to both
hat objectivity is an invariant and value-neutral matter,
ought to yield the same conclusions for all competent in-
fostered a neglect of the theoretical aspects of historio-
the nature of explanation, interpretation and objectivity.
view, they could not attain objective truth without con-
heory, and on the other they could not attain objective
if they did invoke theory.[4] In neither case was theory
le investment. Better to be "pragmatic", to rely upon
s" or is "successful" in practice, became the strategy
rking historians. However, the problems connected with
ns as explanation, interpretation, cause, motive and
p interfering with such practical serenity until even-
problem of clarifying these notions is imposed upon the
r of history.

n the philosopher's hands the problem takes a different
s not always recognized by the historian as his problem.
ears to him that the philosophers are forcing his practice
rocrustean bed, that they are legislating criteria for
historical practice, criteria of proper explanations,

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id., p. 179.

of causal imputations and of the objectivity of conclusions. At any rate, this situation serves to introduce some of the problems underlying the major controversies for contemporary philosophers of history: what is the nature of historical explanation? Does it differ fundamentally from scientific explanation? What relation is there between a philosophic model or theory of explanation and actual historical practice? Can historical explanations be objective? Does objectivity presuppose a value-neutral appraisal of the acceptability of conclusions? What, if any, is the role of purpose and efficiency in the process of explanatory inquiry?

Three Theses of Weber

In what follows, I shall consider certain aspects of this controversy by a careful investigation of three interrelated theses advocated at one time or another by Max Weber, perhaps one of the most sophisticated and influential social scientists of our century. I shall argue that all three theses are unacceptable, but that they are so in extremely suggestive and important ways. In particular, they form the basis of most recent controversies about Hempel's "covering law" theory of historical explanation, at least in the sense that all sides to the dispute have accepted one or more of these theses while most reject at least one other. Moreover, though these beliefs are unacceptable, I find Weber's methodological writings in general to touch in an unsystematic but instructive way on most of the conclusions to be advocated herein. In fact, there is little doubt that the major positions held in this controversy are all influenced by Weber's writings. This includes the naturalistic views of Nagel, Popper and Hempel, the non-naturalism of Lavine, Natanson and Schutz, and the analytic position of Gardiner, Dray, Donagan and Scriven.

Before turning to Weber's theses, let me characterize briefly what has become known as the "covering law" (CL) theory of explanation, since it is at the center of the recent controversy, and since Weber's theses can best be viewed in contrast to this "covering law" theory. But since chapter two is devoted to a detailed examination of this theory, the present account will be

to the following sketch:[5]

Vhat is scientifically explained, in addition to laws, er a type of event or action nor a unique concrete phenom- t an aspect, property or description of an event or action.

To explain scientifically and completely some event or s, ideally, to provide an argument, deductive or inductive, ng a description of the phenomenon to be explained as the on, and a statement of the appropriate general laws and nt conditions as premises.

To be adequate or rationally acceptable, a scientific ex- n must contain as essential premises or explanans, general ch are both testable and either true or highly confirmed elevant available evidence.

Scientific explanation and prediction, two of the central f scientific inquiry, are structurally identical. They nly pragmatically, so that all adequate explanations have al predictive force.

, the three fundamental theses of Weber's methodology, which use as a framework for considering the above controversy, ollows:

..) Thesis of Value-Neutrality (VN): The objectivity of rical explanations requires as a necessary precondition that iiner, qua scientist or historian, make no value judgments.

:. Hempel and P. Oppenheim, "The Logic of Scientific Ex- ı," in Feigl and Brodbeck (eds.), Readings in the Philoso- :ience (N.Y.: Appleton-Century-Crofts, 1953), pp. 319-350.

.ar he must remain evaluatively neutral when appraising
ıbility or correctness of his explanations. Hence objectiv-
ıriant in the sense of yielding the same conclusion for
ent inquirers. This thesis follows from the sharp logi-
ion of questions of fact and questions of value, and
ıbsequent distinction between descriptive statements and
proposals. In Weber's terms, "a systematically correct
proof in the social sciences, if it is to achieve its
ıst be acknowledged as correct even by a Chinese";[6] it
"unconditionally valid type of knowledge".[7] This means
st be acceptable to all, independently of variable per-
rests, attitudes or values. "For scientific truth is
what is _valid_ for all who _seek_ the truth."[8]

) Thesis of _Subjective Understanding_ (SU): Although
ıble empirical explanations must be objective in this
manner, adequate historical explanations of purposive
ns cannot be attained by mere subsumption of the action
ring general laws. Instead, the explanatory force of
the connecting link between the action and its causes,
reasons, is provided by some sort of subjective or in-
e understanding. Our aim in the cultural sciences is
tanding of the characteristic uniqueness of the reality

Weber, _The Methodology of the Social Sciences_ (Glencoe,
Free Press, 1949), p. 58.

d., p. 63.

d., p. 84.

we move...and the cultural significance of individual
[9] We are, consequently, "concerned with psychological and
tual phenomena the empathic understanding of which is
a problem of a specifically <u>different type</u> from those
e schemes of the exact natural sciences in general can or
solve."[10] In other words, since the subject matter of
as a cultural science, consists of purposive human actions,
only be adequately explained or understood from the sub-
point of view of the agent. Hence, the mere subsumption
actions under covering laws, from the external point of
the observer, is insufficient to explain completely the
n's subject matter. SU, accordingly, entails the denial
theory.

ii.) Thesis of <u>Ideal-Types</u> (IT): To eliminate "the
stic prejudice that the goal of the social sciences must
eduction of reality to '<u>laws</u>',"[11] and to achieve the
ve understanding required by historical explanations, the
must utilize "a kind of concept-construction which is
and, to a certain extent, indispensable to the cultural
."[12] Weber refers to the product of this peculiarly cul-
thod of concept formation as an ideal-type or utopia.
generic limiting concepts, "purely analytical constructs

<u>Ibid</u>., p. 72.

<u>Ibid</u>., p. 74.

<u>Ibid</u>., p. 101.

<u>Ibid</u>., p. 89.

created by ourselves,"[13] used as a standard to analyse historically unique configurations and to compare or measure the culturally significant components of human action. The statements containing these constructs are to be clearly distinguished from judgments of value ideals as well as from empirical or descriptive hypotheses or laws. Weber, however, is extremely vague about the nature of ideal-types, and hence about the exact way in which they constitute an alternative to the CL theory of explanation as subsumption under laws. But for our purposes we need only consider this thesis as advocating some alternative to the CL theory, an alternative using ideal-types and satisfying the requirements of the SU and VN theses.

In the succeeding chapters, it will be shown that the non-naturalists (Natanson and Schutz) accept some version of all three theses, the naturalists (Nagel, Popper and Hempel) deny SU and IT but defend VN, while the analysts (Dray, Scriven and Donagan) deny VN and IT but defend SU in a revised version. Accordingly, in denying all three theses, I shall attempt to mediate this controversy, to synthesize the important denials of both the naturalists and the analysts. But while my synthesis will include a defense of the CL theory of explanation, I will also extend the theory in such a way as to incorporate within it the significant contributions of many opponents of this theory, and hence in a way that none of these three groups would find especially felicitous. Such is my embarrassment.

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[13] Ibid., p. 96.

The important controversy recently aroused about historical explanations centers, as we have noted, on the CL model of explanation defended by Hempel, Popper, Nagel, Braithwaite and others of a naturalist persuasion, and challenged by such varied non-naturalists as Schutz, Dray, Donagan and Scriven. With the defenders emphasizing the formal and logical aspects of explanation and the challengers stressing the epistemological and pragmatic aspects, the controversy has not always proved fruitful, since often they seem to be arguing past each other. The main issue at hand is whether or not historical explanations are best construed according to the CL model of scientific explanations.

One of the most frequent objections of the challengers, grounded on the VN and SU theses, has been that a different kind of model is required for historical explanations because of the extreme objectivity prevailing in the CL scientific model. Some suggest a wider use of the term 'explanation,' a more generic version common to all types of explanation; others advocate a peculiarly different and characteristically historical kind of explanatory model. But in both cases the epistemological and pragmatic dimension[14] of explanation is emphasized so that, unlike objective scientific cases, the role of the person accepting explanations cannot be ignored. Hence, both groups distinguish sharply between explanation *qua* potential scientifically testable prediction and explanation *qua* intelligibility or understanding; and between the objectivity of

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[14] J. Yolton, *Thinking and Perceiving* (LaSalle, Ill.: Open Court, 1962), pp. 117-150.

testable and confirmable scientific explanations achieved independently of variable personal judgments and the subjectivity of historical explanations grounded in these valuational beliefs and personal judgments of acceptability.

Some, however, have argued for "a scaling of explanations in terms of their objectivity"[15] on the basis of the latter aspect, of the relative independence from any one individual, instead of for a clear-cut dichotomy between an objective science and a subjective history. No doubt, this view reouires emphasizing not the logical structure of explanation but the epistemic criteria of rationally acceptable explanations. Yet Yolton, representing most recent opponents of the CL model, takes this question to concern the "explanatory force" of theories or hypotheses, and hence erroneously, I think, views this force not as a logical matter but as one concerning conceptual schemes, general attitudes and empathetic understanding.[16] But in this case it is not clear whether he is opting for intelligibility or understanding as providing a different kind of explanatory force in historical explanations, or as a precondition of all explanations.

Yolton's objection rests on the belief that testability is appropriate for scientific explanations but not for historical ones. For in history, general attitudes and conceptual schemes, i.e., an element of Verstehen as subjective understanding not explanation, is more relevant even though there are no clear-cut tests or

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[15] Ibid., p. 122.

[16] Ibid., pp. 124-128.

criteria for determining the correctness of the understanding. The question then is whether or not a system of statements must meet the criteria of testability and deducibility in order to be explanatory. Yolton holds that it must meet these requirements only to be a scientific but not an historical explanation. The latter requires only the criteria of intelligibility or understanding which "varies in kind with the difference of needs, of objective," of context, purpose, values and interests. In fact these are the preconditions, he maintains, of all explanation, while deducibility and testability are the ideal controls only of the sciences. But again the former can be arranged in a scaled order of degrees of objectivity and independence of particular needs, values and interest, and degrees of empirical testability.

Now it seems to me that the challengers of the CL model are correct in advocating a widening of the notion of explanation so as to include these epistemological and pragmatic aspects. They have, nevertheless, failed to upset the CL theorists' claim that a logical relation, deductive or inductive, between laws or generalizations and the events to be explained provides the explanatory force in both scientific and historical explanations. Instead, the inclusion of pragmatic or purposive elements weighs heavily against the VN thesis, but not in favor of SU as they think.

The notion of Verstehen as embodying criteria of intelligibility, to be effective against the CL theory, must be directed not to the logical explanatory force of an argument, but to the objectivity and acceptability of the explanans. The important questions such challengers raise, however unclearly, are first whether or not historical explanations, not easily amenable to empirical confirmation, can be appraised as acceptable on inductive criteria or indices other than testability or confirmation. And secondly if so, whether such criteria support the denial of the VN thesis. I think an affirmative answer is due each question. But the challengers of the CL model either have not seen this or have unsuccessfully defended it, and either for two basic reasons.

First, they have assumed that if there were other criteria than testability, they must be incompatible with, and hence replace, the criterion of deducibility which requires the event to be explained to be deduced from the explanatory premises. This led them to lodge their attack against the logical aspects of the CL model, and hence to support SU as a thesis about the explanatory force of arguments, instead of against VN as a thesis about the acceptability of explanatory hypotheses. Some, in fact, have confused the two theses by conflating them, by suggesting that normative generalizations provide the explanatory force or connection between antecedent conditions and explanandum. Secondly, they have emphasized, along with CL theorists, the deductive model almost to the total neglect of the probabilistic model. As a result, the usual arguments of those who do oppose VN have been misdirected and inadequate, and the arguments of those who defend the VN thesis have been swayed by the unfortunate notion of objectivity associated with the deductive model.

Consequently, I want to argue, against Weber's three theses, that explanations in all the various empirical disciplines have the same explanatory force, which is adequately explicated by the CL theory as an ideal type or idealization. But, additionally, the objectivity of explanations in all disciplines, insofar as they take the form of inductive arguments, seems to require additional inductive criteria of acceptability beyond confirmation and testability. Moreover, one such criterion entials the denial of VN by requiring the making of value judgments about the costs associated with the possible mistakes of accepting or rejecting explanatory generalizations. Finally, these considerations suggest that the various disciplines can perhaps be distinguished according to how much weight must be placed upon this latter criterion, even though it is necessary in some degree in all disciplines, since some can more easily establish policies that are invariant regarding various goals and hence are relatively more value-free than others.

Plan of the Present Work

My general plan then will be, first of all, to examine in some detail the CL theory of explanation as formulated by Hempel and Popper. This, the task of chapter two, will include a discussion of both the deductive and probabilistic nomological models of explanation, the conditions of adequacy for sound explanations, the status of the models as complete idealizations, and various senses of incompleteness or approximations to these two ideal models. Then, since the SU thesis entails the denial of the legitimate extension of the CL models to historical explanations, I will examine various formulations or reconstructions of this thesis. In chapter three both the idealist formulation of SU and the standard reply of such CL theorists as Abel, Hempel and Nagel to this intuitive version of *Verstehen* will be considered.

This early exchange leads to more recent reconstructions of the SU thesis, to alternative defenses of peculiarly ideographic historical explanations in contrast to the nomothetic explanations found in the sciences. In particular, the non-naturalist position of Natanson and Schutz will be considered as a rebuttal to the standard CL or naturalist answer to all empathy theorists. Schutz' reconstruction of *Verstehen* and the SU thesis raises the question of the status of the CL theory of explanation, of how it relates to explanations actually offered by historians. Hempel's answer to this question is that such philosophical theories are explications or, extending Weber's notion of ideal-types, idealized models to be appraised in part on grounds of their usefulness in attaining certain purposes or goals. This answer will receive special emphasis since it fits one of my main contentions: that the major attempts to rehabilitate the SU thesis by some form of empathy turn on the inclusion of such related pragmatic, personal and purposive factors as decisions, interests and attitudes. However, we will argue that the necessary inclusion of these pragmatic elements fails to support the SU thesis. Instead, as noted above, they support a denial of the VN thesis, a point not sufficiently appreciated by the non-naturalists.

But analytically-oriented philosophers (especially Dray, Scriven and Gardiner), on the other hand, do clearly recognize the bearing of these pragmatic factors on the VN thesis. We will, consequently, turn in chapter four to a detailed critical examination of Dray's "rational model" of explantions, viewed as an analytic reconstruction of the SU thesis. For Dray uses his model for the two-fold purpose of defending the SU thesis and of denying the VN thesis. He does so by substituting normative principles of action for Hempel's descriptive empirical covering laws as the source of explanatory force. As already indicated, however, we shall contend that Dray's criticism of the CL theory fares no better than the earlier critiques of the idealists and the non-naturalists. They all fall short of their mark. To this extent will we attempt to defend the CL theory of explanation: to the extent of supporting it against the SU and IT theses.

In the last two chapters, we will examine the extension of the CL theory to cover historical explanations of purposive human actions.

Chapter five is devoted to Hempel's broadly dispositional analysis of human actions, while chapter six raises some problems for the probabilistic model of the theory. In chapter five we will consider Hempel's alternative CL theory of reason-explanations by contrasting it with Gilbert Ryle's version of dispositional predicates, since the latter serves as the basis for more moderate criticism of the CL theory. The discussion at this juncture centers on the criticisms of Donagan, R. B. Brandt and Scriven to the requirement of including general laws as a necessary condition for adequate historical explanations. Here again Hempel's CL theory will be defended against attempts to disunify the empirical sciences, to contrast sharply the ideographic and nomothetic sciences.

Finally, in chapter six, we shall examine some aspects of Hempel's probabilistic model of explanation, since the general laws required to explain historical actions will usually be statistical in nature, and since the inclusion of such laws bears heavily on the VN thesis. In particular, we shall argue that while Dray's argument against the VN thesis is unconvincing, his conclusion, the denial of VN, can be adequately supported on other grounds. These grounds relate closely to Hempel's probabilistic model and to the criteria of acceptability for statistical hypotheses. To this extent will we defend some of the varied opponents of the CL theory. The argument will try to show that the insistence of empathy theorists on purportedly non-experimental factors, which force the historian to consider pragmatic and evaluative aspects of inquiry, is cogent. And in a way which meets the standard or official CL answer by placing the issue clearly in the context of the logic of justification.

It is noteworthy in this context to notice the constancy of almost all CL theorists in their advocacy of value-neutrality and in their unwillingness to provide the pragmatic dimension of explanation with any important systematic function. While not denying the existence of this dimension nor even its significance, they relegate it to a pre-systematic, non-theoretical or psychological status concerning the discovery rather than the justification or confirmation of explanatory generalizations. They then regard the objectivity of scientific and historical explanatory accounts as independent of pragmatic or purposive considerations, and hence as supporting the VN thesis. Objective justification of explanatory generalizations involves, for them, only the requirements of deducibility, testability and evidential or confirmatory strength. But this position also requires depicting the scientist as essentially a guidance-counselor of decision makers, not himself as a decisionmaker. It requires distinguishing sharply between the theoretical goal of achieving truth and nothing but the truth on the one hand, and the practical goal of deciding to accept or reject hypotheses or theories on the other. Accordingly, our defense of the opponents of the CL theory turns on a criticism of this latter distinction, the notion of objectivity it supports, and on the tenability of widening the notion of explanation to include the pragmatic dimension.

Lest this twofold defense appear paradoxical, however, it must be noted that the apparent paradox results from an assumption shared in common by both sides of the controversy, as well as by most historians: that the CL theory of explanation (and hence a denial of SU) entails VN. If this entailment did hold, my twofold defense of the CL theory and of the denial of VN would indeed be inconsistent. Such, it will be argued, is not the case.

But recognition of the fact that the entailment does not hold, that the denial of VN is compatible with and perhaps even required by the CL theory, has been obscured by the undue emphasis placed on the deductive model of explanation and the use of universal or deterministic laws as necessary ingredients in the explanans. Both sides are, I fear, partly responsible for the neglect of the probabilistic model and the subsequent lack of investigation of statistical generalizations, so important for social and historical explanations. Only when the latter model receives proper attention can it be seen how VN can be successfully denied and, at the same time, why this denial does not entail the affirmation of SU or the denial of the CL theory. The usual attempts to deny VN fail because, as Weber clearly saw, they locate the value element in the context of discovering or imaginatively constructing plausible explanatory hypotheses. Most CL theorists readily concede this point without damaging their VN thesis. For the latter thesis concerns not the discovery but the justification, corroboration or confirmation of explanatory hypotheses.

Further, most defenders of SU, from Weber and the early idealists to Schutz and Dray, fail to locate the value element in the context of justification for much the same reason that they object

to the CL theory in the first place: because they believe this commits one to a crude form of behaviorism or pragmatism or both. Our task, accordingly, will be to show that this belief also lacks foundation. In other words, we will defend the CL theory of explanation by extending it to include a denial of VN, but in such a way as to avoid any committment to a completely behavioral account of the acceptance of beliefs or hypotheses, or to a completely pragmatic version of evidence or the rational acceptability of empirical hypotheses. All that is necessary to oppose VN successfully, I think, is to show that the acceptance and acceptability of explanatory hypotheses entails some behavioral aspect and some pragmatic criterion of appraisal. It is not necessary to show that they are equivalent. That is, a denial of VN neither requires beliefs to be reducible to actions nor truth and confirmation to be replaceable by utility. The cogency of either of these latter theses we leave an open question, though the latter surely seems less so than the former.

Much of the point at issue amounts to the charge made by experimentalists[17] that philosophers as varied as Weber, Dilthey, Schutz, Dray and Hempel fail to supply a broad enough model of scientific or historical inquiry. In particular, they tend to evade the issue which

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17 E. A. Singer, Experience and Reflection (Philadelphia: University of Pennsylvania Press, 1959); C.W. Churchman, Theory of Experimental Inference (N.Y.: Macmillan, 1948), Prediction and Optimal Decision (Englewood Cliffs, N.J.: Prentice-Hall, 1961), "Statistics, Pragmatics, Induction," Philosophy of Science, Vol. XV (July 1948); P. Frank, Philosophy of Science (Englewood Cliffs, N.J.: Prentice-Hall, 1957); R. Braithwaite, Scientific Explanation (N.Y.: Harper Bros., 1953); R. Rudner, "The Scientist Qua Scientist Makes Value Judgments," Philosophy of Science, Vol. XX (1953).

experimentalists take as central: the theory of experimental action. For when such a theory is considered at all, its efficacy seems at best construed at the level of Weber's "subjective selectivity,"[18] instead of at the level of experimentally controllable notions such as teleology, production, function and purpose. These latter notions turn on Weber's neo-Kantianism, particularly in his use of ideal types or limiting processes to relate observational data to theoretical and evaluative ordering structures.

Further, these varied philosophers fail to supply a sufficiently broad model of rational inquiry largely because they tend to isolate questions of fact from questions of evaluation, questions of confirmation or evidential strength of beliefs from questions of purposes or application. Hence, by emphasizing the formal to the neglect of the purposive aspects of explanation, they fail to even consider the necessary conditions for a complete theory of explanatory inference of methodology, a theory for selecting the most reasonable explanations. Such a theory, it would seem, requires not only the semantical criterion of confirmation or agreement with facts and the syntactical criteria of consistency and simplicity or economy, but also the pragmatic criterion of utility and efficient purposive behavior.[19]

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[18] Weber, _op. cit._, p. 82.

[19] Frank, _op. cit._, Chapter 15.

CHAPTER II

THE COVERING LAW THEORY OF EXPLANATION

K. Popper's Early Formulation

Before considering some of the issues surrounding the controversy about the applicability of the covering law theory of explanation to historical inquiry, let me pull together some of the various strands of this theory. It will be helpful, I think, to have a fairly full statement of the CL theory before us in order to see whether or not it can be fruitfully extended to cover historical, as well as scientific, explanations, *i.e.* whether or not it can be defended against the various interpretations of the SU thesis. Most of the formulations of the CL theory occur in the context of natural science explanations, particularly of the causal variety. Consequently, much of the discussion will be limited to those aspects of the theory which bear most directly on the case of historical explanations of purposive human actions. This means that certain important aspects of the theory will receive more detailed treatment than others. In particular, we must forego any but the briefest account of the relationship between explanation and prediction, *i.e.* of Hempel's structural symmetry thesis. Additionally, we can but mention the difficult ontological problems about what it is that can be explained by the CL theory, except insofar as the question relates to the central notion of a complete explanation which will be discussed in some detail.

The plan of this chapter, accordingly, is to outline briefly Popper's early formulations of the CL theory and then to look more closely at Hempel's recent systematic treatment. The theory will be shown to include two formal models of scientific explanation, and various criteria or requirements for determining what is to count as both "an explanation" and "an acceptable or sound explanation." Different senses of ideal completeness along with various degrees of approximation or incompleteness will also be considered. Finally, in this connection we will raise the question as to the nature of this particular enterprise, i.e. of offering a theory or explication of the notion of explanation.

Let me begin with Karl Popper's formulation of the CL theory, since he claims to be its author, having put it forth as a general theory of explanation as early as 1935 in Logik der Forschung, more recently translated as The Logic of Scientific Discovery; and again in the two works cited above with special reference to history. The central thesis of the theory, however, has historical roots in the comparable views of Weber, Campbell, Mill, Galileo and even Aristotle. In brief, Popper follows Weber in characterizing the explanation of natural phenomena as the subsumption of the many under the unity of the one, in the sense of subsuming what is to be explained under general laws. Hence explanation, unlike description, takes the form of an inference or argument containing general laws as essential premises. To offer an explanation of some phenomenon is to offer an argument, not merely descriptive information. For it is the logical or inferential connection between the general laws and the phenomenon to be explained which provides the explanatory relevance of the former to the latter and assures the explanatory force of the covering laws. Accordingly, Popper writes: "To give a causal explanation of a certain event means to derive deductively a statement (it will be called a prognosis) which describes that event, using as premises of the deduction some universal laws together with certain singular or specific sentences which we may call initial conditions."[1]

He then illustrates this pattern by reconstructing a causal explanation of the breaking of a given piece of thread found capable of carrying one pound only, but with a two pound weight put on it. The appropriate explanation will contain both kinds of constituent statements just mentioned, viz. two laws and two initial conditions. The two universal laws are:

(L_1) "For every thread of a given structure S (determined by its material, thickness, etc.) there is a characteristic weight W, such that the thread will break if any weight exceeding W is suspended from it," and

(L_2) "For every thread of the structure S, the characteristic weight W, equals 1 lb."

The two initial conditions then are:

(C_1) "This is a thread of structure S.", and

(C_2) "The weight to be put on this thread is equal to two pounds."[2]

From these four statements, both kinds of which are necessary ingredients of a complete causal explanation, we can thus deduce the

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1 K. Popper, The Logic of Scientific Discovery (N.Y.: Basic Books, 1959), p. 59.

2 Ibid., p. 60, new footnote *1.

prognosis, conclusion or description of the event to be explained: (E) "this thread will break." The situations described by the initial conditions (C_1) and (C_2) are then spoken of as the cause of the event in question, and the event described in the prognosis (E) as the effect.

But while differing from description in this inferential way, explanation is also similar to description in at least one important sense for both Popper and Weber. All scientific explanations and descriptions of facts are highly selective; they are always theory — dependent and never occur in isolation. The reason for the impossibility of avoiding selectivity is, of course, the "infinite wealth and variety of the possible aspects of the facts of our world,"[3] and the finite limitations of descriptions. Thus our descriptions and explanations will always remain incomplete, a mere selection according to our interests of the facts available for description. The point is a result of what Popper calls his "searchlight theory of science," since description depends on our point of view, theories and interests; much as what a searchlight makes visible depends upon its position, our way of directing it and its intensity. There can be then no such thing as an actually complete description, no less a complete explanation, of any individual event or fact in the world. Both require abstracting from and selectivity of the infinite subject matter.

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[3] K. Popper, The Open Society and Its Enemies (N.Y.: Harper and Row, 1952), vol. II, p. 261.

In his later work Popper also derives three important consequences from this deductive model of causal explanation. Events, first of all, are causes or effects only relative to some universal laws covering them, not absolutely. There is little doubt of Popper's allegiance to a Humean view of causality which involves the denial of necessary connections between events, and instead emphasizes the connection in terms of empirical regularities. Yet his theory "differs from Hume (1) in that it explicitly formulates the universal hypothesis that events of kind A are always and everywhere followed by events of kind B; (2) that it asserts the truth of the statement that A is the cause of B, provided that the universal hypothesis is true."[4] In other words, in addition to Hume's events A and B, Popper establishes a third element, a universal law, with respect to which we can speak of a causal link, or even a "necessary connection." However, Popper readily admits, in a passage influencing some recent critics, that "these universal laws are very often so trivial (as in our own example) that as a rule we take them for granted, instead of making use of them."[5]

Secondly, he formulates loosely what has recently been labeled the "structural symmetry or identity thesis" concerning explanation, prediction, and confirmation or testing. "There is no great difference between explanation, prediction and testing. The difference is not one of logical structure, but one of emphasis; it depends on our interests *what we consider to be our problem* and what we

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[4] *Ibid.*, p. 363.

[5] *Ibid.*, p. 262.

do not so consider."[6] Further, this pragmatic emphasis serves to distinguish three kinds of sciences, parallel to the three kinds of scientific interests, purposes or problems we may have. The "theoretical or generalizing sciences" (e.g. physics, biology, sociology) use the pattern to test and establish universal laws or hypotheses considered as problematic. The "applied generalizing sciences" (e.g. engineering) take the premises as given and use them as means for predicting the prognosis and hence deriving some new information. And the "historical sciences," by contrast, take the prognosis as the given explanandum and attempt to uncover the premises, initial conditions and laws, from which to deduce and hence explain the given particular event, instead of testing or predicting. Accordingly, Popper accounts for the oft-repeated view that historians are interested in explaining particular events, not in formulating or establishing universal laws. The laws are formulated by the generalizing sciences (e.g. sociology) and 'assumed' by the historian. However, he is careful to block the conclusion which many have inferred from this point, viz. that historical explanations need not utilize general laws.

Finally, Popper's deductive model and the derived division of the sciences serves to eludicate his view concerning the role of theories, interpretations or points of view in history. Unlike the generalizing sciences, in history we have no "unifying theories;

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[6] K. Popper, The Poverty of Historicism (London: Routledge and Kegan Paul, 1957), p. 133.

or, rather, the host of trivial universal laws we use are taken for granted; they are practically without interest, and totally unable to bring order into the subject matter."[7] Some of these laws are indeed trivial, as Popper's case of explaining the defeat of Poland's first division in 1776 by appealing to the following law clearly indicates: "If of two armies which are about equally well armed and led, one has a tremendous superiority in men, then the other never wins." Yet Popper also endorses the historian's practice of appealing to selective principles, points of view or interpretations which are merely "quasi theories," often preconceived notions as the great-man thesis or the causal priority of economic conditions, geographic conditions or moral ideas. Though such "historical theories" contrast sharply with scientific theories in so far as they are untestable (unfalsifiable) by facts independent of the preconceived theory itself, and hence as non-scientific though still cognitively and empirically significant, they are nevertheless given an important role and status as "inevitable" in historical inquiry. They serve as foci, "centers of interest" or working hypotheses for collecting additional facts and records, as well as being of topical interest by elucidating the problems of the day.

We will want later to inquire whether such "theories" constitute constituents of proper or merely pseudo explanations when combined with appropriate antecedent conditions. We might note

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[7] Popper, The Open Society and Its Enemies, p. 264.

here, however, that not all such interpretations are of equal merit for Popper. In fact, he accepts some kind of continuum ranging from high-level interpretations to singular hypotheses serving as initial conditions, with "all kinds of intermediate stages."[8] This would seem to mean either that there are other criteria for appraising general interpretations than evidential strength or testability, or that the criterion of testability must itself be weakened to one of degrees. In the latter case, interpretations would be taken as merely less testable or falsifiable than scientific theories or singular statements, instead of as untestable in principle.

Let me reserve comment on the problems surrounding the notion of testability and empirical significance until a comparative analysis with Hempel's notion of incomplete explanation can be made. So far, then, we have seen that explanation in Popper's view requires a selective process, consists in the deductive subsumption of particular events under general laws or hypotheses, differs from prediction and testing only pragmatically but not structurally, and can often be accomplished in historical inquiry by substituting general interpretations or "theories" for scientific laws on the basis of selection and ordering.

Before moving to Hempel's more detailed theory of explanation, two comments pertaining to Popper's later writings seem noteworthy for our purposes. In a new essay, "Facts, Standards and Truth,"

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[8] Ibid, p. 266.

published in 1961 as an addendum to The Open Society and Its Enemies, Popper makes explicit two important and related points which were at best implicit in his earlier work. The first concerns indirectly Weber's IT thesis. For while Popper quite explicitly denies the thesis in Weber's sense, i.e. in any way which would conflict with the CL theory, he nevertheless uses the notion of an ideal-type, limiting standard or regulative principle in characterizing the notion of the truth of explanatory hypotheses. As the essential element of a general attack on relativism or skepticism-- the view that the choice between competing explanatory theories is arbitrary-- Popper clearly distinguishes between "knowing what truth means, or under what conditions a statement is called true" and "possessing a means of deciding-- a criterion for deciding-- whether a given statement is true or false."[9] Following Kant and C.S. Peirce, he construes the idea of truth as a regulative ideal which can be approximated but not known to be achieved.[10] Hence, though there is no general criterion of truth, there are criteria of progress toward truth. We can know when our theories are approximating to the ideal standard or meaning of truth, and when not.

Now for our purposes his theory of truth is not so important as is his use of ideal regulating principles. For in the last section of this chapter, I will suggest that the basic tenets of the CL theory are best considered in just this manner. This is, I take

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[9] Ibid., p. 371.

[10] Ibid., p. 376.

the CL theory to be a philosophic reconstruction or explication of some important and useful meanings of the term 'scientific explanation'. And the status of such a theory or explication I take to be that of a set of regulating principles, an ideal-type or standard for appraising and clarifying our ordinary explanations as approximations. In this way Weber's IT thesis, though incorrectly opposed to the CL theory, serves to illumine one important aspect of the issue at hand.

Still, it is not entirely clear whether Popper would countenance such an extension of his theory of truth to apply to his theory of explanation. Though in general this extension might not be objectionable to him, the specific analysis of the CL theory which I will propose undoubtedly would. And this brings us to our second comment, which pertains to Weber's VN thesis.

Unlike many supporters of the notion of value-neutrality in the acceptance of explanatory hypotheses, expecially those who appeal to a clean distinction between facts and decisions to defend the thesis, Popper has recently acknowledged the essential decisional aspects of accepting hypotheses as well as of proposing normative or value judgments. In other words, the VN thesis is often defended on the Weberian grounds that the truth of value judgments depends on human decisions to adopt certain standards, while the truth of factual assertions or explanatory hypotheses does not. But Popper, in following out the consequences of his fallibilism thesis whereby no hypothesis is immune or exempt from error and criticism, finally rejects this position and concedes that we will have to decide when the evidence for such hypotheses warrants our accepting them. Hence, "in this sense, decisions enter into the critical method," i.e. in the sense of justifying "the tentative acceptance" of some theories as preferable to others.[11]

Nevertheless, Popper maintains his support of the VN thesis by shifting the grounds for it to a dualism of facts and policies or standards, instead of his earlier dualism of facts and decisions. We will, accordingly, pursue this kind of defense in some detail in Chapter VI. It might suffice for the moment to suggest a point of clarification concerning the VN thesis. The issue concerns whether or not an adequate explication of the notion of acceptable explanation would require a scientific or historical inquirer to make value judgments. It is not a question of value judgments being identical with or reducible to factual judgments; nor is it a question of the subjective or objective character of value judgments. For both of these questions, however important, are independent of the main issue. Hence, in addition to investigating Hempel's version of the CL theory, we will also attempt to see what bearing these two points concerning ideal-types and value-neutrality have had on it.

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[11] Ibid., p. 380.

C. Hempel's Deductive Model

Overlapping the work of Popper for the past twenty years, Hempel's essays on explanation develop Popper's theory in a forceful, lucid and influential manner. Perhaps because of these very reasons, they have been subjected in recent years to serious critical investigation which in turn has spurred Hempel to elaborate and also to modify his earlier position in important ways. To what extent such elaboration of detail raises additional difficulties, and whether the modifications amount to a retraction of the original deductive model are some of the questions to be treated below. Our main consideration will eventually rest with the question of how far the theory can be reasonably extended to include such non-natural science inquiries as history.

Hempel's original essay on the theory of explanation, "The Function of General Laws in History,"[12] generalizes the deductive model beyond Popper's strictly causal form, but remains essentially similar on most counts, while filling in the theory with a more detailed analysis of central aspects. Perhaps the prime motivation of both Hempel and Popper, clearly expressed in this essay, was and remains the rebellion against the earlier idealist tradition, arising in Germany and spreading to England and the Continent, which argued for the SU thesis and for a radical difference in kind between the explanatory methods employed by historians and those utilized in the sciences. This purported demarcation of sharp boundaries between

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12 C. Hempel, "The Function of General Laws in History," in P. Gardiner (ed.), Theories of History (Glencoe: Free Press, 1962), pp. 344-356.

the different fields of scientific inquiry, and the consequent autonomous development of each field, was enshrined in the contrasts between ideographic and nomothetic disciplines, unique and repeatable events, between "Geisteswissenschaft" and "Naturwissenschaft," and between "Verstehen" and "erklaren" or "begreifen." In opposing these basic contrasts, Popper and Hempel concur in advocating the methodological unity of all the empirical sciences. The influence of Comte, Mill and Buckle, as well as Hume, is clear. Their approach is to reform the social and humanistic domains by making them more scientific and subject to empirical controls.

In our present case this reforming attitude manifests itself in their insistence upon assimilating historical explanations to scientific ones, in particular to the deductive or covering-law pattern as a prototype or model. To be sure, such an assimilation flies in the teeth of the multiple and varied arguments used by idealists and their recent analytic defenders: arguments from the uniqueness and complexity of data, from the presence of value bias and the need for empathy, from the existence of free will and self-fulfillment, from teleological causation, from the inaccessibility and non-physicality of the mind, and from the requirements of morality. Accordingly, we shall in later chapters look carefully at some of these arguments since they are persuasive enough to be revived by such critics of the covering-law model as Lavine, Schutz, Dray and Scriven. However, a good deal still remains to be said about Hempel's construal of the CL theory.

In addition to this concurrence with the thesis of methodological unity, Hempel, in his early essay, indicates further doctrines shared with Popper. Scientific explanation, for example, is said to be formulable in terms of a deductive argument containing general laws which are understood to be "statements of universal conditional form...capable of being confirmed or disconfirmed by suitable empirical findings...and assumed to assert a regularity"[13] between the initial conditions and the explanandum. Hempel and Oppenheim insist, with Popper, upon the strong logical relationship of entailment between explanans and explanandum, on the necessity of universal general laws as part of the premises, and on the empirical content or testability of these laws— all as necessary though not sufficient requirements for an explanation to be scientifically sound, and hence distinguished from both unacceptable and pseudo explanations.

In a later essay, "Studies in the Logic of Explanation," these are codified into the following four logical and epistemic conditions of adequacy for the soundness of any proposed or potential explanation:

(R_1) The explanandum must be alogical consequence of the explanans.

(R_2) The explanans must contain general laws, and these must actually be required for the derivation of the explanandum. (But unlike his earlier essay and also Popper's

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[13] Ibid., p. 345.

account, Hempel no longer requires nonlawlike initial conditions. This allows explanation of laws or generalities as well as of particular events.)

(R_3) The explanans must have empirical content, i.e. it must be capable, at least in principle, of test by experiment or observation.

(R_4) The explanans must be true.[14]

And the schema for a sound scientific deductive explanation is then presented as

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\begin{array}{lll}
\text{(D)} & (1) & L_1 \cdot L_2 \cdots\cdot L_k \\
 & (2) & C_1 \cdot C_2 \cdots\cdot C_m \\
\hline
 & (3) & E
\end{array}
$$

where '$L_1 \cdot L_2 \cdots\cdot L_k$' represent universal laws, '$C_1 \cdot C_2 \cdots C_m$' represent statements of initial or boundary conditions, 'E' the statement of the explanandum event, and (1) and (2) together as the explanans logically entail (3).

Thus, Popper's example concerning the thread's breaking can readily be seen to fit pattern (D) since (L_1), (L_2), (C_1) and (C_2) serve as explanans and have (E) as a logical consequence. Another example, cited by Hempel, explains why the part of an oar which is under water appears, to an observer, to be bent upwards.

> The phenomenon is explained by means of general laws-- mainly the law of refraction and the law that water is an optically denser medium than air-- and by reference

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[14] C. Hempel and P. Oppenheim, "Studies in the Logic of Explanation," p. 321.

> to certain antecedent conditions-- especially the facts that part of the oar is in the water, part in the air, and that the oar is practically a straight piece of wood. Thus, here again, the question 'Why does the phenomenon happen?' is construed as meaning 'according to what general laws, and by virtue of what antecedent conditions does the phenomenon occur?'[15]

Further points of similarity with Popper are the structural symmetry or identity of explanation and prediction, i.e. that there is only a pragmatic difference of direction, interest or purpose between the two; the centrality of explanation and prediction as primary goals for scientific inquiry; and the ontological thesis that what is explained is not merely a type or kind of event nor a concrete event but rather an aspect, property or description of an event, i.e. an event of a certain kind. Concerning the latter thesis, both Hempel and Popper are anxious to follow Weber by denying the possibility of even a complete description, not to mention a complete explanation, of a concrete individual event (such as the assassination of Huey Long) since this "would require a statement of all the properties exhibited by the spatial region or the individual object involved, for the period of time occupied by the event in question." Their intent of course is to undercut the idealist notion that the peculair function of history is to "grasp the unique individuality" of its subject matter by arguing that history can do this "no more and no less than can physics or chemistry."[16] We will return to this ontological thesis in connection

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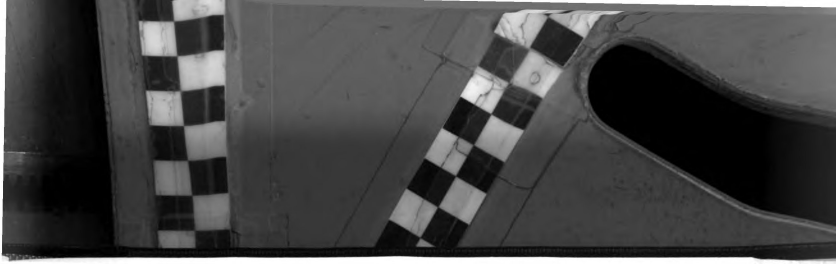
[15] Ibid., p. 320

[16] C. Hempel, "The Function of General Laws in History," p. 346.



with the distinction between complete and incomplete explanations later in this chapter. Meanwhile, pursuing the relationship between explanation and prediction will perhaps help to clarify some of the point of explanatory arguments.

Following Popper, Hempel maintains that

> the same formal analysis, including the four necessary conditions, applies to scientific prediction as well as to explanation. The difference between the two is of a pragmatic character. If E is given, i.e. if we know that the phenomenon described by E has occurred, and a suitable set of statements C_1, C_2, ..., C_k, L_1, L_2 .... L_r is produced afterwards, we speak of an explanation of the phenomenon in question. If the latter statements are given and E is derived prior to the occurrence of the phenomenon it describes, we speak of a prediction. 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."[17]

It will be noticed, however, that in this passage two theses are really being propounded, a weaker and a stronger one. The stronger and hence more controversial thesis maintains a structural symmetry or identity between scientific explanation and prediction, while the weaker thesis merely holds that all adequate scientific explanations must have potential predictive force. We will refer to them as the "Symmetry Thesis" and the "Predictive Thesis" respectively. Both theses, however, concern only explanatory and predictive arguments, not statements that merely describe some past, present or future event. They refer to the logical derivation or inferability of the explanandum from the explanans not to the mere assertability of the explanandum. This distinction is of

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[17] C. Hempel and P. Oppenheim, "Studies in the Logic of Explanation," p. 323.

some importance since many attacks on the symmetry thesis seem to have misfired due to a failure to preserve Hempel's distinction. Many philosophers have pointed to clear cases of asymmetry between the assertability of scientifically predictive and explanatory statements, but such cases are irrelevant to a thesis about logical relations and inferability.[18]

Rather than become embroiled in the many controversies concerning this thesis, a task well beyond the scope of the present chapter, it might instead be more instructive for our immediate purposes to consider the weaker predictive thesis. Some of the arguments recently used to oppose this thesis raise questions bearing heavily on our later inquiry about historical explanations. For example, if scientific explanations are taken inferentially as arguments instead of as descriptions, what then are they intended to show? What is the point of such explanatory arguments? How do they differ from other kinds of arguments? Or is it the case that all arguments are explanatory? To come by an adequate answer to these questions will require, of course, a fuller analysis of requirements (R_1)-(R_4). But a beginning can be made by examining some of the arguments against the potentially-predictive thesis recently proposed by I. Scheffler and J. Kim.

In the course of defending the scientific legitimacy of *ex*

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[18] Cf. the instructive defense of this thesis by A. Grunbaum in "Temporally-Asymmetric Principles, Parity Between Explanation and Prediction, and Mechanism Versus Teleology," *Philosophy of Science*, Vol. XXIX (April, 1962).

post facto explanations, Kim[19] follows Scheffler's[20] lead in opposing Hempel's potentially-predictive thesis by distinguishing sharply between scientific explanation and prediction. In this way he attempts to vindicate ex post facto explanations as legitimate scientific explanations, even though they lack any significant predictive power. While it is conceded that scientific explanations do indeed take the form of an argument or inference, it is nonetheless maintained that they do not purport to establish, support or prove their conclusions or explanandum-statements. They do not, in other words, "purport to show that the event to be explained actually took place or is taking place."[21] But predictive arguments, on the other hand, are intended to show just this. As attempts to gain knowledge of particular events or states by projection from known to unknown data, they are intended to substantiate or support their conclusions. Hence their premises do function as evidence for the predictive conclusion which in turn is dependent upon the premises for evidential support.

Now, since Popper and Hempel concur with this view of prediction

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[19] "Ex post facto" explanations refer to those cases where the antecedent conditions have to be ascertained after the request for an explanations is made, and where our knowledge of the actual occurrence of the explanandum-event plays an essential evidential role in ascertaining these conditions.

[20] I. Scheffler, "Explanation, Prediction and Abstraction," British Journal for the Philosophy of Science, 7 (1957). This entire paper is included in revised and enlarged form in Scheffler's most recent work, Anatomy of Inquiry (N.Y.: Knopf, 1963), Part I.

[21] J. Kim, "Inference, Explanation and Prediction," Journal of Philosophy, LXI, No. 12, June, 1964, p. 362.

but still take scientific explanation to be potentially predictive, the issue clearly turns on an alternative version of explanation. Scheffler and Kim point out, correctly I think, that explanatory and predictive arguments are not merely abstract kinds but are instead concrete arguments given at adefinite time, in a specific context, for a specific purpose. This is to say that they are "not argument-types or inference-types, but specific argument-tokens and inference-tokens." But from this pragmatic, context-bound view of arguments, they argue that explanations, unlike predictions, are only attempts to systematize known events and states; not, as Hempel and Popper suggest, attempts to establish, support or prove a conclusion, nor to show that the explanandum-event actually took place. Explanations are intended "merely to exhibit logical relations obtaining between statements,"[22] in order to show the connection, mediated by laws, between the events described in the antecedent-conditions and the conclusion.

But this conclusion is based on arguments that are at best misleading when taken as objections to the Hempelian theory of explanation. Hempel has, in many essays, insisted that scientific explanations must establish or support the conclusion of an inference, even if this is not their main task. In his most recent statement, he submits a general and necessary condition of adequacy for all rationally acceptable scientific explanations of a given event, viz. that "any such explanation... of the type 'why did X occur?' must provide information which constitutes good

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22 Ibid., p. 362.

grounds for the belief that X did in fact occur."[23] The predictive thesis then follows as a consequence of this condition of adequacy.

Kim offers two arguments to deny this condition of adequacy. The first is that often the truth of the explanandum statement is actually known with greater certainty than that of the explanans statements. The second appeals to the fact that when we ask "why did X occur?" we presuppose or presume that X did occur. Hence, he claims, in providing an explanation we neither intend nor need to give proof or support or good grounds for our belief that X did occur.

Now, I think we can concede both of these points as accurate descriptions of some ordinary and scientific practice of providing explanatory arguments. Yet neither point entails the denial of Hempel's condition of adequacy. And the underlying reason why they do not is because they concern the psychological-pragmatic aspects of explanation, while Hempel's theory is an explication or reconstruction of the logic of explanation. This is not to suggest that the pragmatic elements of explanation are unimportant or fruitless. To the contrary, we will argue in later chapters that such elements are central, even for Hempel's reconstruction. However, the point here is that recent analytic critics of Hempel's CL model have not always met the issue clearly. They have argued for the pragmatic

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[23] C. Hempel, "Reasons and Covering Laws in Historical Explanation," in S. Hook (ed.), Philosophy and History (N.Y.: New York University Press, 1963), p. 146.

aspect of explanation by describing ordinary and scientific explanatory practices. But since Hempel is not likewise offering an alternative description, but instead a methodological prescription or explication, of these practices, there is no essential conflict between them and hence no refutation of Hempel's condition of adequacy. Surely there is no conflict in explanations serving both purposes: systematizing known events and states; and also establishing, supporting or proving their conclusions.

Further, while it is correct to say that actual explanatory arguments are always concrete or argument-tokens, it does not follow that one cannot profitably and accurately abstract from these concrete cases some important logical structures and conditions or rational acceptability or adequacy as ideal types or idealizations. In fact, if one could not, it is doubtful how, or even whether, we could elicit any reasonable criteria upon which to appraise critically such arguments as acceptable or not, as genuinely scientific or pseudo-scientific. In this sense the CL model of explanation, following Weber, is instructively compared to the concept of mathematical proof as construed in meta-mathematics. Surely all actual proofs are also concrete or proof-tokens. Yet this fact does not preclude the significant construction of a theory of proof as a theoretical account abstracted from the concrete cases where someone proves something to some other person at a definite time, in a specific context, for a certain purpose.

Hence, it seems that the two reasons offered by Kim, representative of many recent arguments concerning historical explanations to be considered later, are though true at best misleading when employed as objections to the CL model. Moreover, the question is not merely whether pragmatic elements enter into the analysis of scientific explanation. Indeed they do. But CL theorists have never denied that they do and their reconstructed model does not require that they deny it, so long as they are limited to the descriptive level. For in this case Hempel can reply that such objections miss their mark since they apply to his non-pragmatic or theoretical concept of explanation standards that are only proper for a pragmatic construal. The question of importance, instead, is whether or not the CL model itself, as an explication or reconstruction of ordinary scientific explanations, requires the inclusion of pragmatic elements. Since Hempel and Popper claim that it does not, a more reasonable objection to their theory would seem to be one showing that such elements are required for this task. Accordingly, the last chapter will be devoted to just this topic.

So far, then, the CL theory emerges as an analysis of scientific explanation which insists upon their status as deductive arguments, i.e. as satisfying (R_1). In addition, the explanans of such arguments serve to support evidentially, as well as to organize and systematize, their explanandum-events, and hence have potential predictive power.

Pseudo, Genuine and Acceptable Explanations

Yet suppose the latter point to be conceded, viz. that the explanans of a genuine and of an acceptable scientific explanation must be capable of evidentially supporting the explanandum. Still, obviously not all deductively valid arguments are explanatory. Hence we need examine more closely Hempel's other conditions of adequacy, (R_2)-(R_4), in order to determine what distinguishes explanatory arguments from others. More specifically, we will want to see how Hempel distinguishes between genuine and pseudo scientific explanations, as well as between those genuine cases which are rationally acceptable and those which are not.

To make a start in this direction, we might ask for a defense of (R_2). Even conceding (R_1), why and in what sense must the explanans contain essentially-occurring general laws in order to provide adequate support for the explanandum? For surely any singular statement can be deduced from some set of premises, none of which are of the form of universal laws, i.e. of the form 'All A is B' or '(x) (Ax Bx).' Any defense of (R_2), accordingly, must be independent of the reasons for maintaining (R_1) or deducibility. Moreover, a defense of (R_2) will become important for our purposes in later chapters. For many critics of the CL theory, and proponents of altermative theories of historical explanation, rest their case on the inadequacy of (R_2) and on the subsequent claim that scientific and historical explanations can be genuine, complete and rationally acceptable without containing general laws. The question at issue then is why Hempel takes (R_2) to be a necessary condition of the adequacy of genuine and acceptable explanatory arguments.

Hempel, unfortunately, has not explicitly formulated a defense of (R_2). Instead, his writings reveal brief relevant comments to this issue, followed by mostly futile attempts to answer quite a different question: what is meant by a law or a lawlike hypothesis? But however important and difficult this question may be, an answer to it will surely not serve to defend (R_2). We will do well, consequently, to start from Popper's allegiance to Hume, mentioned earlier, an allegiance also shared by Hempel. Our earlier comments, when conjoined with Hempel's brief defense, suggest two primary arguments to support (R_2) as a necessary condition of adequacy.

The first is an argument from the meaning of 'explanation' and such closely associated terms as 'cause' and 'because.' As our earlier reference to Popper indicated, part of what it means to say 'A caused B' is "that events of kind A are always and everywhere followed by events of kind B," i.e. that A and B are nomologically connected. In other words, the very meaning of statements used as evidence, reasons, causes or explanations is such that they are at least implicitly general, that they presuppose generalities or laws which serve to connect the events in question. For example, to say that some piece of thread broke because a two-pound weight was put on it is to say that the same kind of effect will be produced in all relevantly similar cases where the same kind of cause is present. For if one were to deny the latter generalization and still hold that the events and circumstances were relevantly similar, we would be puzzled as to what one meant by the 'because' in the former statement. Hence, 'A caused B,' 'A is a reason for B' and 'A

explains B' all seem to be incomplete or elliptical statements. They are elliptical in the sense that they are relative to or dependent upon the appropriate generalization.[24] Much of the point of this argument, then, turns on the epistemological question of the nature of empirical explanatory or causal statements, and on the Humean answer that all empirical knowledge requires as part of its meaning an appeal to regularities or laws.

The second argument, though closely related to the first, can be developed independently as an argument from challenge. It concerns the way one might defend, say, a causally explanatory statement of the form 'A caused B', if challenged. While a scientist or historian might not always mention a law in the explanation he offers, still, CL theorists argue, in order to defend such an explanation or causal connection against challenge, he would have to invoke some lawlike connection. Only in this way could he claim objectivity for his statement. Inability or unwillingness to specify the lawlike connection would mark the statement as subjective and hence not an objectively genuine explanation at all. Another way of putting this is to say that a scientist's personal explanation makes a claim to being "an explanation," a genuine scientific explanation and not merely a pseudo one. It makes a claim to be more than just his personal explanation. Thus, any explanation will be genuinely empirical and scientific only if it is objectively defensible, and it will be objectively defensible

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[24] Cf. the instructive parallel treatment of moral terms in M. Singer, Generalization in Ethics (N.Y.: Knopf, 1961), pp. 34-60.

only if it presupposes the truth of some empirical lawlike generalization which warrants connecting the events in question.

On the basis of these two arguments, finally, CL theorists maintain that not all deductively valid arguments, but at best only those containing statements of general laws, are explanatory. Yet, of course, not just any lawlike generalization will be genuinely explanatory either. For (R_3) remains to be invoked. In addition to containing essentially general laws, genuine and acceptable scientific explanans must also contain empirically testable or falsifiable statements, i.e. statements with empirical content or import.

However, the question of what constitutes an empirically testable or significant statement has of yet received no generally acceptable answer, not even among CL theorists. Hempel, in fact, would be the first to admit that there is little likelihood of finding such a general criterion applicable to all or even most scientific explanations. We might even have to learn to live with degrees of testability, with some explanatory systems having more than others.[25] In any case, there does seem general agreement that some cases must be ruled out on the basis of (R_3). And while we are unable to make a clear distinction in all cases, it surely does not follow that

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[25] Cf. C. Hempel, "Problems and Changes in the Empiricist Criterion of Meaning," in L. Linsky (ed.), Semantics and the Philosophy of Language (Illinois: University of Illinois Press, 1952); C. Hempel, "The Concept of Cognitive Significance," Proceedings of the American Academy of Arts and Sciences, LXXX (1951-54), 61; and I. Scheffler, The Anatomy of Inquiry, Part II.

anything goes, that any purported explanation is thereby genuine, or that the line between empirically significant and pseudo explanations cannot be drawn in particular cases. Instead of pursuing the complexities and problems of the issues surrounding (R_3), let me turn to a related but more general issue.

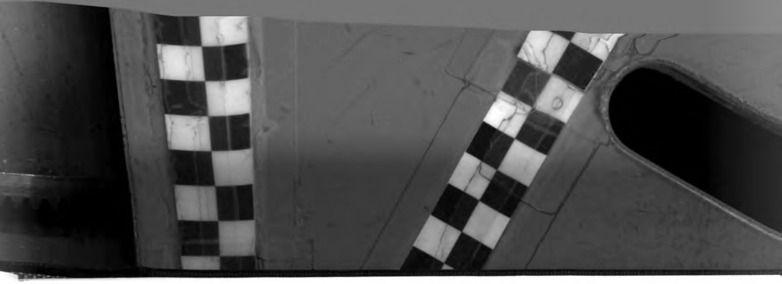
Having examined briefly Hempel's first three conditions of adequacy, it is still not clear what they are conditions of. Are they, e.g., conditions marking off genuine from pseudo explanations, or are they intended instead to distinguish between those genuine explanations that are rationally acceptable and those which are not. Unfortunately, neither Popper nor Hempel has been either explicit or clear concerning these questions. Popper, of course, has for some time used the criterion of falsifiability to demarcate between genuinely scientific and non-scientific but still empirical explanations. But it is not clear whether he also intends this criterion to demarcate between acceptable and unacceptable genuinely scientific explanations.

Again, Hempel at times seems to take the CL theory as an ideal standard of genuine scientific explanation in contrast to pseudo-explanations; but at other times, particularly when commenting generally on the "conditions of adequacy" he is clearly trying to distinguish "sound" or acceptable explanations from inadequate or unacceptable, though genuine and not merely pseudo, explanations. For example, in describing a potential danger of motive or teleological explanations as that of lending itself "to the facile construction of ex post facto accounts without predictive force,"

Hempel never makes clear whether his objection to such cases, which violate (R_3) and hence lack cognitive significance, is that they are pseudo, i.e. merely "alleged motivational explanations," or that they are unsound or unacceptable while still genuine.

Again, Hempel distinguishes in various essays between a potential explanation which satisfies only (R_1) - (R_3) and an actual explanation which in addition satisfies either (R_4) or its weakened version of high confirmation.

An argument will be an actual explanation only if its premises are in addition true or highly confirmed, and hence actually do explain its conclusion. Since we obviously can find many potential explanations for any given event, the problem is to find hypotheses which actually explain, which are either true or highly confirmed. Another way of putting this point is to distinguish between a merely valid argument and a sound or rationally acceptable argument. For then a potential explanation will be formally valid but not necessarily empirically sound, while a sound explanation will also contain rationally acceptable premises or explanans. However, the issue now concerns the ambiguity of the term 'actual' explanation. Clearly Hempel does not mean by this that any argument anyone actually intended to be explanatory in ordinary affairs was thereby genuinely explanatory, for such cases might even violate either (R_1), (R_2) or (R_3) and thus not even be potential explanations. But we are still left with our original puzzle: does 'actual' explanation mean 'an explanation,' a genuine explanation, or does it mean a good, sound, acceptable or better explanation?



Professor S. Barker has recently complained of the CL theory that "it precludes the giving of any real account of what it is for one explanation to be better than another," of how we are "to choose among competing explanatory theories."[26] It would seem then that if Hempel construes "actual" explanation as "sound," "good," "scientifically acceptable" or "better than" others, he is obliged to meet Barker's demand, to indicate some criteria for the application of the latter predicates, as an essential task of his theory of explanation. And in this case (R_1) - (R_4) could be construed as just such criteria or requirements, for (R_4) in particular can be used as a criterion for such a choice. Hence, as Rudner has noted in reply to Barker's charge,[27] nothing in the CL model in any way precludes such criteria for choosing the better among rival putative explanations.

However, Rudner's particular defense of Hempel against this charge depends on our taking "actual" explanation not as good, sound, acceptable or better, which would require criteria for such, but as merely genuine, "not pseudo," "merely putative" or "an explanation," and hence not requiring such criteria. This becomes clear in his reply that "at any rate lack of a criterion for constituting a better explanation does not entail lack of a criterion for constituting an explanation at all."[28]

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[26] S. Barker, "The Role of Simplicity in Explanation," in Feigl and Maxwell (eds.), Current Issues in the Philosophy of Science (N.Y.: Holt, Rinehart and Winston, 1961), p. 273.

[27] R. Rudner, "Comments," in ibid., p. 284.

[28] Ibid.

This, of course, is true. But if the CL model is merely establishing criteria of adequacy for an argument to be "an explanation," as Rudner suggests, instead of being a sound, acceptable or better explanation; then Hempel's objection to ex post facto explanations of, say, motivations or reasons for an action must be that they are not even explanations, that they are merely pseudo-explanations, rather than that they are just unacceptable, unsound or not the best explanations. In other words, lacking predictive power, adequate confirmation or cognitive significance, for example, suffices to incriminate putative explanations as not actual and indeed as not even potential. We seem driven then to the same predicament as above. For, if "actual" explanation is taken to mean simply "an explanation," or a genuine explanation, the opposite of pseudo explanations, then we are forced to concede that most of what we take to be competing explanations of some phenomenon really are not explanations at all. Consequently, just as we will find Hempel finally acknowledging the "questionable merit" of his early defense of (R_4) on these grounds, so it seems we must reject Rudner's construal of "actual" explanation as "an explanation," and hence depict it instead as meaning "scientifically acceptable," or "better" or "sound." Though this does not preclude some such explanations being adjudged better than others.

But then we are still free to take "potential" explanation as meaning "an explanation." In this case there can indeed be many competing genuine explanations of the same phenomenon, viz. all that meet criteria (R_1) - (R_3) or all potentially explanatory sets of of hypotheses. Yet at the same time not all of these will be the best, correct or most acceptable, since not meeting (R_4).

Now, if the above argument is cogent, i.e. if the CL theory of explanation requires criteria of rational or scientifically acceptable explanations, serious problems will arise in regard to Hempel's acceptance of Weber's value-neutrality thesis of science, based as it is on his sharp distinction between pure and applied science, especially when the CL model is extended to cover probabilistic as well as deductive explanations. For if it can be shown, as I will attempt to do in the final chapter, that rationally acceptable statistical explanations require pragmatic criteria and the making of decisions and value judgments; then the CL theory of scientific explanation, as espoused by Hempel and Popper, will require the denial of the value-neutrality thesis as an essential ingredient. And it is largely because this denial depends essentially on a pragmatic construal of the concept of explanation that we will analyze closely the recent criticisms of the CL model and the subsequent reconstruction of Weber's position, made especially by William Dray.

However, one major thesis of the present work will be that Dray's defense of the extension of the notion of explanation so as to include a pragmatic dimension, and his consequent grounds for denying the value-neutrality thesis, are misplaced and need to be redirected. His case for the SU thesis, for example, does not adequately support his accompanying objections to the CL theory of explanation. In other words, I submit that Hempel's advocacy of the CL theory as presented in this chapter and Dray's denial of the value-neutrality thesis are not incompatible positions, as Hempel and Dray seem to believe.

But much remains to be done before attempting a defense of this position. Let us therefore look more closely at Hempel's epistemic condition of adequacy for empirically sound or acceptable scientific explanations. We will want to ask, in particular, whether (R_4) is necessary to distinguish between those genuine scientific explanations which are acceptable and those which are not, or whether this condition is too restrictive and hence requires weakening to that of high confirmation.

Clearly, the three logical requirements mentioned above, (R_1) - (R_3), are not alone sufficient to guarantee the soundness, rational acceptability or adequacy of a scientific explanation. Some sort of empirical or epistemic condition must supplement these three formal requirements. Not as evident, however, is what this condition must be. Aristotle, e.g., stringently required that the premises be true, be known to be true and be "better known" than the explanandum or conclusion. But as Professor E. Nagel clearly indicates in his recent illuminating book,[29] the latter two requirements are unacceptable as originally presented by Aristotle. That the premises be "better known" than the explanandum refers of course to Aristotle's metaphysical notions of "necessary" objects of scientific inquiry and to "first" principles. The former, universals,

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[29] E. Nagel, The Structure of Science (N.Y.: Harcourt, Brace and World, 1961), pp. 42-46. Cf. Aristotle, Posterior Analytics, in W. Ross (Ed.), Complete Works of Aristotle (N.Y.: Random House, 1941), 71b10-72a10.

are contrasted with merely contingent particulars which are not proper objects of science; and the latter, first principles, are those better known by nature in contrast to what is better known to man from sensory perception.

Of more immediate concern is the Hempelian position concerning the first two of Aristotle's requirements. That the premises be "known to be true" in order for an explanation to be scientifically acceptable likewise presents difficulties. For, as Nagel argues, few if any accepted scientific explanations meet this condition and hence would be satisfactory according to it. At best scientific hypotheses or laws seem to be known with more or less degrees of confirmation or probability. Hence, if the requirement is insisted upon, it would simply lead in practice to the introduction of a new term to distinguish "known to be true" from "known with high confirmation."[30]

Nevertheless, these considerations suggest that the stipulation might be more acceptable in a weaker though perhaps more vague form instead of merely discarded altogether. In fact, this is the alternative Hempel opted for initially when he maintained that, along with a set of initial conditions, "the scientific explanation of the event in question consists of ... a set of universal hypotheses, such that the statements of both groups are reasonably <u>well confirmed</u> by empirical evidence...."[31] Let us refer to this weaker

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[30] <u>Ibid</u>., p. 43.

[31] Hempel, "The Function of General Laws in History," p. 345.

condition as (R_4'). However, in a later essay the requirement was changed from this modified version of Aristotle's second condition to his first condition, i.e. from the epistemic requirement of "reasonably well confirmed" to the much more rigorous requirement (R_4), that the explanatory premises or explanans be true.

Now, the reason for this change deserves some attention. It is not that the weaker requirement is vague or that no precise and generally accepted standard is available for judging when an hypothesis is "reasonably well confirmed by empirical evidence," even though this is no doubt the case. Instead, Hempel defended (R_4) by arguing that (R_4'), the well-confirmedness requirement, leads to "awkward consequences," viz. to a relativized concept of explanation.

> Suppose that a certain phenomenon was explained at an earlier stage of science by an explanans which was well supported by the evidence then at hand, but which had been highly disconfirmed by more recent empirical findings. In such a case we would have to say that originally the explanatory account was a correct explanation, but that it ceased to be one later, when unfavorable evidence was discovered.

Furthermore, the awkwardness and hence the erroneous aspect of this temporal relativization consists in its counterintuitiveness, i.e. in the fact that

> This does not appear to accord with sound common usage, which directs us to say that on the basis of the limited initial evidence, the truth of the explanans, and thus the soundness of the explanation, had been quite probable, but that the ampler evidence now available made it highly probable that the explanans was not true, and hence that the account in question was not-- and had never been-- a correct explanation.[32]

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[32] Hempel and Oppenheim, "Studies in the Logic of Explanation," p. 322.

The charge of counterintuitivity results from Hempel's belief that according to sound common usage the correctness of a given explanation is independent of temporal factors, just as is the truth of a given statement. Still, it remains unclear how, or even why, common usage or counterintuitivity serves as a relevant criterion for judging such cases. And this is not, it would appear, an unimportant consideration. Hence it is unfortunate that Hempel fails to elaborate upon the relationship between his theory or explication of the notion of scientific explanation and the ordinary usage(s) of the term. At any rate, while this issue has been pointedly pressed by recent critics and will require subsequent treatment shortly, M. Scriven helps to clarify the situation somewhat by directing our attention to another facet of Hempel's defense of the requirement of well-confirmedness.[33]

A fundamental defect of this seemingly persuasive argument is the ambiguity discussed earlier concerning the term 'explanation.' For Hempel's defense turns on a shift from an analysis of 'explanation' which admits of many legitimately competing explanations of the same phenomenon some of which are not well-confirmed, *i.e.* as a possible potential or genuine explanation; to an analysis which does not countenance such competition, *i.e.* as a "correct" or acceptable explanation or even in some cases as "the" explanation.

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[33] M. Scriven, "Explanations, Predictions and Laws," in Feigl and Maxwell (eds.), *Minnesota Studies in the Philosophy of Science*, Vol. III (Minneapolis: University of Minnesota Press, 1963), pp. 190-1.

If the truth-requirement were upheld, surely an even more awkward consequence would result.

For then what would the false explanation be, if not an explanation? What would an invalid argument be if not an argument, or a false proposition? Surely Hempel's proposal of (R_4) is at least as counterintuitive as the requirement of well-confirmedness (R_4'). Besides the tale that lies therein concerning the use of counterintuitivity as a criterion for deciding such cases, have we not good reason to accept (R_4')? No doubt the correct or better explanation is obtained only when we have uncovered true premises, at least ideally. Still, the only way of discovering which genuine explanation is likely to be true and hence to satisfy (R_4) is by employing the notion of evidential strength and choosing the one with the highest degree of confirmation.

As the result of such considerations Hempel, in his most recent and most complete analysis of the logic of explanation, "Deductive-Nomological vs. Statistical Explanation," acknowledges the "questionable merit" of his earlier defense of (R_4), the truth-condition, in the following passage.

> For in reference to explanations as well as in reference to statements, the vague idea of correctness can be construed in two different ways, both of which are of interest and importance for the logical analysis of science: namely, as truth R_4 in the semantical sense, which is independent of any reference to time or to evidence; or as confirmation by the available relevant evidence R_4' -- a concept which is clearly time dependent.[34]

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[34] Hempel, "Deductive-Nomological vs. Statistical Explanation," in Minnesota Studies in the Philosophy of Science, Vol. III, p. 102.

He then proceeds to distinguish between true explanations and those that are more or less well confirmed by a given body of evidence. Accordingly, he defines a "potential explanation" or genuine scientific explanation as one meeting (R_1) - (R_3), *i.e.* as one whose explanans need contain a set of statements, L_1, L_2 ..., L_m, which are empirically testable and also lawlike (*i.e.* laws except for possibly being false) instead of necessarily being laws and hence true. In turn, "true explanation" and "well-confirmed explanation" are derivatively defined as potential explanations whose explanans satisfy (R_4) or (R_4') respectively.[35] The upshot then seems to be that agenuine explanation must satisfy conditions (R_1), (R_2) and (R_3). But in order to qualify as scientifically adequate or correct and hence to provide complete understanding of why something did or will occur, genuine explanations must also meet either (R_4') or (R_4).

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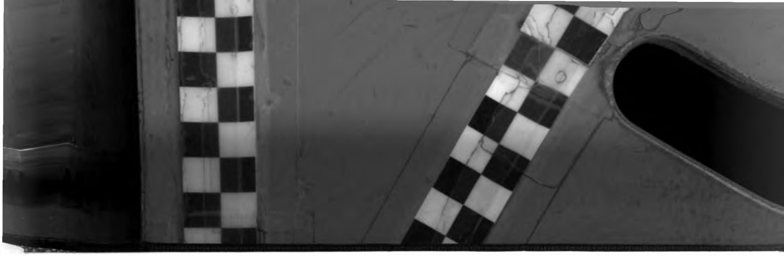
35 *Ibid*., pp. 102-3.



C. Hempel's Probabilistic Model

So far we have attended almost exclusively to the deductive-nomological model of explanation, endorsed commonly by Popper, Hempel and all other CL theorists. But before considering the notion of complete explanation, and in what sense explanation qua potential prediction constitutes an adequate pattern of a complete explanation, mention must first be made of another pattern of explanation more recently endorsed by Hempel, one with no little import and interest for historical explanations and the value-neutrality thesis. This pattern is of course a non-deductive, probabilistic, inductive or statistical systematization of explanation. And its only difference with model (D) lies in the fact that the lawlike statements in the explanans can be statistical. This requires a weakening of (R_1) to the logical relationship of inductive probability between explanans and explanandum, which we shall call (R_1').

Lest this appear as a recent innovation or stipulative expediency, it should be remarked that in his initial essay on explanation, Hempel suggested that the deductive pattern was not the only ideally complete model of explanation.

> Many an explanation offered in history seems to admit of an analysis of this probabilistic kind: if fully and explicitly formulated, it would state certain initial conditions, and certain probability hypotheses, such that the occurrence of the event to be explained is made highly probable by the initial conditions in view of the probability hypotheses.[36]

And again in "Studies in the Logic of Explanation," he and Oppenheim refer to the subsumption of the explanandum under statistical

[36] Hempel, "The Function of General Laws in History," pp. 350-1.

laws. But here they recognize that "Analysis of the peculiar logical structure of that type of subsumption involves special problems."[37]

Thus it is clear that while these essays are restricted to an analysis of the deductive causal type of explanation, Hempel makes no claim that this pattern constitutes the only kind of legitimate or genuine scientific explanation. This fact is of some import since many critics of the CL theory rest their objections on this very claim, much to the detriment of their arguments. Perhaps such an oversight is excusable in a sense, however, because of Hempel's failure to do more than mention the existence of a different kind of explanatory pattern in these early essays. His failure to elaborate its status and "peculiar logical structure," is, I think, partly responsible for some of the widespread misconception of his own views. Not until publication of much of this criticism did Hempel finally elucidate the probabilistic-nomological or statistical pattern.[38] Perhaps of even more importance for this misunderstanding is the fact that other defenders of the CL model, such as Popper and Professor M. Brodbeck, have still not

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[37] Hempel and Oppenheim, "Studies in the Logic of Explanation," p. 324. My italics.

[38] Hempel has elaborated this probabilistic version of the CL theory in greatest detail in "Deductive-Nomological vs. Statistical Explanation," but less complete accounts also appear in the following essays: "The Theoretician's Dilemma," Minnesota Studies in the Philosophy of Science, Vol. II, 1958; "The Logic of Functional Analysis," in L. Gross (ed.), Symposium on Sociological Theory (Evanston, Illinois: Row, Peterson, 1959); "Inductive Inconsistencies," in Logic and Language (Holland: Reidel Publishing Co., 1962); "Reasons and Covering Laws in Historical Explanation;" and "Explanation in Science and History," in R. Colady (ed.), Frontiers of Science and Philosophy (Pittsburgh: University of Pittsburgh Press, 1962).

relinquished the exclusive claim of the deductive model.

Unlike the deductive-nomological systematization (D), which contains laws and theoretical principles of strictly universal form, probabilistic or inductive explanations account for a given phenomenon nomologically by reference to laws of probabilistic-statistical form. Such statements usually assert that if certain specified conditions are realized, then an occurrence of such and such a kind will come about with such and such a statistical probability, roughly with long-run relative frequency. The basic laws of genetics, the fundamental principles of quantum mechanics, and the laws of radioactive decay are examples of such probability statements used in science for the systematization of various empirical phenomena.

As an illustration, Hempel suggests that

> the subsiding of a violent attack of hay fever in a given case might well be attributed to, and thus explained by reference to, the administration of 8 milligrams of chlor-trimeton. But if we wish to connect this antecedent event with the explanandum, and thus to establish its explanatory significance for the latter, we cannot invoke a universal law to the effect that the administration of 8 milligrams of that antihistamine will invariably terminate a hay fever attack: this simply is not so. What can be asserted is only a generalization to the effect that administration of the drug will be followed by relief with high statistical probability....

Hence the explanans will take the following form:

> John Doe had a hay fever attack and took 8 milligrams of chlor-trimeton.
> The probability for subsidence of a hay fever attack upon administration of 8 milligrams of chlor-trimeton is high.[39]

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[39] Hempel, "Explanation in Science and History," p. 13.

Since the logical connection between this explanans and the explanandum, "John Doe's hay fever attack subsided," is clearly not deductive, the form of the logical transition not being uniformly truth-preserving, the truth of the explanans makes the truth of the explanandum at best likely or "practically certain." The requirement of deducibility (R_1) is thus weakened to that of probability (R_1').

Such an inductive or probabilistic-nomological systematization can be represented by the following schema:

$$
\begin{array}{ll}
\text{(P)} & Fi \\
 & p\,(O,\ F) \text{ is very high} \\
\hline\hline
 & Oi
\end{array}
$$

Here the explanandum, expressed by 'Oi', the fact that in this particular instance, i, (John Doe's allergic attack), an outcome of kind O (subsistence) occurred, is explained by two explanans-sentences. The first, 'Fi', corresponding to C_1, C_2, ... C_k in (D), asserts that in case i, the factors F were realized. The second, a law of probabilistic form, states that the statistical probability for O to occur in cases where F is realized is very high or close to 1. Finally, the double line represents the logical relation of inductive probability , high confirmation or likelihood, in contrast to that of deductive implication in (D).

Hempel, following Carnap's account, also stresses the distinction between the two kinds of probability statements, between the notion of likelihood and that of statistical probability, occurring in (P). Statistical probability concerns the long-run relative frequency with which one occurrence (say F) is accompanied by another (say O) and hence is a relation between kinds of occurrences. The former notion of likelihood, however, is a logical relation between statements and refers to the degree of rational credibility, evidential strength or of inductive support conferred upon the explanandum by the explanans. Or, in Carnap's terms, it is the logical or inductive probability possessed by the explanandum relative to the explanans.

The covering-law theory of explanation finally emerges, then, as two distinct patterns even though each refers to a certain kind of subsumption under covering law, statistical or strictly universal. The difference between them lies in the character of the laws invoked, and hence in the logical relationship linking premise and conclusion.

If it be asked whether patterns (D) and (P) are really or essentially distinct logical models, an affirmative answer can be supported by noticing the following quite distinct fundamental logical characteristics. The deterministic 'because' is a deductive, either-or, unambiguous relation. The statistical one, however, is an inductive relation, admitting of degrees, and exhibiting an ambiguity which calls for relativization to the total evidence available.

Moreover, this very difference gives rise to many complex problems about statistical explanations. For example, one of the most compelling aspects of model (D) is the requirement that the explanans provide reasons for ruling out the possibility of the

explanandum-event failing to occur, and hence for showing conclusively why it actually did occur. In other words, (R_1) requires the explanans to focus attention on precisely what was to be explained. But once the connection is weakened to (R_1'), a suspicion arises that the statistical laws abandon this focus on the particular case, since they are compatible with both the occurrence and non-occurrence of the particular explanandum-event.

If this is so, then in what sense, we might ask, does a probabilistic explanation offer any explanatory understanding? What constitutes its explanatory force or import? To answer this requires showing how and why statistical laws lose their hold on individual case, in what way they are compatible with both E and non-E, and what additional requirements can be imposed on model (P) to eliminate this objectionable feature. Some aspects of this problem, which turn on the peculiar ambiguity or inconsistency of inductive explanations, will be examined in our final chapter, since they bear heavily on the value-neutrality thesis. From this brief characterization of model (P), however, it should be clear that for any probabilistic explanation to qualify as scientifically adequate it must, like deductive ones, satisfy (R_2), (R_3) and either (R_4) or (R_4') by containing empirically testable and at least highly confirmed general laws with potential predictive power.

The question might arise, at this juncture, as to the nature and status of these two models or patterns, (D) and (P), particularly in view of the relationship between Hempelian explanation and prediction such that an explanation of form (D) or (P) is not complete unless it is potentially predictive or might have served as a prediction. To be sure, seldom if ever are explanations in ordinary practice, historical or even scientific inquiry ever complete in this sense of the term. Seldom do our explanations satisfy the four conditions of adequacy stipulated by Hempel. The charge, accordingly, that such models are too rigorous or too far removed or abstracted from our usual explanatory practice to reflect adequately such practice will surely be raised. After all, it seems not an unimportant fact that we cannot find a clear unambiguous case of a complete explanation in Hempel's sense. It is not then a useless category? Would it not really be more scientifically and philosophically fruitful to replace Hempel's sense of "completeness" with one, or perhaps many different kinds, actually manifested in our scientific practice.

This kind of objection, raised by both idealists and some followers of Wittgenstein and Ryle, usually receives an official reply from Hempelians, as indicated earlier. It amounts to the counter-claim that the task of the philosopher is not merely to record, mirror or describe the actual explanatory practice of working scientists or historians, but rather to construct a general theory in which these practices receive a systematic analysis, codification or rational reconstruction. Use of the term "model," Hempel suggests, reminds us that the two types of explanation as characterized by (D) and (P) constitute "ideal types or theoretical idealizations,"[40] and as such provide explications of certain modes

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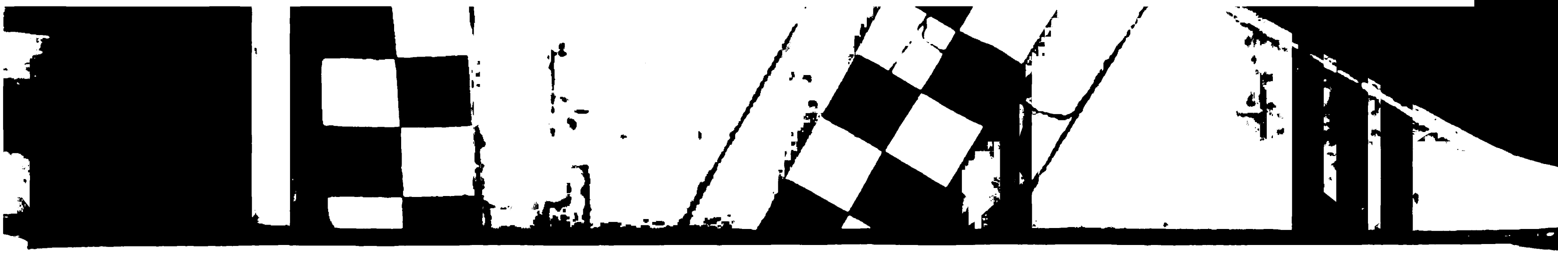
[40] Ibid., p. 15.

of scientific explanation. In most illuminating and persuasive fashion, he compares them to the concept of mathematical proof as used in meta-mathematics, a concept also regarded not as a mere descriptive account of how mathematicians actually formulate proofs, but as a theoretical model or ideal standard to which the actual proofs only approximate.

Such a theoretical model also serves additional functions, for actual explanations as well as actual proofs. It exhibits the rationale of explanations by revealing their logical structure, provides standards for a critical appraisal of any explanation of the kind governed by the model, and affords a basis for a theory of explanation, prediction, confirmation and related concepts. In sum, complete explanations are not attainable goals or objectives but rather ideals which, though unattainable in our actual explanatory practice, may still be approached or approximated closer and closer. Hence, in this sense, an explication or theory of explanation is said to be treated theoretically as context-free. It is related to and respectful of, but not bound by, our actual usage of the term in actual contexts of application.

This brief characterization or outline, barely indicated by Hempel, gives rise then to many additional problems. Can we specify more precisely, e.g., in what sense the two models are context-free? And how they are related to actual contexts? Is it in fact possible to exhibit the rationale of explanations and to provide standards for their critical appraisal by the use of idealized models without introducing, as Popper suggests, pragmatic considerations of intentions and purposes associated with actual explanatory contexts? And if not, what sort of controls, if any, are there on the idealized models themselves? How, e.g., do we determine what the ideals of science are at any given time? Are they relative to time and our scientific evidence or are they somehow purely formal or perhaps philosophic matters for which empirical evidence is irrelevant? Moreover, can we specify some of the ways in which actual explanations offered in the sciences, and in history, fall short of the ideal of completeness? Is it possible to arrange them in some order of degrees of approximation to the ideal? And finally are there different senses of "completeness".

In the remaining section of this chapter I want to consider two major aspects of these questions. First we will examine the notion of completeness, some of the kinds of approximations to ideal completeness and what it is that we can completely explain. The theoretical, context-free aspect of philosophic explication will then be discussed in this context, where Popper's pragmatic emphasis on explication will be further developed.



Complete Explanation and Approximations

We have already suggested that according to the CL theory any purported explanation which violates (R_1), (R_2) or (R_3) fails as even "an explanation"; it is a pseudo instead of a genuine scientific explanation. And since deducibility is largely where you find it, being independent of the presence of laws, while testability is built into the notion of an empirical law, much of the emphasis on whether or not an explanation is complete revolves around (R_2), i.e. the inclusion of empirical laws among the explanans statements. Moreover, there seem to be many important kinds of purported explanations, especially those proposed by historians, which fall short of model (D) or (P) but which would not ordinarily or pre-analytically be considered pseudo explanations. Hence, rather than do violence to ordinary practice by construing all violations of (R_1) - (R_3) as pseudo explanations, Hempel has from his initial essay countenanced a separate category of incomplete cases. Such explanations fall between complete and pseudo ones. They can also be considered, I think, as degrees of deviation from or approximations to the appropriate model as an ideal type, standard or regulating principle. We will limit our discussion to three different degrees of deviation from model (D), but many more could perhaps be specified.

First of all, when presenting an explanation, a scientist or historian will often merely omit mention of some statements which he presupposes implicitly in his argument. Judged by ideal standards, the argument will be incomplete in the inessential sense of being an elliptical or enthymematic formulation. When we explain,

e.g., that a car radiator broke because it was left in the cold and was filled with water, or that a small rainbow appeared in the spray of the lawn sprinkler because the sunlight was reflected and refracted by the water droplets, we tacitly assume certain general laws or particular facts, which we assume others can readily supply and which could be explicitly cited so as to yield a complete argument. Hempel offers two reasons to account for why most explanations offered in history and sociology are thus elliptical: either the universal hypotheses are so familiar to everyone from ordinary experience as to be tacitly taken for granted, or it is too difficult to formulate them explicitly with specific precision without loss of empirical content. It is conceded, too, that "in many cases, the content of the hypotheses which are tacitly assumed in a given explanation can be reconstructed only quite approximately."[41]

Another, more essential and important, degree of approximation deviates still further from the theoretical model. For often, even when we have reasonably reconstructed the implicit hypotheses assumed or taken for granted, they, together with the statements explicitly stated, explain the given explanandum only partially. This kind of deviation or incompleteness is of special interest in social, psychological, and historical explanations. Such explanatory arguments are referred to as partial and usually occur in cases of explaining particular events functionally, i.e. according to the function or role the item serves in the operation or maintenance of some larger

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[41] Hempel, "The Function of General Laws in History," p. 350.

system. The explanans statements of such arguments, though providing deductively conclusive evidence for expecting some item or member of a class of events K to occur, offer only inductive support for expecting the occurrence of the particular explanandum item or event X as a member of K. In other words, while the explanans completely explains why some member or other of K had to occur, it only partially or inductively explains why the particular member was X instead of some other member of K. In case X were the performance of a given kind of action K, a partial explanation would consist in explaining deductively why some action or other of kind K had to occur, but only inductively why the particular action X did actually occur.

The main point of partial explanations can be illustrated by Freud's account of a written "slip" in his "Psychopathology of Everyday Life."

> On a sheet of paper containing principally short daily notes of business interest, I found, to my surprise, the incorrect date 'Thursday, October 20th,' bracketed under the correct date of the month of September. It was not difficult to explain this anticipation as the expression of a wish. A few days before I had returned fresh from my vacation and felt ready for any amount of professional work, but as yet there were few patients. On my arrival I had found a letter from a patient announcing her arrival on the 20th of October. As I wrote the same date in September I may certainly have thought 'X ought to be here already; what a pity about that whole month!', and with this thought I pushed the current date a month ahead.[42]

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[42] S. Freud, The Basic Writings of Sigmund Freud (N.Y.: Modern Library, Random House, 1938), p. 89; cited by C. Hempel, "Explanation in Science and History", op. cit., p. 17.

Freud's intended explanation is clearly incomplete in the sense of being elliptical since it implicitly assumes but does not explicitly mention any general laws or hypotheses connecting the slip of the pen S with the subconscious wish W and other antecedent circumstances. There seems little doubt, however, that his explanation relies on some such hypothesis as (1) "when a person has a strong, though perhaps unconscious, desire, then if he commits a slip of the pen, tongue, memory, or the like, the slip will take a form in which it expresses, and perhaps symbolically fulfills, the given desire."[43]

Yet even if the reconstructed hypothesis (1) is included in the explanans together with the appropriate singular statements, the resulting explanans still does not permit the deduction and hence the complete explanation of the explanandum. Since Freud's subconscious wish could, of course, be expressed and symbolically fulfilled by many other kinds of slips of the pen than S, the explanans at best permits deduction of the more indeterminate conclusion that Freud's slip "would, in some way or other, express and perhaps fulfill his subconscious wish."

> In other words, the explanans does not imply, and thus fully explain, that the particular slip, say S, which Freud committed on this occassion, would fall within the narrow class, say W, of acts which consist in writing the words 'Thursday, October 20th'; rather, the explanans implies only that S would fall into a wider class, say F, which includes W as a proper subclass, and which consists of all acts which would express and symbolically fulfill Freud's subconscious wish *in some way or other*.[44]

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[43] Hempel, "Explanation in Science and History," p. 17.

[44] *Ibid.*, pp. 17-18. Cf. also Nagel's instructive analysis of partial explanations in *The Structure of Science*, pp. 552-558.

Clearly then, this kind of incompleteness constitutes a much more serious form of incompleteness than an elliptical explanation. But more importantly, we might note at this point that it also raises a question of the relation between Hempel's two models, (D) and (P). For, at least in examples like that of the "Freudian slip," Hempel seems to consider part of the incompleteness as due to the fact that Freud's reconstructed explanation is of the probabilistic form (P) and hence falls short of (D). If so, any explanation of the form (P) would for that very reason be an incomplete explanation. Hence instead of having two fundamentally distinct and equally complete but different kinds of ideal models, we would possess only one covering law model of complete explanation, viz. the deductive model (D). If so, it would appear that such critics of the deductive model as Dray and Scriven are not guilty of misrepresenting Hempel's proposed explication. However, Hempel has explicitly denied that (P) is any less an ideal model than (D). But if this is the case, i.e. if (P) is an autonomous ideal model, then Hempel is surely required to elucidate, more than he has, in what way the probabilistic model can be complete in itself, i.e. what its completeness conditions are.

Professor Brodbeck, in a lively and lucid defense of the deductive model, opts for the former position of only one model,[45] while Hempel's recent treatment of the subject would indicate a

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[45] M. Brodbeck "Explanation, Prediction and 'Imperfect' Knowledge," in Minnesota Studies, Vol. III. pp. 238-9.

preference for the latter alternative of two independent models and an attempt to codify some of the required conditions. And it is these conditions of adequacy for probabilistic explanations that raise themost serious difficulties for Hempel, as Chapters V and VI will attempt to show. Part of the confusion here results from Hempel's earlier concentration on the deductive model and his resulting lack of attention to the completeness conditions of model (P). It is this, I suspect, which leads him to contrast the two kinds of inference which occur in a partial explanation. For when juxtaposed in the same argument, the grounds for expecting S to be a member of F, which are conclusive, seem somehow to be better or more complete than the inconclusive but perhaps highly probable grounds for expecting S to fall within W, since the latter grounds do not strictly imply this explanandum.

In fact, it is not at all clear why Hempel would characterize such explanations as "partial" unless he thereby meant to suggest that they do not fully or completely explain, according to the model (D). But since the reason for this claim, that the explanans does not deductively entail the explanandum, applies to all inductive arguments, it would naturally follow that model (P) is itself incomplete and not an ideal explication of a different kind of scientific explanation. At best, then, Hempel has marked a distinction in such functional explanations as Freud's between the conclusion that can be deductively inferred, "S is a member of F," and the one that can be only inductively inferred, "S falls in class W." But such a distinction is nevertheless of major interest to the explainer. By using the terms "complete" and "partial" to so mark this distinction, however, he tends to confuse the issue of completeness, and with it his claim of the independent status of the two models and of the two kinds of explanation they represent.

As a final case of deviation from the ideally complete pattern of explanation (leaving open for the moment whether this is (D) or (P) or both), there is what amounts to a lower limiting case in the continuum of approximations. Some explanatory accounts depart even further than elliptical or partial ones and in fact border closely on being untestable pseudo explanations. In an early essay Hempel labeled such accounts "explanation sketches" and described them as:

> a more or less vague indication of the laws and initial conditions considered as relevant, which needs 'filling out' in order to turn into a full-fledged explanation. This filling out requires further empirical research for which the sketch suggests the direction.[46]

Consider two examples: first, we might explain that the Dust Bowl farmers migrated to California because continual drought and sandstorms rendered their existence extremely precarious, which seems to assume that populations tend to migrate toward regions with better living conditions. Secondly, a particular revolution might be explained by reference to the discontent of a large part of the population, together with certain prevailing conditions. While a general regularity is implicitly assumed as the connecting

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46 Hempel, "The Function of General Laws in History," p. 351.

link in this case also, it is most difficult to know to what extent and what specific form the discontent has to assume, and what environmental conditions must be, to bring about a revolution. Still, the sketch contains no empirically insignificant terms and does seem to offer direction for research into conditions which might tend to confirm or refute the more specific implicit statements of the explanans.

Now we have so far considered, however briefly, all of the conditions of adequacy laid down in "Studies in the Logic of Explanation." (R_3), that the explanans have empirical content, requires more consideration in this context, since the problems surrounding the distinction between pseudo explanations and genuine explanatory sketches turn on just this condition. For, while some might take the above description and examples of such a sketch as illustrating a pseudo explanation, others might charge that such cases are not merely sketches or incomplete in any way but rather are explanations of a different kind altogether and hence complete of their type. Such an issue turns largely on how rigorously the criterion of significance or empirical meaningfulness is employed, a problem which, as we noted earlier, is much too complex to be handled adequately in this paper.

Nevertheless, independently of whatever criterion (or better, criteria) of significance turns out to be adequate, it remains the case that "an explanation sketch does not admit of an empirical test to the same extent as does a complete explanation; and yet there is a difference between a scientifically acceptable explanation sketch and a pseudo explanation (or a pseudo explanation sketch)."[47] The difference, simply put, is that pseudo explanations have no empirical content and thus are untestable in principle, thus violating (R_3). So wherever the line between empirical significance and non-significance may happen to be drawn, sketches and pseudo explanations will occupy opposite sides. And this is because of the employment in the latter of empirically meaningless terms, which precludes even a rough indication of the kind of inquiry that might lead to evidence either confirming or disconfirming the purported explanation.

Since Hempel's concern is not only to set a lower limit to the degrees of deviation from the ideally complete model of explanation but also, by so doing, to distinguish legitimate or genuine explanations (in whatever degree) from pseudo ones, it might appear that his position diverges drastically from Popper's view, mentioned earlier, concerning historical interpretations or theories. You will recall that while Popper likewise contrasted such interpretations and scientific theories in so far as the former were untestable, he nonetheless gave them an important role and status in historical and social inquiry as foci or working hypotheses for collecting additional information. In fact, their role and status was to be precisely that which Hempel assigns to explanation sketches. Yet Hempel considers them testable and hence scientifically acceptable, while Popper takes them to be untestable or unfalsifiable but still

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[47] Ibid.

scientifically useful and even appraisable in some sense. Part of this disagreement is merely apparent. And it is so largely because of an obvious ambiguity concerning the terms "untestable" (or, to use Popper's terminology "unfalsifiable") and "scientific." Popper views such interpretations or sketches as empirically significant, but he disagrees with Hempel by giving them, for this reason, scientific status also. In other words, his notion of testability is designed as a criterion of both empirical significance and scientific status.

I am not suggesting that there are no important differences on this issue between Hempel and Popper, or that their disagreement is completely verbal, for indeed I think there are such differences.[48] But for our present purposes, it suffices to show that they are agreed on taking sketches to be empirically testable, and hence satisfying (R_3), to be lower approximations to an ideal model of explanation, to be scientifically fruitful as heuristic hypotheses guiding their own development toward completeness or "filling in," and hence distinguishable from pseudo explanations. One point of importance emerging from the foregoing, then, is that neither Popper nor Hempel denies the existence of purported or potential explanations which do not conform completely to the requirements of the ideal models (D) and (P). Instead, it has been argued, their position regarding such purported explanations can be characterized as incomplete approximations to the ideal models.

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[48] Cf. I. Scheffler's penetrating discussion of this issue in <u>Anatomy of Inquiry</u>, pp. 137-50.

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If the above considerations are cogent, we can take the CL theory of explanation to consist in part of two ideal models of completeness and various degrees of approximation to these models or degrees of incompleteness. The notion of completeness however is still not as clear as might be. There seem to be various meanings or senses of the term as used in recent discussions, four of which I want now to examine. Let us refer to them as deductive, concrete, factual and descriptive completeness.

One such usage, the deductive completeness alluded to earlier in our comparison of models (D) and (P), derives from Hempel's earliest essay on explanation, and has subsequently been the source of much debate and criticism, most recently by Dray and Scriven. In this sense, we have a complete explanation only when our explanans contains strictly universal laws and the explanandum is logically entailed by the explanans; in other words, only when we have a deductive-nomological explanation of the form (D). And since probabilistic-nomological explanations of the form (P) invariably contain statistical or probabilistic premises in their explanans, which thus implies its explanandum not with deductive necessity but only with more or less high probability, they must be intrinsically incomplete in this sense. If this is the intended usage, then clearly for Hempel there would be only one ideal model of explanation, not two independent ones. In his most recent discussion of this meaning,[49] Hempel does little more than acknowledge

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[49] Hempel, "Reasons and Covering Laws in Historical Explanation," pp. 151-2.

its existence, not at all clarifying his own view as to whether or not it is a sense of completeness he intends or even finds acceptable.

Three other uses of the term remain to be considered, each of which involves ontological questions as to just what it is we explain, whether partially or fully. It should be noted that the first sense of completeness just mentioned, that of deductive completeness, applies only to the explanation of *aspects* of events described by statements not to the explanation of concrete events themselves. This point emerges more clearly perhaps by a consideration of the second sense in which completeness has been taken, what we shall call concrete completeness. Often 'complete explanation' has been used to mean something like Weber's "grasping the unique individuality of concrete events in their infinite variety or fullness." This usage is associated mostly, I suppose, with an idealist defense of the autonomy of history or the Geisteswissenschaften generally. And for this reason both Popper and Hempel have, on various occasions, registered their objections to it. In this context, an individual event is typically characterized, as Hempel says, "by an individual name or by a definite description, such as 'the Children's Crusade,' 'the October Revolution,' 'the eruption of Mt. Vesuvius in 79 A.D.' ... and the like." The objection to this usage, *i.e.* to talk of a complete explanation of such *concrete events*, is based on the grounds that

> Individual occurrences thus understood cannot be explained by covering laws nor in any other way; indeed it is unclear what could be meant by explaining such an event. For any event thus understood has infinitely many aspects and thus cannot be even fully described, let alone explained ....

> Evidently, a complete characterization, let alone explanation, of an individual event in this sense is impossible.[50]

Thus, Hempel wisely preempts the counter-charge that his own position implies a mechanistic view of man, society or historical processes, and employs "robustly materialistic language." Moreover, another objection to the position of explanation as a relationship between explanatory premises and concrete events, noted by Scheffler,[51] exhibits the thrust of Hempel's point. The force of the criticism turns on a consideration similar to that which produces the inconsistency or ambiguity of inductive inference. For if we talk of explaining the event b by providing appropriate statements A and L, having b's description, B, as a logical consequence (much as Popper[52] does on at least one occassion), we are led to a contradiction. Suppose, _e.g._ we have a particular spatio-temporal chunk, k, described as blue by the statement, (1) Bk, and as hot by the statement (2) Hk. And suppose additionally we have the following set of premises:

(3) $(x)\ (y)\ (Wx \,.\, Rxy \supset Hy)$

(4) Wj

(5) Rjk

where 'j' represents a spatio-temporal chunk or slice, 'W' a predicate applicable so such chunks, 'R' a relational predicate applicable

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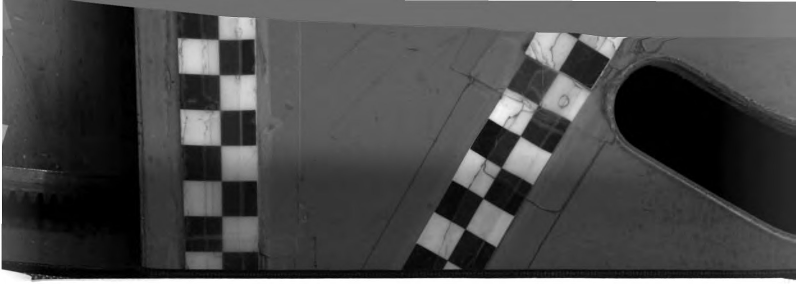
50 _Ibid._, p. 150.

51 I. Scheffler, _Anatomy of Inquiry_, pp. 58-9.

52 K. Popper, _The Open Society and Its Enemies_, Vol. II, p. 362, note #7.



to certain pairs of them, and (3) is assumed to be lawlike. If we further assume that (3), (4) and (5) are true, the deductive pattern will clearly apply to (2) as an explanandum.

Roughly, then, we can say that (3) - (5) constitutes an explanation of k, or in Scheffler's symbols: (6) ' E(3) - (5), k '(to be read: (3) - (5) explains k). Yet, (3) - (5) just as surely does not explain k, since not yielding (1) as a logical consequence and hence not explaining the event described by it, viz. : (7)' E (3) - (5), k' (to be read: (3) - (5) does not explain k). Accordingly, we end with a contradiction regarding (6) and (7), since (3) - (5) both explain and do not explain k. Furthermore, the reason appears to be that concrete events or spatio-temporal chunks are describable in alternative, logically independent ways, hence making it false to parade any given description of such a concrete individual as its unique description. As in the above example, one of these alternative descriptions may well be implied by a given explanans while others may not.

It is perhaps clearer now why Hempel insists that deductive completeness applies only to aspects of events as described by statements and not to concrete individual events characterized by a definite description or an individual name. The above considerations, moreover, point to a way of avoiding the contradiction and, as a result, to a third or factual sense of completeness. Instead of depicting scientific explanation as a relationship between explanans and concrete events, Hempel opts for a relationship between explanans sentences and "aspects of, or facts about, concrete

<u>events</u>,"[53] <u>i.e.</u> facts about events or chunks-as-qualified-in-certain-ways. In our example, we should take (3) - (5) as explaining k-as-described-by-(2), not as explaining k. Consequently, we are a long way from any sort of "robust materialism." What is explained on this is not a concrete event or

> spatio-temporal individual chunk, but something of another sort associated with it, of which there are as many as there are logically independent descriptions of the chunk. These new entities (let us call them hereafter 'facts') are not themselves spatio-temporal entities: they are neither dated nor bounded. Nor are they identified with the descriptions themselves. They are abstract ('logically intensional') entities, intermediate between chunk and descriptions, each such entity corresponding to some class of logically equivalent (true) descriptions uniquely.[54]

By the introduction of facts, then, Hempel apparently resolves the above-mentioned problem, since in place of the one entity or chunk, he now embraces two entities of a different kind associated with it: facts. In short, the contradiction is avoided since what is explained and also not explained by (3) - (5) is no longer the same thing, but two different things. The fact that k is hot is explained by (3) - (5), $(8)'\ E\ (3) - (5),\ f(Hk)'$; while it is instead the fact that k is blue which is not explained, $(9)' \sim E\ (3) - (5),\ f(Bk)'$. Thus, this third sense of 'complete explanation,' the only one in which Hempel countenances a covering law explanation of an individual event, amounts to a complete explanation of a particular aspect or fact about a concrete event.

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53 Hempel, "Reasons and Covering Laws in Historical Explanation," p. 150.

54 Scheffler, <u>Anatomy of Inquiry</u>, pp. 59-60.

But Hempel also speaks of a fourth sense of 'complete explanation,' a sense in which one might speak of partial and more complete descriptions as well as explanations of concrete events. Instead of completely explaining one aspect or fact about a concrete event, one set of explanations might explain more aspects or descriptions of it than does naother, and in this sense be said to be more complete than another set. A simple case would occur when the aspects explained in one set of explanations, S_1, each of which explains some description or aspect of a concrete event, forms a proper subset of those aspects explained in another set, S_2. In this case S_2 provides a more complete explanation of the event than does S_1.

However, Hempel's appeal to an ontology of facts about or aspects of concrete events, to replace particular individual events as the proper objects of explanations, itself needs defense against various charges. Two such objections will be considered briefly. One might charge, first, that Hempel's proposal is liable to the same sort of difficulty as is the event-ontology, only on a new level. In other words, since according to a fact-ontology the same fact may still be associated with different descriptions, i.e. logically equivalent ones, it might be claimed that the same contradiction arises as to facts as arose for events. But the two cases are not parallel in this sense. The problem arose regarding events only because a set of explanatory premises implied one description of the event, Bk, but not another Hk, and hence both explained and did not explain k. In the case of facts, however, since the different descriptions associated with them are logically equivalent, whatever explanans implies one such description must also imply the other as well, and hence cannot both explain and not explain the same fact.

One might charge, nevertheless, that the successful avoidance of a contradiction such as that between (6) and (7) has been purchased at too high a price for anyone with a puritanical philosophical conscience, viz. at the price of an abstract intensional ontology.[55] The question thus arises as to whether or not we can consistently construe science as an abstractive and selective enterprise with scientific statements explaining things without being driven to presuppose the abstractness of these things. Scheffler's most illumined discussion of this question indicates that it is indeed possible to do so. His main strategy consists in rendering the entire analysis of scientific explanation explicitly concrete by assimilating event-explanation to the explanation of laws or generalizations in the sense of providing sentences as objects of explanation. That is, sentences are to be rendered, generally, as inscriptions or tokens (physical objects of certain shapes) instead of as abstract shapes or types. And explanations are to be expressed in relational manner, but now relating sentences to other sentences, not to events or facts. While fulfillment of the CL theory requires connecting two sorts of sentence-strings, i.e. explanatia and explananda, this nominalistic interpretation omits

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[55] Ibid., pp. 61-76.

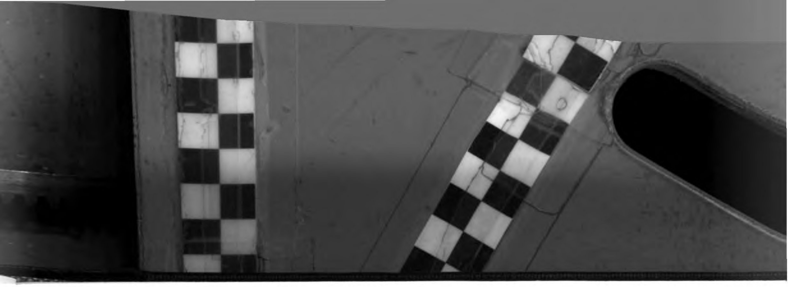
postulation of the intervening facts as objects of explanation and goes directly to the event-descriptions themselves. It also accounts for the importance of selectivity and abstractiveness, but without quantifying over, and hence without committment to, abstract entities.

The Covering Law Account as a Theory of Explanation

Now, for our purposes ther is no need to choose between these competing explications of what it is we explain scientifically, between Scheffler's inscriptional ontology and Hempel's intensional account. Of more importance is the nature of the explicative or constructional method itself.

So far in the process of analysing Hempel's version of the CL theory we have had brief occasion to defend his account against a common misunderstanding and ensuing objections. We will find need to extend this defense in later chapters when attention is turned to historical explanations. Our case was grounded, you will recall, on the interpretation of the CL theory as a philosophic explication or reconstruction of the important but inexact pre-analytic or ordinary notion of explanation, and of its systematic relation to such other notions as inference, empirical significance, laws and confirmation. Since much was and will be made to turn on this method, it might be helpful to offer some additional comments of clarification concerning both it and its relation to the value-neutrality thesis, upheld jointly by Popper and Hempel.

No doubt the main task of the CL theorists is to find an adequate definition of the concept of scientific explanation, one providing a basis for a theory of explanation. In this they follow the lead of Professor R. Carnap, who describes the philosophic task of explication as "making more exact a vague or not quite exact concept used in everyday life or in an earlier stage of scientific or logical development, or rather of replacing it by



a newly constructed, more exact, concept."[56] The defined or earlier term is referred to as the *explicandum*, and the term used for the proposed or defining concept as the *explicatum*. Some important cases where vague concepts have been explicated are Frege's theory of arithmetic based on the analysis of the number "two" as the class of all couples, Russell's analysis of definite descriptions as incomplete or syncategormatic expressions, Tarski's semantical version of "truth" and Carnap's proposal to analyze one sense of 'probability' along the lines of "degree of confirmation".[57]

Hence, the philosophic task is not merely to transcribe or duplicate the meaning of the explicandum, but to improve upon it by progressively "refining or supplementing its meaning,"[58] in Quine's phrase. Nor is it a case of finding a synonymous expression for the explicandum, or even of exposing hidden meanings. In fact, Carnap suggests that the explicatum need not correspond very closely to the meaning of the explicandum at all. The philosopher begins with some inadequately formulated concept, with a vague, ambiguous or incomplete explicandum which nevertheless serves some important functions. We have in ordinary or scientific discourse, in other words,

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[56] R. Carnap, *Meaning and Necessity* (Chicago: University of Chicago Press, 1947), pp. 7-8.

[57] Cf. C. Hempel's discussion in *Fundamentals of Concept Formation in Empirical Science* (Chicago: University of Chicago Press, 1952), Chapter I, especially p. 11.

[58] W. Quine, *From A Logical Point of View* (Cambridge, Mass.: Harvard University Press, 1953), p. 25.

> an expression or form of expression that is somehow troublesome. It behaves partly like a term but not enough so, or it is vague in ways that bother us, or it puts kinks in a theory or encourages one or another confusion. But it also serves certain purposes that are not to be abandoned. Then we find a way of accomplishing those same purposes through other channels, using other and less troublesome forms of expression. The old perplexities are resolved.[59]

It should be emphasized that "tightening up," i.e. making more exact and precise, the explicandum is not a sufficient condition of adequacy for the explicatum. Maximum precision is clearly not the only goal of an explication. But recognition of this fact has led some to suppose that the explicatum must also satisfy our intuitions in the matter. For, then, if a proposed analysis such as Hempel's violates "sound common usage" or is counter-intuitive, it is abandoned as inadequate. Even Hempel, you will recall, invoked such a criterion in originally discarding (R_4') or well-confirmedness for (R_4) or truth.

However, we are not told by those who invoke such a criterion what conforming to an intuition means, nor what constitutes a justifiable intuition, nor even why intuition must be a deciding factor at all. As a result, most constructionalists follow the view that the explicatum be usable in place of the original vague explicandum. As the above passage from Quine suggests, we fix on the purposes served by the unclear expression which make it worth bothering about, and then devise more efficient ways of achieving these same goals of functions. In place of a criterion of intuitiveness or maximum precision we have one of efficiency, or significant,

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59 W. Quine, Word and Object (N.Y.: Wiley and Sons, 1960) p. 260.

relevant or usable precision, based on the parallelism of function between explicandum and explicatum.

Yet it might be objected to such an enterprise, as many who invoke the criterion of counter-intuitiveness have done, that philosophic reconstructions such as Hempel's tend to neglect the immense richness and complexity of ordinary discourse and hence force it onto a Procrustean bed of "neat simplicities." But the other edge of this sword is, I think, even sharper. For, as Feigl and Maxwell have recently argued,[60] this immense richness of ordinary language often turns out to be an embarrassment of riches, and hence requires selection, abstraction and systematization for philosophic as well as scientific purposes. They suggest, in particular, three reasons for the need to reform or explicate ordinary language: in order to analyse at all, since most interesting terms of ordinary language are systematically ambiguous; to abstract our invariants of such usage and to systematize general principles in order to eliminate irrelevancies, and to arrive at viable approximations; and finally to correct the implicit rules of ordinary language which reflect false beliefs.[61]

So, while ordinary language is indeed the "first word" and the groundwork of philosophic inquiry, to which our explications must in some sense correspond, it remains only something to be respected not to be bound by. The main correspondence of explicatum

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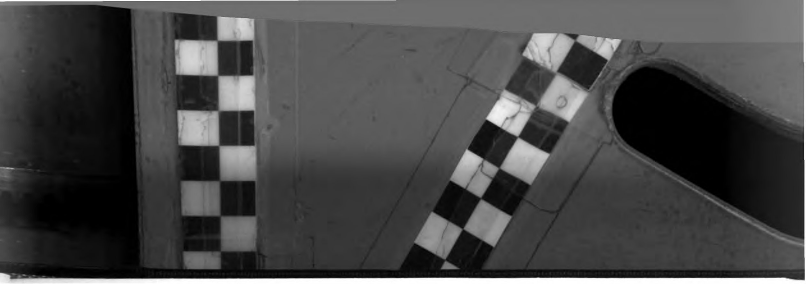
60 G. Maxwell and H. Feigl, "Why Ordinary Language Needs Reforming," The Journal of Philosophy, 58 (August, 1961), p. 492.

61 Ibid., p. 496.



to explicandum is one of function or purpose, not necessarily of synonymy, logical equivalence, extensional identity, nor even structural isomorphism. Even those who opt for the latter relationship recognize, with N. Goodman, that the opposition to constructionalism which greets, say, Hempel's proposed definition of a scientific explanation with the protest that explanation is "Not Merely" such but "Something More" fails to grasp what Hempel is doing. For in defining 'scientific explanation' along the lines of the CL theory, "he is not declaring that a so-and-so is nothing but a such-and-such," and the "' =df' in a constructional definition is not to be read 'is nothing more than' but rather in some such fashion as 'is here to be mapped as,'"[62] with the mapping to be appraised on grounds of efficiency in fulfilling specified purposes or goals.

Now, if our account of the explicative or constructional enterprise is adequate, two important consequences seem to follow, not both of which have been clearly recognized or acknowledged by constructionalist philosophers. First of all, since the adequacy of any explication depends on the efficiency with which the explicatum fulfills the purposes not so efficiently served by the explicandum and since the determination of purposes and efficiency of concepts is an empirical matter, it follows that the philosopher is not exempt from the controls of empirical science. He is not engaged in a totally different enterprise than the scientist, a

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[62] N. Goodman, "The Significance of Der Logische Aufbau Der Welt," in P. Schilpp (ed.), The Philosophy of Rudolph Carnap (La Salle, Illinois: Open Court, 1963), p. 554.

conceptual analysis of concepts, of eliciting meanings and establishing conventions, as Ayer e.g. has suggested.[63] Philosophic theses or definitions, in other words, are not merely analytic statements as opposed to the synthetic or empirical statements of the scientist. Though definitions and hence neither true nor false, they are subject to empirical criteria of adequacy connected with establishing purposes and efficiency.

In this respect it is puzzling to find Popper disagreeing with Carnap's views on explication on the grounds that one cannot "speak about exactness, except in a relative sense of exactness sufficient for a particular given purpose -- the purpose of solving a certain given problem."[64] For Carnap has consistently advocated just such a position. This is especially clear in his distinction between internal questions which occur within a linguistic framework and external questions about the acceptability of the framework or categorial principles governing it. While existential statements made within the system are analytic for Carnap, statements about the framework or system are construed pragmatically as practical matters requiring decisions. That is, to offer a philosophic explication is much like constructing a linguistic framework or theory, and the question of its adequacy is largely a decisional or pragmatic matter of how efficiently the explicatum resolves the problems for which it was constructed.

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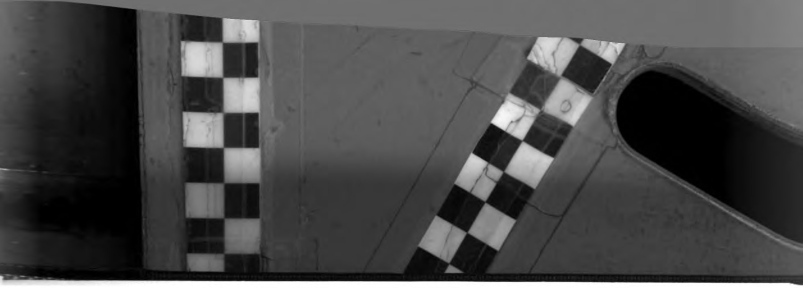
[63] A. J. Ayer, Language, Truth and Logic (N.Y.: Dover Publications, 1946), Chapter II.

[64] K. Popper, "The Demarcation Between Science and Metaphysics," in The Philosophy of Rudolph Carnap, p. 216, footnote.



Thus, though the decision to accept the explication or an appropriate framework is not purely a factual or theoretical matter, it will indeed "be influenced by theoretical knowledge."[65] Moreover,

> The acceptance or rejection of abstract linguistic forms, just as ... in any branch of science, will finally be decided by their efficiency as instruments, the ratio of the results achieved to the amount and complexity of the efforts required.[66]

Hence, Popper's disagreement with Carnap and Hempel on this point seems based on a misunderstanding of their position.

But once this agreement is seen, we are led to the second consequence of our account of constructionism, one that pertains indirectly to the value-neutrality thesis. Once it is conceded that our explications are to be appraised pragmatically, it becomes apparent that in most, if not all, contexts we will have a variety of purposes to be satisfied by any given explicatum, interpreted now as an instrument. This requires that some of the various purposes or goals be weighted as more important than others. Hence the decision to accept or reject a proposed explicatum will require the making of value judgments.

This consequence applies of course to the philosophic level of external questions about a linguistic framework. So far, at any rate, we have not argued for its application to scientific questions, assertions or explanatory hypotheses. But if such a case can be made, then the value-neutrality thesis of Weber, Popper

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65 R. Carnap, "Empiricism, Semantics and Ontology," added as a supplement to <u>Meaning and Necessity</u>, p. 208.

66 <u>Ibid</u>., p. 221.

and Hempel will be unacceptable. This, as mentioned earlier, will be the main burden of our final chapter. Our more immediate task, however, is to see how far the CL theory of explanation can be extended, to examine whether the Hempelian account can serve as an adequate explication of historical, as well as of scientific explanations.

CHAPTER III

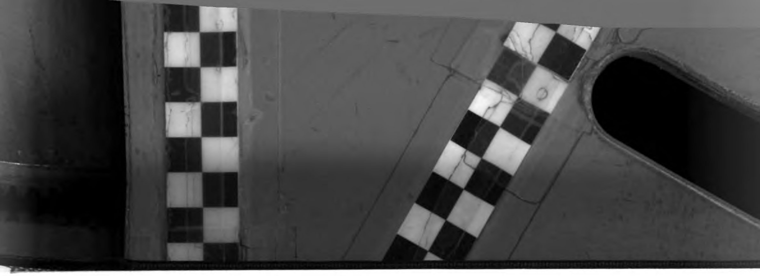
RECONSTRUCTIONS OF THE SU THESIS

Idealism and the SU Thesis

In the present chapter we will begin to consider how far the CL theory of explanation can be fruitfully extended to historical explanations. In particular, we examine two suggested reconstructions of the SU thesis. First, it will be shown that the idealist emphasis on the subjective element of intuition or an immediate grasp of historical reality is an inadequate reconstruction, one successfully warded off by CL theorists. Secondly, a more recent non-naturalistic reconstruction will be investigated and seen to offer important suggestions concerning both the VN and SU theses.

Various arguments have been offered to support SU, to show that any extension of the CL theory to historical inquiries is impossible since history is in some sense autonomous. And arguments drawn from the historian's special subject matter represent some of the most influential and suggestive of recent attempts to prove history autonomous. Our main concern in this and the remaining chapters will be limited, however, to a narrow range of cases, *viz.* to explanations historians give of purposive human actions considered important enough to be mentioned in historical narrative. Usually such explanations offer reasons why some individual person decided to act in a specified manner under given circumstances.

The peculiarities of such historical actions have led many recent critics of the CL theory to rehabilitate Weber's SU thesis in the form of a traditional doctrine of idealist philosophers of



history: that the objects of historical inquiry, being these human actions, differ fundamentally from those of the sciences. This difference is then parlayed into the charge "that the explanation of individual human behavior as it is usually given in history has features which make the CL model peculiarly inept."[1] For, even if the CL model applies to natural events, by explaining them as subsumption under empirical laws, it would still be inapplicable in history because of the latter's peculiar subject matter. In other words, this difference and peculiarity, stimulating much sympathy with the idealist position, is used to defend the distinction not merely between the different sources and kinds of empirical laws but between different <u>types</u> of explanation. Historical explanation, it is claimed, requires a different kind of understanding and has a different kind of "logic" than does scientific understanding. Hence, to accept the CL theory as applicable to history would be to conflate the distinction between explanation types.

Aristotle's comment in the <u>Poetics</u>, that poetry is of graver philosophic import than history, serves to introduce some of the issues between covering law theorists on the one hand and both past and recent critics on the other.

> The distinction between historian and poet...consists in this, that the one describes the thing that has been, and the other a kind of thing that might be. Hence poetry is something more philosophic and of graver import than history, since its statements are of the nature rather of universals, whereas those of history are singulars. By a universal statement I mean one as to what such and such

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[1] William Dray, <u>Laws and Explanation in History</u> (London: Oxford University Press, 1957), p. 118.

> a kind of man will probably or necessarily say or do..., by a singular statement, one as to what, say, Alcibiades did or had done to him.[2]

Aristotle's view--taken seriously by such later idealist philosophers as Windelband, Collingwood, Oakeshott, Dilthey and Croce, and by historians as diverse as Butterfield, Beard, Trevor-Roper and Trevelyan-- led to Windelband's widely accepted distinction between two different kinds of sciences. The <u>nomothetic</u> or generalizing natural sciences attempt to establish abstract general laws concerning pervasive, universal and indefinitely repeatable events. <u>Ideographic</u> or historical sciences, on the other hand, seek to understand what is special, singular, unique and nonrecurrent. Many covering law theorists concur in the marking of such a distinction. Popper, you will recall, distinguished between the theoretical and historical sciences on the ground that the former seek to establish general laws, while the latter assume these laws in order to establish warranted singular statements.

Idealists, however, use Windelband's distinction to support two further claims: that the logical structure of explanation differs essentially in the two kinds of sciences, and that historical explanations are *sui generis*. Consequently, they argue for a methodological disunity in the empirical sciences, since the historian, to explain his subject matter quite satisfactorily in his own way, need not appeal to general laws. A glimpse of the issue can be seen in Collingwood's declaration that history is not a

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[2] Aristotle, *The Basic Works of Aristotle*, Richard McKeon (ed.) (New York: Random House, 1941), 1451b 1-11.

spectacle. The scientist, subsuming events or actions under laws, patterns or regularities, remains essentially a spectator. The historian, on the other hand, adopts the standpoint of the agent, viewing events or actions from the "inside," not just externally or from the "outside." Hence he explains actions by appreciating the agent's problems, goals and beliefs, and by appraising the agent's responses to his problems. The central contention of The Idea of History is that history is an autonomous discipline with its own concepts and methods and with a unique kind of understanding. Understanding the thoughts of historical agents constitutes the primary task of the historian.

By the "outside" of an event Collingwood means everything belonging to it which is describable in mechanistic terms of bodies and motion. By the "inside" of the event he means "that in it which can only be described in terms of thought: 'Caesar's defiance of Republican law, or the clash of constitutional policy between himself and his assassins."[3] The historian, accordingly, investigates not mere events, having only an "outside," but actions, consisting of the unity of an event's "outside" and "inside." His main task is to think himself into the historical action, to discern the thought of the agent expressed in the event, and thus to achieve historical, as contrasted with scientific, understanding or intelligibility.

The scientist also goes beyond the events he encounters in

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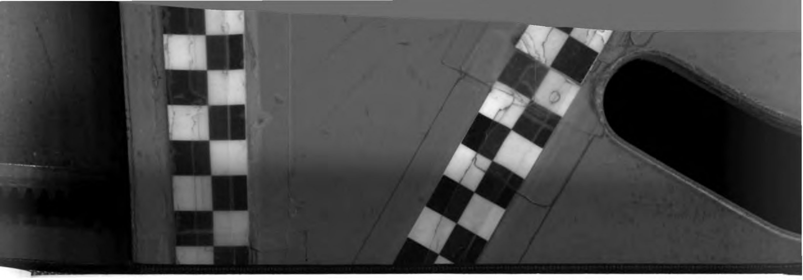
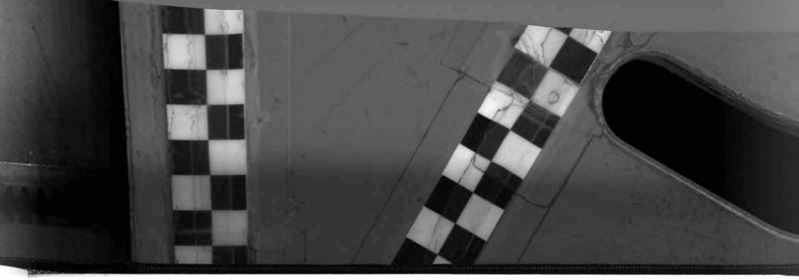
[3] R. G. Collingwood, The Idea of History (New York: Oxford University Press, 1946), p. 213.



inquiry, but only by relating events to others and thus subsuming them under general formulae or laws of nature. So Colingwood concedes the CL position when applied to science, since nature is always and merely a phenomenon or spectacle presented to the inquirer. In history, however, events are never mere phenomena, mere spectacles for contemplation. They are what the historian looks past or through to penetrate the thought or idea within; they are purposive calculated human actions. When one discovers the thought expressed in an event, one already understands the event. To know what happened is already to know why it happened. Such understanding results not from "merely" subsuming the event under laws, but is discerned instead by "re-thinking" the thoughts, by "re-enacting" the past, in one's own mind.

By denying that human actions consist only of physical movements from which we can infer the motive or reason behind them, Collingwood concludes that thoughts must be known directly or immediately by a special kind of non-discursive or intuitive knowledge. All history, consequently, is the history of thought, of the plan or idea of human actions. In another idiom, historians explain in the way that art, not science, explains: by illumination instead of deductive inference, by revealing the universal in the particular. Hence history cannot possibly be causal explanation or the science of human behavior.

In opposing these further claims, CL theorists also try to eliminate the deficiency which Aristotle attributes to history in contrast to poetry. The ideal model they establish for historical



explanations of individual actions requires that the functions of history and poetry, viewed as distinct by Aristotle, be united. The historian must not merely describe the particular, whether from "inside" or "outside." He must additionally reveal the universal which it embodies, but by subsuming it under general laws. Hence, in Aristotle's phrase, he raises history to the level of art and knowledge.[4] Accordingly, the CL position, along with that of their idealist critics, implies the denial of Aristotle's dictum that history has less philosophic import than poetry. The issue between them turns, instead, on how the universal is revealed in the particular: by subsumption under general laws or by an imaginative, intuitive "re-interpretation" of the total context of the event.

Further clarification of the issue arises from posing for the Idealist the question, "What is the explanatory force, nature or logical structure of these allegedly distinctive historical explanations?" In considering this question we hope to elicit and develop the more recent criticisms of the CL theory of explanation. But since the import of this criticism is best exhibited as an outgrowth of the earlier dispute, we will first examine briefly the idealists' reconstruction of the SU thesis. Then, having considered the "official" CL rejoinder to it, we shall note how some recent philosophers rehabilitate and revive the older position.

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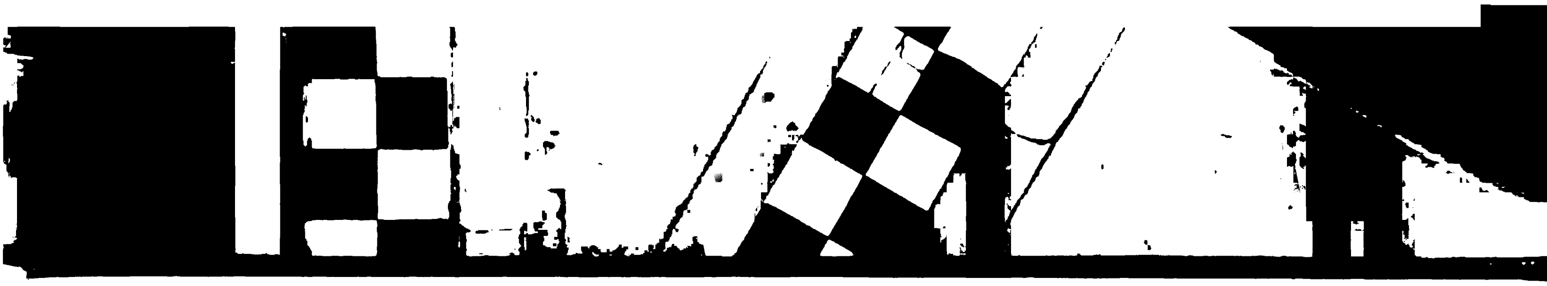
[4] E. Barker, "Rational Explanations in History," in S. Hook (ed.), Philosophy and History (New York: New York University Press, 1963), p. 179.

Perhaps the main claim of the earlier idealist philosophers of history, already noted, is that the subject matter of historical inquiry differs fundamentally from that of the natural sciences, since concerning the thought and actions of humans. As a result, explanation by subsumption under empirical laws is considered singularly inappropriate in history. For even if individual human actions could be subsumed under law, this would not constitute understanding of these actions in a sense proper to the special subject matter. The additional intuitive factor required to achieve historical explanation proper, "empathetic understanding" or Verstehen, is usually contrasted with the allegedly superficial knowledge gained through tests and statistics. With men considered the ultimate unit of historical and social life, and the mind of man construed as a given immediate reality, understanding of the social world is founded on one's personal experience. One understands the experiences of other persons, especially in an historical context, only through "re-experiencing" or "re-living" these experiences.

Butterfield, an historian exhibiting the influence of the idealists, summarizes much of their case in the following passage:

> Our traditional historical writing... has refused to be satisfied with any merely causal or stand-offish attitude towards the personalities of the past. It does not treat them as mere things, or just measure such features of them as the scientist might measure; and it does not content itself with merely reporting about them in the way an external observer would do. It insists that the story cannot be told correctly unless we see the personalities from the inside, feeling with them as an actor might feel the part he is playing-- thinking their thoughts over again and sitting



> in the position not of the observer but of the doer of the action.... the historian must put himself in the place of the historical personage, must feel his predicament, must think as though he were that man.... Traditional historical writing emphasizes the importance of sympathetic imagination for the purpose of getting inside human beings. We may even say that this is part of the science of history for it produces communicable results-- the insight of one historian may be ratified by scholars in general, who then give currency to the interpretation that is produced....[5]

But Max Weber offers perhaps the most influential account of this procedure in his postulate of subjective interpretation, which stresses the primacy of consciousness and subjective meaning in interpretations of social actions. The historian's primary task, claims Weber, is to attempt

> the interpretative understanding of social action in order thereby to arrive at a causal explanation of its course and effects. In 'action' is included all human behavior when and in so far as the acting individual attaches a subjective meaning to it.[6]

Weber introduces the notion of "ideal types" as a device to explain concrete historical phenomena, such as the development of modern capitatism, in their uniqueness. Such understanding, concerning the individuality of a phenomenon, "is not a question of *laws* but of concrete causal *relationships*. It is not a question of the subsumption of the event under some general rubric as a representative case but of its imputation as a consequence of some

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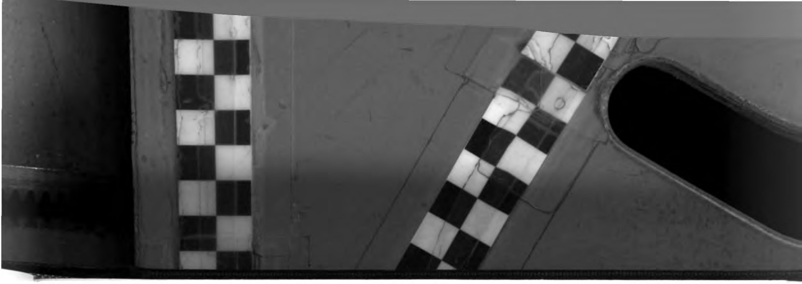
[5] H. Butterfield, *History and Human Relations* (London: Oxford University Press, 1951). pp. 145-6.

[6] M. Weber, *Theory of Social and Economic Organization* (New York: Oxford University Press, 1947), p. 88.



constellation."[7] To afford acceptable historical explanations, such causal relationships must be meaningful as well as "causally adequate" and "objectively possible." They must, in other words, be based upon those aspects of human behavior containing cultural significance, valuation or other motivating factors. These causal connections are expressed in terms of principles, classified as "general empirical rules," which convey knowledge derived from our personal experience. Weber also introduces, as a means of discovering such meaningful explanatory principles, the method of *Verstehen* or empathic understanding.

The distinctive aim of the historian and social scientist thus appears as "understanding" social phenomena by using "meaningful" categories and imputing "subjective" states to human agents participating in social processes. This requires understanding the meaning an act has for the actor himself, not for the external observer of his actions, *i.e.* *Verstehen*. One seeks in such understanding not a set of universal laws but the total intentional framework of the actor which clarifies the meaning of his specific act. Instead of subsuming the specific act under some set of covering laws, one refers it back to its intentional matrix which, as the ground of its meaning, helps to interpret it.

> Thus for a science which is concerned with the subjective meaning of action, explanation requires a grasp of the complex of meaning in which an actual course of

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[7] M. Weber, *Methodology of the Social Sciences* (Glencoe: Free Press, 1949), p. 78.

> understandable action thus interpreted belongs. In all such cases, even where the processes are largely affectual, the subjective meaning of the action, including that also of the relevant meaning complexes, will be called the 'intended' meaning.[8]

Weber thus resorts to two separate spheres of scientific cognition: we explain natural events and we understand human actions. We approach the former from without and the latter from within. Only as humans are we in a position to comprehend the subjective meaning an actor attaches to or intends by his action, and thus to formulate the general principles for understanding human actions. For example, the conduct of a man about to be cut by a knife will surely be different depending on whether the knife-wielder intends a surgical incision or a mutilation.

However, idealists modify SU so that interpretative understanding seeks the meaning of action in empathic intuition of a whole, of the realm of subjectivity which cannot be conceptualized but requires to be re-experienced or reproduced as a whole. "Conception is reasoning; understanding is beholding."[9] Unlike Weber, they take historical thinking to be intuitive, not discursive. Howard Becker interprets Weber's SU thesis in the following way.

> Here, reduced to its barest, most obvious terms, is what is meant by interpretation, no more and no less. The interpreter puts himself in the place of the actor as best he can, and the degree to which he views the situation as the actor views it determines his success in predicting the further stages of the conduct.[10]

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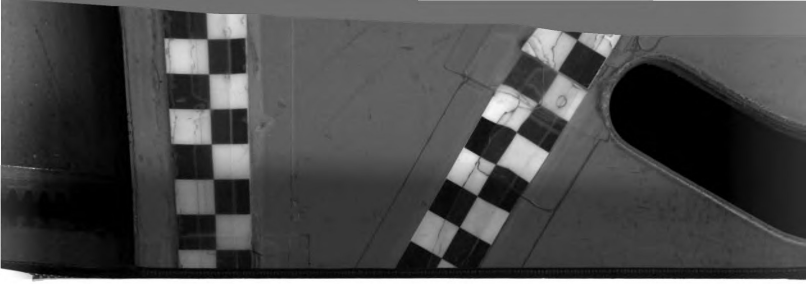
[8] Weber, *Theory of Social and Economic Organization*, p. 95.

[9] L. von Mises, *Epistemological Problems of Economics* (New York: Van Nostrand, 1960), p. 134.

[10] H. Becker, *Through Values to Social Interpretation* (North Carolina: Duke University Press, 1950), p. 191.



And H. Cooley, the American sociologist, concurs. We understand human behavior by sharing their "state of mind" or intended meanings as a special kind of knowledge distinct from statistical knowledge which, without Verstehen, is superficial and unintelligent.

The main point of the method of Verstehen common to the views of Weber and idealists, then, is that human actions are informed by meanings and purposes in a way in which only meaningful behavior can be. Hence, to treat them as mere physical events in a causal nexus would prevent the observer from apprehending the action as historical or social, irreducible to non-social elements. More is involved than just the individual action in a behavioristic sense. One must also be familiar with the total social context of the action in order for it to be intelligible or understood, and no amount of general laws produces this understanding. At best they produce a causal explanation, but not understanding. But while Weber nowhere indicates unambiguously just what kind of knowledge empathic understanding provides the idealists clearly opt for a non-discursive form of knowledge and hence sharpen the distinction between the nomothetic and ideographic sciences.

The Standard Covering Law Answer

Unfortunately, however, neither Butterfield's summary nor the varied accounts of philosophers, social scientists or historians, including Weber, succeed in clarifying either the nature of this empathic method of understanding, the logical structure of its corresponding kind of explanation, or what is peculiar about it. We are given no clear or unambiguous analysis of what the method of Verstehen amounts to in practice, nor what import to attach to the results of the method. Consequently, Theodore Abel, in a definitive essay drawing heavily upon Weber's own position as well as Hempel's essays, set himself the task of illustrating, analysing and evaluating "The Operation Called 'Verstehen'." His analysis reveals

> two particulars which are characteristic of the act of Verstehen. One is the 'internalizing' of observed factors in a given situation [the stimulus and the response]; the other is the application of a behavior maxim which makes the connection between these factors relevant. Thus we 'understand' a given human action if we can apply to it a generalization based upon personal experience. We can apply such a rule of behavior if we are able to 'internalize' the facts of the situation.[11]

Abel illustrates the act of Verstehen as an explanatory tool in the following way. Although competent statistical research established a high correlation between the annual rate of crop production and the rate of marriage among farmers in a given year, we often feel we can forego statistical tests of such correlations

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[11] T. Abel, "The Operation Called 'Verstehen'," in E. H. Madden (ed.), Structure of Scientific Thought (Boston: Houghton Mifflin, 1960), p. 161.

because we "see" from our own experience of motivational patterns the connection as "relevant". We "understand " why the rate of marriage in farming areas follows the rate of crop production. Our information is that failure of crops (C) materially lowers farmers' income (C'), and that one makes new commitments (E') when one marries (E). Then we internalize (C) into a "feeling of anxiety" (Vc), and (E) into "fear of new commitments" (Ve) because of our own past personal experience. This gives us direct knowledge or understanding of the "obviously" relevant connection or behavior maxim, "People who experience anxiety will fear new commitments" (Vc-Ve). The operation of Verstehen is thus based, according to Abel, on the application of personal experience to observed behavior. The explanation thus takes the following form:

(a) The failure of crops (C) produces feeling of anxiety and insecurity (Vc).

(b) We understand from our own personal experience that (Vc) leads us to fear new commitments (Ve).

(c) (Ve) leads to low marriage rates (E).

(d) Therefore, we can understand why (E) occurs under conditions (C).

Another classic example of "meaningful" explanation of a social phenomena is Max Weber's account of modern capitalism as due in part to the ascetic forms of "The Protestant Ethic." Here the former (E) occurs under the complex conditions of the latter (C). But individuals participating in (E) are assumed to be subjectively committed to certain values (Ve), and those participating in (C) to be committed to other values (Vc). And since

(Vc) and (Ve) are "meaningfully" related, due to the motivational patterns we personally experience by Verstehen, we thus "understand" why (E) occurs under conditions (C). The form of the explanatory argument follows:

(a) Calvinistic Protestantism (C) developed individuals with subjective states, values or an attitude towards life called "Protestant Asceticism" (Vc).

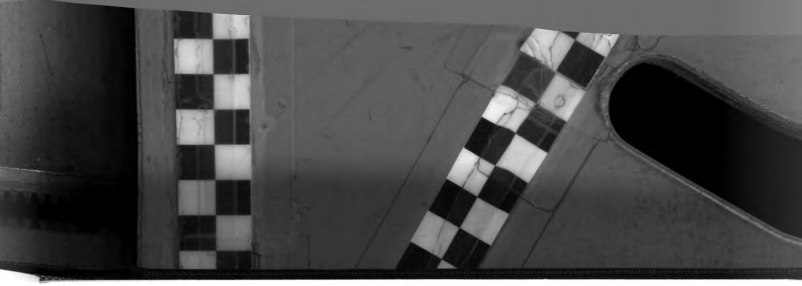
(b) We understand from our own personal experience of motivational patterns that (Vc) is "meaningfully" related to and leads to the attitude towards life called "The Spirit of Capitalism" (Ve).

(c) Individuals with (Ve) were mainly responsible for the development of modern capitalism (E).

(d) Therefore, we can understand why (E) developed under conditions (C)

The main difference between such explanations and a CL one, then, seems to be that the antecedent conditions are connected with the explanandum-event not by a general empirical law but rather by a statement of what the historical agent valued or believed just prior to his action. Such a statement of what was meaningful, significant or intended by the agent, the idealists claim, can only be known by imaginatively constructing or empathizing with his situation.

However, from Abel's analysis of Verstehen, we see clearly the gross limitations of such an operation. At best it suggests plausible or possible explanations or hypotheses; it is a source of "hunches" or discovery. And Abel, Hempel, Popper and Nagel quickly offer the "official" CL answer to those advocating Verstehen as a peculiar kind of historical understanding. In



Hempel's terms:

> This method of empathy is, no doubt, frequently applied by laymen and by experts in history. But it does not in itself constitute an explanation; it rather is essentially a heuristic device; its function is to suggest certain psychological hypotheses which might serve as explanatory principles in the case under consideration...; but its use does not guarantee the soundness of the historical explanation to which it leads. The latter rather depends upon the factual correctness of the empirical generalizations which the method of understanding may have suggested.
>
> Nor is the use of this method indispensable for historical explanation. A historian may, for example, be incapable of feeling himself into the role of a paranoiac historic personality, and yet be able to explain certain of his actions;notably by reference to the principles of abnormal psychology. Thus whether the historian is or is not in a position to identify himself with his historical hero, is irrelevant for the correctness of his explanation; what counts, is the soundness of the general hypotheses involved, no matter whether they were suggested by empathy, or by a strictly behavioristic procedure.[12]

So, while <u>Verstehen</u> may serve an important heuristic role of suggesting or discovering explanatory hypotheses, it constitutes neither a necessary nor a sufficient condition for justifying or confirming them. Hence, it is not considered a serious competitor or alternative to the CL model of explanation.

In fact, even Max Weber, an advocate of non-intuitionist <u>Verstehen</u>, stresses the need to support any given interpretation of subjective states with adequate observational verification. Otherwise:

> there is available only the dangerous and uncertain procedure of the 'imaginary experiment' which consists in

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[12] C. G. Hempel, "General Laws in History," p. 467.

> thinking away certain elements of a chain of motivation and working out the course of action which would then probably ensue, thus arriving at a causal judgment.[13]

In "The Logic of the Cultural Sciences" he particularly warns against confusing the "psychological course of the origin of scientific knowledge and 'artistic' form of representing what is known...with the *logical structure* of knowledge."[14] He stresses no less, in his "judgments of objective possibility," the importance of counter-factual conditionals to historical inquiry in general and to historical explanatory laws specifically.

Popper further supports the CL answer by denying the uniqueness of the method of "intuitive understanding" to the social sciences or history. Even the physicist, though not helped by such direct observation, "often uses some kind of sympathetic imagination or intuition which may easily make him feel that he is intimately acquainted with even the 'inside of the atoms' -- with even their whims and prejudices."[15] However, Popper also indicates our more direct knowledge of human actions than of physical events. His major point, nevertheless, agrees with that of Hempel and Abel: any hypothesis resulting from intuitive understanding must be empirically testable to qualify as a genuine explanation. In other words, testability or confirmability (R_3) is a necessary

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[13] M. Weber, *Theory of Social and Economic Organization*, p. 97.

[14] M. Weber, *Methodology of the Social Sciences*, p. 176.

[15] K. Popper, *Poverty of Historicism*, p. 138.

condition of an adequate explanation. And the method of Verstehen fails to provide an adequate method for testing or corroborating hypotheses. It is at best a method for discovering possible explanations, which must then be subjected to appropriate objective testing procedures.

Nagel sums up this "official" CL or naturalist answer with three basic countercharges. He argues that there is not a different kind of knowledge involved in understanding social phenomena, that the method of Verstehen as an empathic response to or imaginative reconstruction of another person's motivation involves a fundamental subjectivism which renders it at best nonscientific, and finally that the method offers, on its own, no criteria for testing scientific hypotheses regarding human actions.

Some of the applied impact of this "official" answer can be gleaned from a recent statement of a practicing sociologist, Professor D. Martindale. Speaking of the idealist use of ideal types, of configurations of "meaningfulness" guiding the "re-living" of historical experience, Martindale claims that:

> such formulations have lost their interest for modern students. Even the most tender minded of contemporary students is inclined to see science as all of one piece. The insights produced by intuition, empathy, or some method of verstehen are to the modern student mere untested hypotheses. The funeral oration of the verstehen point of view was gracefully and ceremoniously performed by Abel in his "Operation Called Verstehen."[16]

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[16] D. Martindale, "Sociological Theory and the Ideal Type," in L. Gross (ed.), Symposium on Sociological Thought (New York: Columbia University Press, 1944), p. 82.

Thus, the "official" CL answer is often said to dismiss Verstehen as some sort of methodological dodge, as an inferior way of obtaining the same kind of explanation as can be obtained more reliably by direct subsumption of human actions under empirically testable and confirmed covering laws.

Recent Replies

This "official" answer, nevertheless, fails to convince more recent non-naturalist observers of the issue. There is general agreement, I think, that this neo-Weberian CL answer exposes much mystery-mongering in the idealist position by emphasizing their cloudy mixture of psychological and methodological elements. Still, many feel that the CL argument does not cut as deeply as Abel, Hempel and Nagel assume. They believe that the CL theorists overlook the important features about explanations of human actions in history, features stressed by Weber and the idealists. In other words, more recent philosophers feel that the idealists were after something significant, but couched it in misleading and vague terminology. As a result, they recognize the value of the CL answer as needed clarification, but then use this clarification to reconstruct the important residue of the Weberian position. This residue consists largely of the allegedly non-experimental elements in common sense, scientific and philosophic inquiry. With the general conclusion that the CL answer overlooks a significant element of the idealist case, the present author concurs. But, as I hope to show, the development of this element has not been convincing. It requires not affirming SU but denying VN.

Such non-experimental elements often produce the anxiety of doubt in even the most thorough-going naturalists, who usually identify all knowledge with value-neutral scientific knowledge and opt for the universal applicability of the experimental method. J. H. Randall, for example, expresses just this anxiety in summing up a volume of essays by naturalists:

> The idealists may have lacked scientific knowledge and techniques. But it is often hard not to feel that they have possessed most of the human wisdom...have the edge on insights, on the discrimination of values, on the appreciation of the richness and variety of the factors demanding organization.... Naturalistic philosophizing must become as rich as the idealistic philosophies by incorporating the facts and experiences they emphasized within its own more adequate framework....[17]

Thelma Lavine, enlarging upon this anxiety of occasional naturalistic doubt, attempts to reconstruct the method of Verstehen along naturalistic lines, in order to include these important residual non-experimental elements in a wider naturalistic framework. "For naturalists do not so much seek to deny the fact of the various nonexperimental elements in inquiry as they fear the uncontrolled vagaries which are apt to result from acknowledging them."[18] The main problem of such a reconstruction, however, consists in properly locating such naturalistic safeguards or controls for Verstehen. Miss Lavine suggests only a modification of the naturalistic emphasis on "a single intellectual method." Instead she stresses "a single intellectual criterion for whatever method may be feasible," viz., the criterion of pertinent empirical checks or testability.[19]

But she nowhere makes clear just what the method of Verstehen contributes to the CL model of explanation or to the scientific

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17 J. H. Randall, "Nature of Naturalism," in Y. Krikorian, Naturalism and the Human Spirit (New York: Columbia University Press, 1944), pp. 375-6.

18 T. Lavine, "Note to Naturalists on the Human Spirit," in Natanson (ed.), Philosophy of the Social Sciences (New York: Random House, 1963), pp. 258-9.

19 Ibid., p. 259.

method of naturalism generally. Nor is it clear from her account what the method is or how far its scope extends. In fact, Nagel, in a direct response to her proposed reconstruction, resorts to his earlier distinction between the logic of discovery and the logic of validation in repeating the charge that even in its reconstructed form the method at best serves to generate suggestive hypotheses but does not suffice to verify or validate any. He thus remains constant in interpreting "subjective" in SU along the lines of private, personal and unverifiable judgment.

In response to Nagel's criticism, Miss Lavine makes one telling point of major significance for our purposes. Yet it becomes progressively confused when elaborated and hence is never successfully developed. Arguing that the scientific method of naturalism does not suffice as a general philosophic method, she contends that

> Undeniably, the principle of continuity of analysis does not bar the 'acceptance' of scientific conclusions. But the point I am making lies precisely here: what is entailed in the concept of 'acceptance of scientific conclusions'? Further, what is the relationship between acceptance of scientific conclusions and the philosophy of naturalism? .... Surely in the most common usage of the term 'acceptance,' acceptance of scientific conclusions does not by itself entail any philosophical operations whatsoever and is unworthy of being designated as naturalistic.[20]

So, though granting Nagel's claim that contemporary naturalists neither identify scientific method with overt experimental activity nor fail to recognize the importance of "non-experimental" elements, she argues that these elements remain residual and hence theoretically

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20 Ibid., p. 267.

unexplored. Surely she is correct, at least in regard to the notion of "acceptance" of scientific hypotheses. Since I take this claim to be of primary importance, as the significant though confused insight of *Verstehen* theorists, it will be treated in some detail later. Let me comment here merely that some such naturalistic reconstruction of *Verstehen* is necessary to resolve the main issues debated by CL theorists and their recent ordinary-language opponents. Consequently, Miss Lavine's position deserves much more attention than Nagel's rather conventional reply.

Having brought forth the important and difficult pragmatic concept of "acceptance" in this context, Miss Lavine proceeds to confuse the issue at hand by using it to defend the method of *Verstehen* as "the sole method of philosophy."[21] The confusion involved in her naturalistic reconstruction of *Verstehen* is well marked by Natanson in a recent article in which he propounds an alternative non-naturalistic or phenomenological reconstruction. He complains that to provide "naturalistic safeguards" for *Verstehen*, after placing the philosophic status of these very safeguards in question, is inconsistent and follows a step forward with a step back.[22] This seems indeed to be the case. But instead of pursuing the purportedly non-experimental concept of "accepting" scientific hypotheses, Natanson also moves in another direction.

We will pursue this difficult and central concept further in

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[21] *Ibid.*, p. 260.

[22] M. Natanson, "A Study in Philosophy and the Social Sciences," in *ibid.*, p. 282.

Chapter 5. For the moment let us follow Natanson's argument. To remove the inconsistency, he denies the need to reinvoke naturalistic criteria as correctives for the method of *Verstehen*. Unlike Nagel, he complains not about making *Verstehen* the essence of philosophical method, but only about her reverting to a "notion of *Verstehen* in the narrow sense of method as a conceptual device." This he contrasts with the "broad sense" which "cannot be incorporated into naturalistic methodology, because it is itself foundational." Hence, Natanson's "way out" of the inconsistency is "the transcension of naturalism in favor of a phenomenological standpoint... which takes human consciousness and its intended meanings as the proper locus for the understanding of social action."[23]

Likewise, Natanson's reply to Nagel's objection, that *Verstehen* alone fails to provide any criteria for the validity of hypotheses about human actions, turns on the interpretation of *Verstehen* along quite different, more philosophic, lines. He denies that it was ever intended for such a purpose. Hence Nagel's objection, he claims, is simply misdirected. *Verstehen*, concerning only the "conceptual framework within which social reality may be comprehended",[24] is not intended to provide empirical criteria for determining the validity of hypotheses.

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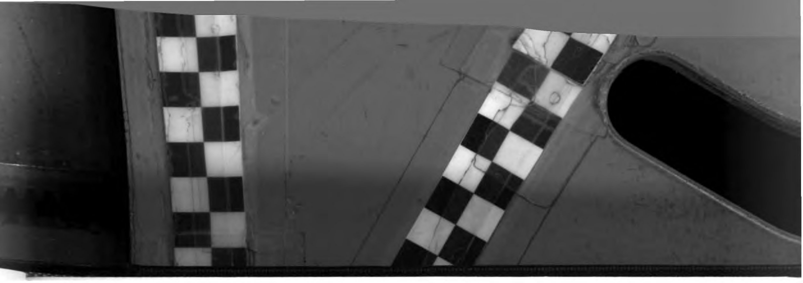
[23] *Ibid.*, pp. 282-3.

[24] *Ibid.*, p. 281.

A. Schutz' Reconstruction of SU

Now, Natanson's identification of Verstehen with the "self-founding," "presuppositionless" conceptual framework of phenomonology derives from another non-naturalist attempt to reconstruct the important residue of the idealist position, that of Alfred Schutz. Schutz' position bears investigation for a two-fold reason. On the one hand, along with Miss Lavine and Natanson, he finds the "official" CL answer to the idealists based on a gross misunderstanding of Weber's postulate of subjective interpretation and of the SU thesis. His analysis, accordingly, serves to clarify further the misunderstanding and, at the same time, the nature of Verstehen. In fact, his reconstruction of Verstehen constitutes, in my opinion, one of the most complete and cogent accounts available; though it is not totally acceptable. On the other hand, his analysis also points directly to problems with the extension of "testability" to apply to the purportedly non-experimental notions of goals, purposes and values. These are notions alluded to in the discussion of Lavine's criticism of Nagel, notions to be developed later in connection with recent analytic-pragmatic criticisms of the CL theory of explanation. But while recognizing the importance of this point, he also fails to develop it as a serious challenge to the CL theory. In particular he fails to see how this tact requires denying VN.

Schutz' defense of the non-naturalist Verstehen position is still particularly penetrating and enlightening. He disavows most of the previous vague and obtuse statements of the position in order to elicit the clear and important parts of it. Not what Weber or



the idealists said but what they meant, or perhaps should have said, is his, as well as our, concern. He concedes at the start, for instance, that most "subjectivists" or non-naturalists had an erroneous view of the methods of natural science, usually depicting it in a most narrow and restricted manner. They were inclined to generalize from the methodological situation in one particular domain, say history, to the situation of the social sciences generally. Instead, Schutz clearly opts for a unity of rules of scientific procedure, rules valid for all empirical sciences. In particular, he shares Weber's fear of private, uncontrolled intuitions. The issue as he views it is not whether all empirical knowledge involves controlled inference, statability in propositional form, or observational verifiability. Nor does it concern the notion of "theory", used to explain empirically ascertainable regularities, as applicable to history and the social sciences generally. On all of these points Schutz readily agrees with Nagel. Moreover,

> ...a method which would require that the individual scientific observer identify himself with the social agent observed in order to understand the motives of the latter, or a method which would refer the selection of the facts observed and their interpretation to the private value system of the particular observer, would merely lead to an uncontrollable private and subjective image in the mind of this particular student of human affairs, but never to a scientific theory.[25]

As a result of these disavowals, the important questions at issue are how to grasp subjective meanings scientifically, and how

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[25] A. Schutz, "Concept and Theory Formation in the Social Sciences," in ibid., p. 235.

to develop methodological devices for obtaining objective and verifiable explanations of a subjective meaning structure. For example, Schutz objects to the identification of experience with sensory observation and to the view of subjective uncontrollable and unverifiable introspection as the only alternative to controllable objective sensory observation. Unlike the idealists, he does not interpret Verstehen as providing a different kind of knowledge than the natural sciences, nor as an unscientific empathic response to or imaginative reconstruction of another person's motivation in social action. Consequently, Schutz takes Nagel to be whipping a straw man, because of his failure to understand Max Weber's SU thesis and his postulate of subjective interpretation. This seems dubious. But of more interest for our purposes is his reconstruction of Weber's postulate and of the Verstehen position generally, to which we now turn.

Schutz' explication of Verstehen occurs in the context of defending three propositions:

A) That the "primary goal of the social sciences is to obtain organized knowledge of social reality," i.e. of "the sum total of objects and occurrences within the social cultural world as experienced by the common sense thinking of men."[26] It is not a private but an intersubjective world common to us all.

B) That identifying experience with sensory observation and the experience of overt action "excludes several dimensions of

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[26] Ibid., p. 236.

social reality from all possible inquiry,"[27] i.e. such non-experimental elements as the observing scientist, the meanings of actions to performers, "negative actions" and beliefs.

C) That "all forms of naturalism and logical empiricism simply take for granted this social reality.... and assume, as it were, that the social scientist has already solved his fundamental problem before scientific inquiry starts."[28] This point deserves special attention, for Schutz here, and elsewhere, elaborates on a point stressed earlier by Miss Lavine, viz. the residual and unexplored element of "acceptance" of scientific hypotheses.

Schutz' criticizes a naturalistically oriented social science, one explaining human behavior in terms of controllable sensory observation, since it

> stops short before the description and explanation of the process by which scientist B controls and verifies the observational findings of scientist A and the conclusions drawn by him. In order to do so, B has to know what A has observed, what the goal of his inquiry is, why he thought the observed fact worthy of being observed, i.e. relevant to the specific problem at hand, etc. This knowledge is commonly called understanding. The explanation of how such a mutual understanding of human beings might occur is apparently left to the social scientist.... This means ... that so-called protocol propositions about the physical world are of an entirely different kind than protocol propositions about the psycho-physical world.[29]

All of these dimensions of social reality, he contends, require not just the CL model of explanation but Verstehen as a

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[27] Ibid., p. 237.

[28] Ibid., p. 236.

[29] Ibid., pp. 236-7.

process, technique or method of understanding. But while both critics and defenders of the process of Verstehen agree that it is "subjective," they unfortunately use this term in different senses. CL theorists suggest that subjective understanding of human motives depends on the private, uncontrollable, unverifiable intuitions of the observer. Social scientists such as Weber mean by "subjective" that the goal of Verstehen is to uncover the actor's intended meaning in his action, to determine what the agent meant instead of what the act "means" for an observer. Schutz accordingly sets himself the task of clarifying the meaning of Verstehen. For, the failure to distinguish clearly between the various levels of Verstehen causes confusions in the CL answer to Weber's postulate of subjective interpretation.

The three different levels of application of Verstehen, according to Schutz, are:

(1) "the experiential form of common-sense knowledge of human affairs" whereby men in daily life do understand and interpret each other's actions by grasping the meanings, motives, attitudes and purposes intended by others. This he takes to be the primary meaning of Verstehen.

(2) "an epistemological problem" of intersubjectivity, of how such understanding or Verstehen is possible, a problem that points to a clear distinction between the objects of knowledge of the natural and social sciences, but also one taken for granted in our common-sense thinking. In this sense Verstehen is a metaphilosophical or categorial analysis of philosophical procedures, and

a conceptual framework for comprehending social reality.

(3) "a method peculiar to the social sciences"[30] whereby the concern is with second-order constructs or typifications of interpretation found in common-sense. Verstehen in this sense involves a theoretical system suitable for the clarification of the interpretative understanding of the ordinary man in daily life.

Let us look at these three levels of Verstehen in more detail. Let us consider them, however, not merely as a reconstruction of Max Weber's postulate of subjective interpretation, but mainly as possible support for SU or as possible objections to the CL model of explanation and a naturalistic attempt to account for historical human actions.

Following the neo-Kantianism of Weber and Georg Simmel, Schutz stresses the importance of theory-laden facts. All facts are interpreted facts. They are always selected by an activity of mind in accord with our purposes and interests. Hence there are, strictly speaking, no pure, simple or given facts. All knowledge involves a set of constructs, abstractions, generalizations or idealizations. But as Weber, Popper and Hempel point out, this does not imply our inability to grasp the reality of the world, those relevant to our purposes and interests.

Nevertheless, the thesis of "theory-laden facts" indicates a crucial difference between the constructs used by natural scientists

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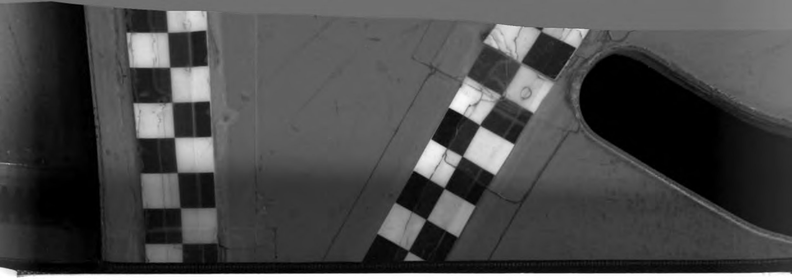
30 Ibid., p. 240.



and those used by historians (or social scientists generally). Since relevance is not inherent in nature itself but results from the interpretative and selective activity of man observing nature, the data or events explained by natural scientists are merely those within his observational field. They do not "mean" anything to the atoms or molecules. They have no meaning or unity, in Simmel's phrase, prior to that given them by the inquiring scientist. "The unity of nature emerges in the observing subject exclusively."[31]

The subject matter for the historian or social sciantist, on the other hand, consists of events and data of quite a different sort. The social world is not essentially structureless or "meaningless" prior to the inquirer's observations. It already contains a unity, meaning and relevance for the human beings acting and thinking therein. For they have already preselected and preinterpreted this world by their common-sense constructs and idealizations. These very constructs or thought objects help them "get on" in their environment by determining their behavior, goals and means. As a result, the constructs used by the historian and social scientist are not first-order constructs about uninterpreted data, as are the natural scientist's, but are instead second-order constructs. They are "constructs of the second degree, viz. constructs of the constructs made by the actors on the social scene, whose behavior the scientist observes and tries to explain..."[32]

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[31] G. Simmell, "How Is Society Possible?", in ibid., p. 74.

[32] A. Schutz, "Common-Sense and Scientific Interpretation of Human Action," in ibid., p. 305.

But in order to explain social reality---in the sense of human conduct and its common-sense interpretations and systems of projects, motives, relevances and constructs---the historian's second-order constructs must include reference to the subjective meaning which actions have for the actors, to the purposive behavior of historical actors. This is what Max Weber intended, Schutz claims, by his postulate of subjective interpretation, which must be

> understood in the sense that all scientific explanations of the social world *can*, and for some purposes *must*, refer to the subjective meaning of the actions of human beings from which social reality originates.[33]

Now, it is not clear how far Schutz wants to extend this generally cogent construal of Weber's postulate. But if he intends it to exhaust all social inquiry, he limits such inquiry arbitrarily and unnecessarily. For all explanations of historical developments of social institutions, to take only one example, would thereby be excluded. Such explanations cannot be made from the standpoint of an historical actor, from the standpoint of subjectivity, but must be made from the viewpoint of the historical observer. For our purposes, however, we need not take Schutz' thesis as extending beyond clear cases of purposive human behavior which are informed by meanings and which become intelligible to an observer when he understands the presuppositions of social action in the subject's community.

Even with this limitation of scope, Weber's postulate still leaves us with the central question of whether or not it establishes the SU thesis and hence refutes the claims of the CL theory. I think

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[33] A. Schutz, "Concept and Theory Formation in the Social Sciences", p. 245.

it does not. But to show why requires asking how the scientist's second-order constructs, ideals or theories are related to the first-order constructs of common-sense; and how it is possible to grasp by a system of objective and verifiable scientific knowledge the subjective meaning structures of human behavior. This was the very question at issue in the dispute between Nagel, Miss Lavine and Natanson, the question concerning methodological devices for attaining objective and verifiable knowledge of subjective meaning structures, the question of establishing controls for *Verstehen*. Schutz' answer follows from his distinction between the two levels of *Verstehen* or interpretative understanding, and between the first-order constructs of common-sense and the historical ideal typical constructs of these constructs. The latter are by no means purely subjective or arbitrary but accord with the "procedural rules valid for all empirical sciences" and are:

> objective ideal typical constructs and, as such, of a different kind from those developed on the first level of common-sense thinking which they have to supersede. They are theoretical systems embodying testable general hypotheses in the sense of Professor Hempel's definition.[34]

Schutz' answer turns on the construction of models of rational action which suggest the importance of teleological explanations in history and the social sciences to supplement causal explanations. His argument leads, it seems, to an empirically-oriented science of teleology in order to devise adequate methods of selection for resolving problems or achieving goals. He clearly recognizes the

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[34] *Ibid.*, p. 246.

dependence of meaning and truth in social inquiry upon the purposes of the inquirer. What he calls "understanding" might then be achieved not by the "intuitionist" construal of Verstehen but by a theory of experimental teleology, which takes explanatory hypotheses as means for achieving the objectives of the inquiry, and hence appraisable on grounds of efficiency.

However, Schutz makes two additional assumptions, each more controversial and at best misleading. One concerns his view of the particular attitude of the historian or scientist to the social world; the other, one of his criteria for appraising these second-order theoretical systems of constructs: the postulate of adequacy.

Following the value-neutrality thesis of Max Weber, adhered to by Nagel and Hempel, Schutz finds the proper attitude of the historian and theoretical social scientist to be the same as that of the natural scientist: the historian must be a mere disinterested observer of the social world. The theoretical scientist "*qua* scientist," not *qua* human being:

> is not involved in the observed situation, which is to him not of practical but merely of cognitive interest. It is not the theater of his activities, but merely the object of his contemplation. He does not act within it, vitally interested in the outcome of his actions, hoping or fearing what their consequences might be, but he looks at it with the same detached equanimity with which the natural scientist looks at the occurrences in his laboratory.[35]

There is, of course, a sense in which this view amounts to sound methodological procedure, especially useful in controlling or

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[35] A. Schutz, "Common-Sense and Scientific Interpretation of Human Action", p. 336.

eliminating various biases of the inquirer. But in a deeper and more important sense, it is most misleading since neglecting the pragmatic dimension of inquiry by suggesting that the scientific enterprise (natural, social or historical) is value-free. It indicates that the scientist qua scientist can and must decide to accept or reject hypotheses independently not only of the specific observed situation, but of all cost, decisional and value considerations. To attain objectivity in scientific results, to keep them under control, Schutz requires the scientist qua scientist to be "governed by the disinterested quest for truth in accordance with preestablished rules, called the scientific method."[36]

First-order common-sense constructs are formed from the perspective of the actor within the world, which depends upon the actor's biographical situation and in turn determines his motives, attitudes and purposes. The historian, however, considers his position within the social world as irrelevant to his explanatory undertaking. He replaces his personal biographical situation with his inquiring situation. What is taken for granted in daily life may be a subject of inquiry for him, and vice versa. What seems relevant on one level may not be on another.

Again, if this means only that the inquirer operates with a different framework, set of presuppositions, purposes and system of relevances than does the common-sense man in daily life, then Schutz is clearly correct. But to the extent he suggests that either

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[36] Ibid., p. 337.

the social scientist or historian operates in a closed system with a fixed set of procedural rules, to this extent is his thesis erroneous. And the use of "objectivity" to mean "value-free" or "independence of the inquirer's attitudes or values" implies that one can decide to accept historical, or other statistical, hypotheses on the basis of purely logical considerations independently of the very purposive dimension he emphasizes.

In a later chapter we argue against this view. Rather, the pragmatic element, and the subsequent making of value judgments, constitutes an intrinsic ingredient in such decisions. Hence "objectivity" must be interpreted in a manner wide enough to allow for them. Neither the historian nor the scientist can really escape his own biographical situation. Nor can the historian avoid being "vitally interested in the outcome of his actions," for the relationship between believing and acting, it will be argued, is much closer than Schutz, Weber, Nagel or Hempel seem to recognize. In fact, this relationship constitutes precisely what I take to be the important insight of <u>Verstehen</u> theorists. That is, one important difference between the various empirical sciences, a difference in degree, concerns the extent to which the acceptability of hypotheses necessitates the use of criteria other than evidential or confirmatory strength, criteria involving the denial of the VN thesis.

If this case can be made, perhaps the peculiarity of the historian's subject matter, intentional meanings, can be accounted for as merely requiring a heavier reliance on these other criteria than do the natural sciences. To see what bearing these elements have

on historical explanations of human actions, we will direct our attention in the next chapter to Dray's normative reconstruction of Verstehen and SU. His subsequent criticisms of the CL model turn on a rejection of VN. Meanwhile, we need comment on the second assumption of Schutz' position, one related to the "epistemological" level of Verstehen.

Although the constructs of the historian and social scientist are removed from and refer to the constructs developed at the common-sense level, they are, according to Schutz, by no means arbitrary. They are appraisable according to three postulates: logical consistency, subjective interpretation and adequacy. The latter deserves our attention, since the second has already been examined and the first obviously needs no elaboration for our purposes. The criterion of adequacy is designed to assure "that the thought objects of the social sciences... remain consistent with the thought objects of common sense, formed by men in everyday life...."[37] That is to say,

> Each term in a scientific model of human action must be constructed in such a way that a human act performed within the life-world by an individual actor in the way indicated by the typical construct would be understandable for the actor himself as well as for his fellow-man in terms of common-sense interpretations of everyday life.[38]

This postulate or criterion of adequacy, however, needs further clarification. In what sense these human actions must be explained so as to be "understandable to the actor himself" in common-sense

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[37] Ibid., p. 342.

[38] Ibid., p. 343.

terms, Schutz fails to specify. It is clear though why he feels compelled to postulate some such condition of adequacy. His primary concern here is with the twofold naturalistic claim: that human behavior should be studied and explained as the natural scientist explains his object, and that the goal of history and the social sciences is to explain "social reality" as experienced by man living his everyday life. For these two claims he takes to be incompatible with each other. This incompatibility results from the fact that the more fully refined and developed the abstract system of second-order constructs becomes, the further removed it is from the first-order constructs of common-sense in terms of which men experience their own and others' behavior. Thus, to avoid this difficulty Schutz postulates his condition of adequacy. In addition, he advocates the use of particular methodological devices, models of rational action, controlled by the postulate of subjective interpretation. Taking the two naturalistic claims to be incompatible, and compelled to accept some sort of consistency between historical or scientific explanations and common-sense understanding,he denies the possibility of explaining human behavior in the same manner as the objects of natural science. Thus his defense of Weber's SU thesis.

Close analysis of Schutz' writings, however, reveals no serious reasons why human behavior cannot be explained in terms of the CL theory. That is, granting that the theory countenances the construction of models of rational actions, and that the laws appealed to in the explanations are not merely mechanistic or universal.

But then neither Popper nor Hempel restrict the kinds of laws usable in the CL model so as to preclude teleological, functional or statistical generalizations. Hence, if the incompatibility between the two naturalistic theses depends on explaining human actions on a purely mechanistic level, Schutz is certainly correct in denying this and opting for the consistency between common-sense and historical understanding.

Two further comments seem necessary at this juncture. First, we will have to pursue in more detail than does Schutz the question raised above: in what sense and to what extent must historical explanations be consistent with those of common-sense and understandable to the historical agents or actors. But since this question relates in important ways to William Dray's pragmatic interpretation of Verstehen along the lines of reconstructing the agent's rationale, to be considered in the next chapter, it will be more instructive to discuss the question in that context.

Further, Dray's early work depicts the notions of historical explanation and understanding so as to make the compatibility of these notions with those of common usage an important, if not necessary, condition for the adequacy of a theory of historical explanation. Schutz' criterion of adequacy, in other words, correlates closely with the recent analytic notion of "counterintuitivity" whereby a theory or meaning is considered at least prima-facie unacceptable if it violates, or is not in accord with, sound common usage. Even Hempel in his defense of requirement (R_4), you will recall, resorted to some such criterion. We argued in Chapter II

that such a criterion was neither clear nor acceptable as a condition of the adequacy of philosophic explications or theories, and that it might be replaceable by a better one, one related to the purposive and pragmatic aspects of explanation. Hence, we will also pursue this aspect of Schutz' criterion in connection with Dray's early work.

But, secondly, before accepting the extension of the CL theory to teleological or motive explanations, we will also consider whether there are other reasons than those adduced so far for rejecting the CL theory as an adequate model for explanations of human behavior. In particular, we will pursue Schutz' suggestion that such explanations require the construction of models of rational action controlled by the postulate of subjective interpretation. This requirement serves as the basis of recent defenses of Weber's SU thesis. Accordingly, some such reasons, and alternative models of explanation, proposed by recent analytic philosophers of history, will be examined in succeeding chapters. We open our discussion of this question in the next chapter with an analysis of Dray's extreme attack on Hempel's CL model, and of his novel alternative model of rational explanation. Then, in Chapter V, we pursue the more moderate criticism of the CL model offered by Gardiner, Donagan, Brandt and Scriven. The main thrust of both the extreme and moderate critics, it will be argued, agrees with that of Lavine and Schutz. The attack should be directed not at the CL theory but instead at the VN thesis. In the final chapter this thesis will be defended.

CHAPTER IV

WILLIAM DRAY'S RECONSTRUCTION OF THE SU THESIS

Some Relations Between the SU and VN Theses

In the last chapter we reviewed some criticisms of the covering law theory of explanation when extended to history. The discussion centered on explanations of human actions, as one kind of historical explanation. We considered various formulations and defenses of the SU thesis and of an alternative type of explanation or understanding. Yet the result seems to be that none of the reconstructions of Weber's position, though offering important suggestions, seriously damage the claims of CL theorists. So far, at any rate, we have found no substantial reason for upholding SU or IT, for suspecting that the insights of Verstehen theorists concerning the peculiar subject matter of history makes the CL model "peculiarly inept." Nor that historical explanations have a "different kind of logic" than that of scientific explanation. That is, so long as purposive elements like human motives, attitudes, goals and purposes can be adequately fit into the model. But this is precisely the point that requires more attention, since much recent criticism of the CL model tries to show the model's ineptness because it cannot account for human actions and dispositions. The inability to explain adequately the rationale or subjective meaning of human actions is still offered as the single greatest obstacle to the CL model.

These and related charges receive one of their most cogent and persuasive statements in a recent book by William Dray, *Laws and Explanation in History*. It is to Dray's attempted rehabilitation or reconstruction of Weber's SU thesis that we now turn.[1] The doctrine under consideration, you will recall, begins with the premise that the objects of historical inquiry, human actions, differ fundamentally from the objects of the natural sciences. It concludes that such objects cannot be explained or understood merely by subsuming them under empirical covering laws. Hence the covering law theory is adjudged inappropriate in history, which is taken as an autonomous discipline with a peculiar "logic" of its own.

Upon brief survey of both the idealist position and the "official" covering law answer to it, Dray decides that the latter evades the main thrust of the former's doctrine. The residue left out of their answer he depicts as not a psychological matter of discovery but something that "should properly be taken into account in a *logical* analysis of explanation as it is given in history."[2] Consequently, he attempts to rehabilitate or "makes sense" of what Weber and some of the idealists said about historical understanding, and to do so in such a way as to make the SU thesis immune to the charges of the "official" answer. His analysis also develops some of the issues raised in the last chapter by Lavine, Natanson and Schutz.

There is, however, one major point of interest, All of the critics of the covering law theory considered in the last chapter

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[1] See especially Chapter V, "The Rationale of Actions."

[2] *Ibid.*, p. 121.

agreed with Hempel, Popper and Nagel that the historian qua historian, as well as the scientist qua scientist, makes no value judgments. Most critics and defenders alike assume historical analysis and explanation to be a value-neutral activity, the results of which ought not to depend in any essential way upon the attitudes, interests or values of the inquirer. It ought instead to yield a set of identical conclusions for all competent inquirers "governed by the disinterested quest for truth in accordance with preestablished rules called the scientific method."[3] Dray, correctly I think, opposes this uneasy alliance. His reconstructed version of "empathic understanding" depends essentially upon denying the value-neutrality thesis. But again, as with those considered in the last chapter, Dray erroneously takes this position to conflict with the main tenets of the covering law theory. For the historian, in order to explain some human action, according to Dray, must appraise or evaluate the action in its context as appropriate and rational, thus appealing for explanatory force to normative principles of action rather than empirically descriptive covering laws. Dray attempts to reconstruct the 'empathy' position, then, by negating one crucial tenet of Weber's position, viz. the value-neutrality thesis of empirical science.

This is especially noteworthy since Dray rejects Hempel's CL theory as an unsatisfactory reconstruction of Weber's position, even though Hempel agrees with Weber regarding value-neutrality. We then

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[3] See footnote #36, p. 129

have the puzzling situation whereby Dray, with Weber, insists on the SU thesis and on reason explanations as autonomous, but only at the price of giving up the value-neutrality thesis; while Hempel disagrees with Weber's insistence on autonomy, but agrees with him regarding value-neutrality. I submit the thesis that Weber is wrong on both counts, and hence that Hempel and Dray are both partially correct. Hempel correctly rejects autonomy and SU in favor of the covering law model, but Dray is equally right to reject the value-neutrality thesis. Moreover, I will argue that these two theses are not only compatible but that an adequate reconstruction of the covering law theory includes the denial of value-neutrality. Thus, if the two theses were incompatible, as Dray suggests, we would indeed have to choose between them. But Dray's criticism of the covering law model on the grounds that it requires value-neutrality is misplaced. Value appraisals enter the domain both of historical and scientific explanations. Yet not in the way Dray suggests, i.e. not as the explanatory force. They enter instead, as Lavine and Schutz both indicate, in the inquirer's use of judgment to determine the acceptability of his explanatory hypotheses.

Accordingly, our investigation of Dray's case centers on three central questions. First, does Dray's reconstruction of the idealist doctrine sufficiently establish the denial of value-neutrality? If not, secondly, is there any other way of securing this denial by modifying or redirecting Dray's argument? Can we, in other words, discover the weakness of his reconstruction so as to improve upon it? Finally, if an affirmative answer to either of the other questions seems advisable, i.e. if the value-neutrality thesis can be successfully attacked on any grounds, how would this bear on the covering law theory of explanation? Would, e.g., the denial of VN entail the SU thesis?

We might note here that the most important relationship between these two theses seems to concern only the probabilistic model (P), not the deductive model (D). This point becomes increasingly important when we see to what extent Dray's criticism of the covering law model generally is directed only to the deductive part of it. Hence, this chapter will be devoted largely to our first question, while succeeding chapters deal with the last two questions.

To elicit in their fullest form Dray's proposals about the connection between historical value judgments and explanations necessitates a consideration of the continuing debate stimulated by Dray's book and related essays. In particular, we will consider Hempel's response to Dray's charges, which occurs in the context of a new attempt to extend the covering law theory to include within its scope explanations of purposive human actions. We propose in this chapter, then, to clarify both the covering law theory and Dray's alternative model of explanation in order eventually to answer our first question.

The Rational Model of Explanation

As a particularly clear and representative example of how historians explain individual purposive human actions in terms of motivating reasons or beliefs, Dray cites G. M. Trevelyan's account of the successful invasion of England by William of Orange. In response to the question, "Why did Louis XIV make the greatest mistake of his life in withdrawing military pressure from Holland in the summer of 1688?" Trevelyan explains that:

> He was vexed with James, who unwisely chose this moment of all, to refuse help and advice of his French patron, upon whose friendship he had based his whole policy. But Louis was not entirely passion's slave. No doubt he felt irritationwith James, but he also calculated that, even if William landed in England, there would be civil war and long troubles, as always in that factious island. Meanwhile, he could conquer Europe at leisure. 'For twenty years', says Lord Action, 'it had been his desire to neutralize England by internal broils and he was glad to have the Dutch out of the way (in England) while he dealt a blow at the Emperor Leopold (in Germany).' He thought, 'it was impossible that the conflict between James and William should not yield him an opportunity.' This calculation was not as absurd as it looks after the event. It was only defeated by the unexpected solidity of a new type of Revolution.[4]

Such accounts Dray labels "rational explanations," because they reconstruct the "agent's calculation of means to be adopted toward his chosen end in light of the circumstances in which he found himself" in order to display "the rationale of what was done."[5] In so doing they constitute a distinctly different kind of explanation than subsumption under empirically verifiable laws and initial or antecedent circumstances, since they employ a quite different

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[4] Trevelyan, The English Revolution, pp. 105-6; quoted by Dray, ibid., p. 122.

[5] Dray, ibid., p. 122 and 124.

criterion of intelligibility from that formulated by the covering law theory. And they employ a different criterion because the "goal of such explanation is to show that what was done was the thing to have done on such accasions, perhaps in accordance with certain laws."[6] Since the infinitive "to do" functions, for Dray, as a value term, he claims that "there is an element of appraisal of what was done in such explanations; that what we want to know when we ask to have the action explained is in what way it was appropriate."[7] Accordingly, the reasons an historian offers to explain in this rational manner must be "good reasons" from the agent's point of view, must be such that "if the situation had been as the agent envisaged it..., then what was done would have been the thing to have done."[8] Hence, since rational explanations need not be covered by general empirical laws of either a universal or probabilistic type, Weber's SU thesis is vindicated.

The distinction to be drawn at this juncture is, of course, that between a cause and a reason, since the expression, "An actor A did X because of Y," is ambiguous. "Because" can serve to indicate sometimes a cause and at other times a reason. When one says, for example, "Louis withdrew military pressure from Holland because he was vexed with James", one has offered a causal "because" and hence a causal explanation that can easily be fit into the

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[6] Ibid., p. 124.

[7] Ibid., p. 124.

[8] Ibid., p. 126.

covering law model. But when one says "Louis withdrew military pressure from Holland because he thought he could conquer Europe at leisure," one offers not a cause but a reason for the action, one produces a rationale of the action, that which tends to either justify or excuse what was done.

In such a case one offers what Dray calls a "rational explanation" in order to make intelligible from the agent's point of view, his grounds for so acting, to make sense of his action. Only by reconstructing the agent's calculations or reasons, "by putting yourself in the agent's position can you understand why he did what he did."[9] The whole purpose of Trevelyan's explanation, according to Dray, lies in showing that Louis' unfortunate action, even though based on miscalculation, was appropriate to the envisioned circumstances. In this way, Dray claims, we begin to see the point of the Weberian and idealist insistence on Verstehen, of behavior maxims and of the "projection" metaphors, which covering law theorists dismiss as merely psychological or "methodological dodges." Collingwood's inner-outer dichotomy, for instance, becomes transformed into the cause-reason distinction and its corresponding causal-rational explanation distinction.

The covering law model is accordingly claimed to be irrelevant to historical actions since we want to know not how actions could have been predicted in advance, but the reasons why people did the things they did. To ascribe causes to human actions apparently

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[9] Ibid., p. 128.

commits some sort of category mistake, in Ryle's terms. In rational explanations it suffices that what provides the agent with a reason for acting be a rationally necessary condition that he had no reason to do what he did otherwise. It is not essential to show that he could not have acted in the same way without having that reason.[10]

But Dray requires an additional characteristic for rational explanations, because the conceptual connection between understanding an agent's action and discerning its reasons or rationale is neither deductive nor probabilistic. Subsuming the action logically under suitable empirical laws is, he argues, neither a necessary nor a sufficient condition of explaining. Hence, he refuses to concede the point we emphasize: all of the above amounts merely to recognizing an additional pragmatic condition historical explanations must satisfy, a condition tacked on to those of the covering law model. Instead, Dray condemns the covering law model as essentially inept in accounting for human actions. Thus he finds it in need not of additional conditions but of essential modification. Rational explanations require not the connecting bond of general descriptive empirical laws but of normative "principles of action", standards of appropriateness and rationality. Such practical principles "express a judgment of the form: 'When in a situation of type $C_1 \ldots C_n$ the thing to do is X.'"[11] To explain a persons

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[10] Dray, "The Historical Explanation of Actions Reconsidered," in S. Hook (ed.), Philosophy of History (N.Y.: New York University Press, 1963), p. 129.

[11] Dray, Laws and Explanation in History, p. 132.

behavior, then, one must represent it as the reasonable thing to have done in the circumstances. One appeals to the general knowledge expressed by rules or principles of behavior instead of empirical generalizations, to knowledge of what to do rather than of what is usually done in such circumstances.

Although Dray does not dwell on the kind of circumstances referred to in these principles, it seems clear from his general analysis that they must include at least reference to the agent's goals or purposes, his beliefs about the empirical circumstances of his action and alternative courses of action, and his own moral standards or principles of conduct. Consequently, rational explanations provide answers to questions of the form, "Why did agent A do act X?" by offering the following type of explanatory argument:

(A) 1. Agent A was in a situation of type $C_1 \ldots Cn$ (*i.e.* C)

2. In a situation of type C the thing to do is X.

3. Therefore, agent A did X.[12]

The first part of model (A) specifies certain antecedent conditions as do the covering law models (D) and (P). But the second part, the connecting link between reasons and action which gives the argument its explanatory force, the principle of action, replaces the general empirical or descriptive laws of (D) and (P). Hence, as Dray claims, (A) clearly differs from the covering law model. It constitutes a distinctly different type of explanation and employs a different criterion of intelligibility, because it contains an element of appraisal.

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[12] Hempel, "Reasons and Covering Laws in Historical Explanation," in S. Hook, *op. cit.*, p. 154.

Because the historian must appraise or make value judgments about the appropriateness of the agent's action to his reasons, historical explanations of human actions are sui generis or irreducible to the CL models. Rational explanations remain essentially different from those offered in the natural sciences even though the determining motives, beliefs or reasons can be classified among the antecedent conditions of motivational explanations. In this way Dray defends Weber's SU thesis by attacking VN.

In emphasizing the importance of appraisals or principles of action as the explanatory force in rational or motivational explanations, Dray follows the recent lead of Patrick Gardiner. In general Gardiner advocates the covering law model in historical inquiries. But the looseness or vagueness of laws used in historical explanations, which allow considerable width of interpretation compels him to modify or refine the theory.

Surely historians offer many explanations without committing themselves to any covering laws. But covering law theorists suggest that such cases are only "explanatory sketches", partial explanations or enthymemes. They require conpletion in the sense of eliciting the governing laws in order to be defensible if challenged. Without such a defense the explanation would be at best a pseudo one. Gardiner agrees that explanations must ultimately rest on warranting generalizations. However, he is impressed by the fact that historians seldom conform to this pattern or defense in practice. Instead of appealing to general laws, the historian often completes his explanation by filling in details about the situation under consideration, by telling more of the story. Further, the historian relies in his defense on personal decisions and judgments, since his interpretations are so loose and porous. Hence, Gardiner, led by the actual practice of historians, claims that they, "like the general or statesman, tend to assess rather than to conclude"; and that "there is, indeed, a point in terming (for example) the explanations provided by the historian 'judgments.'"

But these assessments or judgments are not just incomplete sketches as Hempel suggests. They are not "made, or accepted, in default of anything 'better': we should rather insist that their formulation represents the end of historical inquiry, not that they are stages on the journey towards that end."[13]

If so, however, it is not clear why Gardiner's account constitutes merely a refinement of the covering law model, as he claims. For, in suggesting that these decisional explanations are not "half-way houses" or incomplete sketches to be filled in by explicitly formulated generalizations, but represent rather the "end of historical inquiry," Gardiner surely argues for one version of SU. If such explanations are complete of their own kind, the covering law model is simply inapplicable in such cases. That these judgmental explanations constitute a sui generis category, not reducible to the covering law model, is further attested when he persistently claims not to be

> implying that they are 'subjective' in any vicious sense. For the word 'judgment' must be regarded as being ... simply that the criteria for assessing the validity of any

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[13] Gardiner, The Nature of Historical Explanation (London: Oxford University Press, 1952), pp. 95-6.

given explanation in history are, in general, different from those appropriate to the assessment of explanations as they occur in certain branches of scientific inquiry.[14]

As already noted, Dray correctly holds that the logic of rational explanation requires showing the presence, on the occasion of the action, of the antecedent conditions. The circumstances surrounding the action, the agent's beliefs and goals, and the available alternative courses of action would be considered such determining conditions of the given action. But, of course, however necessary showing this and the thing to do in such circumstances might be, an historian must still establish which, if any, of these factor was in fact the reason for the agent's action. Surely without producing some evidence to this effect the historian's explanation of, say, why Louis withdrew military pressure from Holland in the summer of 1688 would be either incomplete, dogmatic or perhaps even a pseudo-explanation. For, though some factor may have been present when the action was committed, it might indeed have been causally inoperative. For example, in Trevelyan's explanation of Louis' action, Louis might have believed there would be civil war in factious England if William landed there. Still, this fact does not establish this as the reason why Louis withdrew pressure from Holland, any more than the fact that he was vexed and irritated with James proves that this was the reason. How many persons are known to have hated J.F. Kennedy's civil rights stand enough to have assassinated him? Yet this kind of hatred was apparently not directly

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[14] Ibid., p. 95.

responsible for his death, as many at first assumed. Even if the assassin was so motivated, it would still remain to be established that the assassination occurred because of this hatred, instead of for countless other causes or reasons.

But if an historian's proof that a certain factor was present does not establish this factor as a reason why the agent acted as he did, then how can an historian support a claim for the causal efficacy of any given factor? In what way, in particular, does a principle of action provide support for such a claim? To clarify Dray'w own position it will be helpful to dwell for a moment on the position he rejects, the solution offered by Gardiner, Nagel and others.

Covering law theorists generally resort at this juncture, of course, to empirical laws or generalizations to the effect that whenever such conditions occur, events of this sort result. With Nagel they would contend:

> The historian can justify his causal imputation by the assumption that, when the given factor is a circumstance under which men act, they generally conduct themselves in a manner similar to the particular action described by the imputation, so that the individual discussed by the historian presumably also acted the way he did because the given factor was present.[15]

To this Gardiner adds that the "because" in motive explanations, as "John hit you with a hammer because he is bad-tempered," represents an "instance of how he can in general be expected to behave under certain conditions. It sets John's action within a pattern, the

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[15] E. Nagel, The Structure of Science (N.Y.: Harcourt, Brace and World, 1961), p. 555.

pattern of his normal behavior."[16] And such patterns are represented by covering laws or generalizations. Hence, these explanations are not essentially different from psychological and sociological investigations of why people behave as they do on given occasions, from what in current social research is referred to as "reason analysis."

Yet, as noted above, Gardiner is unhappy with the appeal to vague generalizations about human behavior as a defense of the historian's causal imputations. He turns for a replacement to judgments, decisions and assessments. Most covering law theorists, on the other hand, argue that such judgments ought to be replaced by inferences by inferences from covering empirically validated laws or generalizations; otherwise the historian's defense of his explanation would not be certified as rationally acceptable.

No doubt such cases of historical reason explanations are incomplete when judged according to the ideal covering law models. Sometimes they are indeed enthymematic, containing implicit statistical generalizations which do not deductively entail a singular conclusion but only support it with some degree of probability. At other times they amount to mere "explanation-sketches." But often they are partial explanations, described in chapter one. Nagel's recent analysis of the general structure of partial explanatory arguments remains the most instructive:

> Let A_1 be a specific action performed by an individual X on some occasion t in order to achieve some objective O. However historians do not attempt to explain the performance

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16 Gardiner, op. cit., p. 125.

> of the act A, in all its concrete details, but only the performance by X of a type of action A whose specific forms are act A_1, $A_2 \ldots, A_n$. Let us suppose further that X could have achieved the objective O had he performed on occasion t any one of the actions in the subset $A_1, A_2 \ldots A_k$ of the class of specific forms of A. Accordingly, even if a historian were to succeed in giving a deductive explanation for the fact that X performed the type of action A on occasion t, he would not thereby have succeeded in explaining deductively that X performed the specific action A_1 on that occasion. In consequence and at best, the historians explanation shows only that under the assumptions stated, X's performance of A_1 on occasion t is probable.[17]

Apparently, then, historical explanations of individual actions are at best interpreted, according to the covering law theory, as eases fitting model (P), because of the essentially statistical character of generalizations about human motives, reasons and conduct. At any rate, the heart of this view is that only empirical laws can serve the logical function of producing explanatory force by connecting the antecedent conditions (reasons, motives or causes) to the explanandum-event (physical events or human actions) either deductively or probabilistically.

Let us see, now, why Dray objects to this analysis, why he resorts to principles of action as a substitute for empirical generalizations, and how well they respond to empirical evidence. Dray seems most concerned with the claim that historical events and conditions are unique, and hence require to be accounted for by characteristically historical explanations. He contends that the historian, in his attempt to explain the French Revolution say, is "just <u>not interested</u> in explaining it as a revolution," as Nagel's account would suggest.

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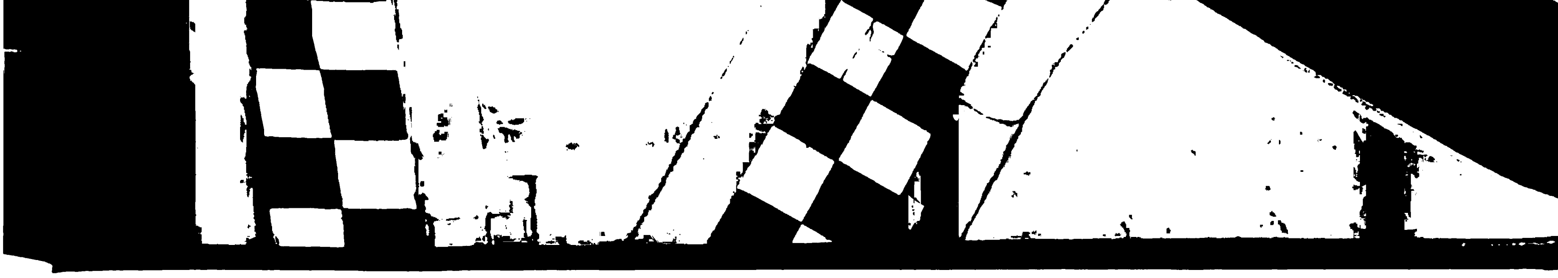
17 Nagel, <u>op. cit.</u>, p. 558.



Instead the historian is

> almost invariably concerned with it as different from other members of its class... that is to say, he will explain it as unique, (not absolutely but) in the sense of being different from others with *which it would be natural to group it under a classification term*...[18]

Consequently, treating such events or actions as instances of anything, subsuming them under classification generalizations or laws, is to abandon historical inquiry for scientific. Such accounts leave out, for Dray, the most important ingredient of judgment.

> The missing element is surely a 'law' or 'rule' which would inform the historian when such a group of 'predisposing' conditions become sufficient.... The conclusion that the revolution or unpopularity could reasonably have been predicted....would be reached by an excercise of the historian's judgment in the particular case.... Collating a number of conditions, including supporting laws, is not applying a further *covering* law, perhaps in a vague way. It is *doing something quite* different and much more difficult.[19]

The "something different" now appears, however, to be something like Lavine's decision to accept the hypothesis that the weight of the evidence suffices to warrant our belief, i.e. a weighing of a set of evidential factors. And Nagel's attempt to represent this judgment in simple, formal terms is considered mere "prejudice."

Another supporter of this position, M. Scriven, sees the "great truth in the *Verstehen* theories" to consist in their badly conceptualized formulation of "the *indispensability* and *efficiency* of the historian's capacity to respond to the cues in a well-described situation, so that he may with justifiable confidence

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18 Dray, *Laws and Explanation in History*, p. 47.

19 *Ibid.*, p. 55.

accept or propose a particular reason-explanation as correct...."[20] With this view we have repeatedly concurred, yet we also stress the failure of the accompanying reconstructions to explicate and develop this "great truth" sufficiently enough to count as a serious challenge to the covering law theory of explanation.

If our interpretation is correct, it would seem that Dray and Scriven have confused three different questions: what constitutes an historical explanation? and how can one justify or defend an historical explanation? and what constitutes an acceptable historical explanation? Dray's argument began by trying to show that unique historical events could not profitably be subsumed under covering laws. But it ends by showing the need for additional judgments in order to defend and collate the covering laws that do profitably enter into historical explanations.

We submit that by clarifying the above confusion, and by relating the appraisal element to the notion of justification and the rational acceptance of hypotheses or explanations, Dray's case can be transformed into a serious challenge to the covering law theory on its probabilistic side. In other words, Dray's argument fails to support the conclusion he wants: that historical explanations can be fully warranted or rationally acceptable without covering laws of any sort. Nevertheless, it suggests a related criticism of the covering law theory: that the historian qua historian, in his explanatory practice, must make value assessments or judgments.

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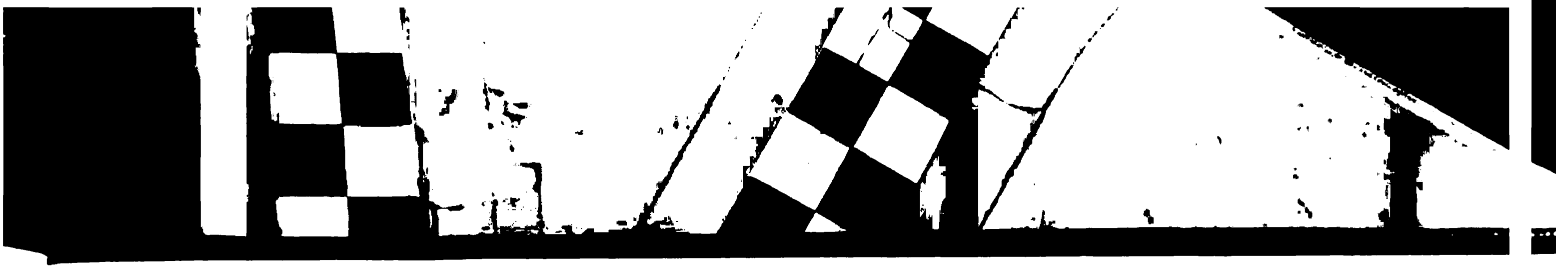
[20] M. Scriven, "New Issues in the Logic of Explanation," in S. Hook, op. cit., pp. 358-9.



And make them not in lieu of but over and above, and especially about, his covering laws. Such judgments concern the extension of the logic of explanation to include model (P), and hence the rational acceptability of the statistical generalizations employed in, for example, Nagel's account of partial explanations. This is a point barely touched on by Lavine, Schutz and now Dray, but nowhere developed to its full potential by any of them in their reconstructions of the SU thesis and the notion of Verstehen. Accordingly, we examine it in greater detail in chapter six, after analysing Dray's position.

But it must also be noted that Dray has in no way established even this point. Indeed he merely asserts it to be the case. He sets out to establish, as we have seen, the necessity for rational explanations to contain assessments, in the sense of principles of action substituted for empirical generalizations. Precisely this move confuses what we take to be his important but undeveloped insight. The issue is not whether laws, as opposed to judgments, provide explanatory connection and force, but how to determine which laws are rationally acceptable as explanans. This, we suggest, as a case of practical judgment, casts suspicion on the VN thesis without in any way supporting SU. And such suspicion opens for inquiry many important questions.

But before considering the tenability of either the "rational" model of explanation as a rehabilitation of the "empathy" point of view, or of its peculiar emphasis on the normative element of appraisals, or even of the specific objections contained therein to

the covering law model, two further aspects of Dray's model need elaboration. One concerns the essentially pragmatic analysis of explanation offered by many critics of the covering law theory. The other relates to Gardiner's fear of introducing viciously subjective elements into historical explanations as a corollary to the entrance of appraisals or judgments. This, of course, raises once again the major problem of testability and pseudo explanations, faced by all advocates of _Verstehen_. Dray recognizes that "To allow the legitimacy of empathy appears to many of its opponents as the granting of a license to eke out scanty evidence with imaginative filler."[21] His case, in other words, needs completion by showing in what sense "rational" explanations are logically, not just psychologically, different from the covering law model; and in what sense they are responsible to inductive evidence and do not go beyond the controls of empirical inquiry. Let us consider the former point first.

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[21] Dray, _op. cit._, p. 129; _Laws and Explanation in History_, p. 129.

Pragmatic Dimensions of Explanation

From our earlier account of the covering law theory, it is clear that its proponents provide a formal analysis of 'explanation,' as showing something to be subsumable under or deducible from general laws, mainly in order to achieve some objective criteria for what counts as sound or rationally acceptable explanation, and also as a genuine scientific theory of human action. They have been most reluctant, consequently, to countenance the pragmatic aspects of explanation, no less the element of empathy, as little more than a psychological peculiarity. It will be essential, then, to consider Dray's complaint that CL theorists mistakenly take 'explanation' to be a term of formal logic instead of mainly a pragmatic term. For Dray proposes to deal not with the psychology but the logic of historical thinking. Yet he uses the term 'logic' not in the narrow sense of formal logic but in the much broader sense made popular by contemporary pragmatic and analytic philosophers. There is, he claims, in the broad sense "an irreducible pragmatic dimension to explanation,"[22] which helps to bring "the analysis of the concept more into line with the way the word is used in the ordinary course of affairs."[23]

This broad interpretation of explanation provides it with greater scope by making it applicable to such ordinary ways of talking as "explaining my meaning," "explaining the use of a tool,"

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22 Ibid., p. 69.

23 Ibid., p. 75.

"explaining my point of view," and "explaining my purpose." In this way his version also correlates with Schutz' criterion of adequacy, mentioned in the last chapter. By thus broadening the notion of explanation, Dray, along with Schutz, hopes to show the implausibility of the CL claim that its restricted formal meaning can apply to historical explanations.

This aspect of his pragmatic interpretation, and of Schutz' criterion of adequacy, we find unconvincing and misleading. For to achieve this goal, they show how the CL model departs drastically from the ordinary meaning of the term 'explanation.' But this is to object to the CL theory because it "prescribes a sense of the term, rather than calls attention to one already accepted,"[24] and moreover, prescribes in the sense of importing into historical cases a special, technical sense of the term designed for narrow scientific uses. Apparently Dray wants a description of ordinary usage in order to revive the earlier idealist distinction between generality and explanation on the one hand, and intelligibility and understanding on the other. In Scriven's terms:

> Explanations are practical, context-bound affairs, and they are merely converted into something else when set out in full deductive array.... Explanation when dressed in its deductive robes becomes a proof or a justification of an explanation (and usually no longer explains but demonstrates).[25]

Hence, Dray and his supporters stress still other aspects of

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[24] Ibid., p. 79.

[25] Scriven, "Truisms as the Grounds for Historical Explanations," in P. Gardiner (ed.), Theories of History (Glencoe: Free Press, 1962), p. 450.

their pragmatic version of explanations. First of all, explanations are "context—bound affairs." They occur not in abstraction but in concrete cases. Provided at a definite time, in specific circumstances, and for a specific purpose; they are, as indicated in Chapter II, inference-tokens not inference-types. Accordingly, to grasp Dray's intended rationale for his rational explanations of historical actions, it is important to see the varied contexts in which we can and do ask for an explanation. He assumes that the demand for explanation arises out of a genuine puzzlement, that we can only offer an explanation in definite contexts where there is some particular gap in our knowledge, some particular perplexity or puzzlement, e.g. "when from the 'considerations' obvious to the investigator it is impossible to see the point of what was done."[26]

To clarify this view, Dray introduces the notions of an explanatory "scale" and a kind of "logical equilibrium." The simplest or complete case of rational explanation occurs when an agent's act is perfectly intelligible, when he did exactly what the inquirer would have done in similar circumstances. From this complete case, rational explanations are then scaled along a continuum, "depending on the amount of 'foreign' data which the investigator must bring in to complete the calculation." The 'foreign' data consists of the agent's beliefs, principles and purposes which differ from those the inquirer might have employed. Such explanation attempts to "match" an action with its calculation, i.e. to achieve a kind of "logical

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[26] Dray, op. cit., p. 125; Laws and Explanation in History, p. 125.

equilibrium."

Since the demand for these explanations only arises from one whose equilibrium is upset, the function or purpose of explanation must be to fill in the gaps and produce understanding by resolving the particular puzzlement and restoring the equilibrium to the given case. This is achieved by uncovering the agent's reasons or calculations for his acting as he did. For, when we uncover, say, Louis' reasons for withdrawing pressure from Holland, we see the point and make sense of his action. We see that given his particular beliefs, goals and grasp of the circumstances, he had reasons for doing what he did. But what aspect of his calculation requires "filling in" depends on the particular gap in the understanding of the person to whom the explanation is directed. Thus, the correctness or appropriateness of the explanation is relative to the context in which it occurs, to the needs or perplexity that produced the demand for an explanation in the first place. Understanding results from the "perception of relationships and hence may be conveyed by any process which locates the puzzling phenomenon in a system of relations," but "we deduce nothing; our understanding comes because we see the phenomenon for what it is...."[27]

Dray's position appeals strongly to many practicing historians. One, a participant in the New York University Symposium on History and Philosophy, suggests he would even call Dray "the historian's

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[27] Scriven, "Explanations, Predictions and Laws," in Minnesota Studies in the Philosophy of Science, Vol. III (Minneapolis; University of Minnesota Press, 1962), p. 193.

philosopher" were it not for the suspicion that this would be "the kiss of death."[28] Another, however, accounts for such popularity on the grounds that Dray's rational model produces "simply the common, garden variety of understanding presumably possessed by each of us," while Hempel's covering law model has "ominously threatened the historian with all sorts of generalizing sciences which he would have to understand."[29]

Now although this psychological account may or may not be adequate, it at least points to the apparent disparity of levels at which Hempel and Dray are operating. Dray claims to be describing the logic of historical explanations as they are actually offered by practicing historians, and hence emphasizes their pragmatic dimension. Hempel, on the other hand, claims to be offering a prescriptive philosophic explication of the logic of historical explanations. Hence he abstracts from the concrete contextual situation of any given explanation in order to codify principles and conditions concerning the syntactical and semantic aspects of the soundness or rational (not just personal) acceptability of such arguments.

This disparity, however, is only apparent not real. For Dray, when pressed, concedes that he is not describing historical explanatory practice any more than Hempel. His quarrel with Hempel and other covering law theorists is not that they misrepresent or overlook the

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[28] L. Krieger, "Comments on Historical Explanation in History," in S. Hook, op. cit., p. 137.

[29] B. Mazlish, "On Rational Explanation in History," ibid., p. 282.

content of rational explanations, but rather that they misinterpret the form or logic of such explanations. But only recently, and often after much persistent argumentation on the part of covering law theorists, has Dray recognized the important philosophic task to be not merely describing or duplicating what historians actually do, but a "'rational reconstruction' which may not, in every instance, coincide exactly with what a practicing historian does."[30] In other words, Dray's rational model of explanation occurs on the same level as Hempel's covering law model. Both are explications not descriptions. They are therefore serious competitors and need to be appraised as such, i.e. to the extent each constitutes an adequate codification of ordinary historical explanatory practice. The issue does not concern which is "closer to" or "better duplicates" such practice, as Dray's earlier writings mistakenly suggest.

But, as already noted, one main goal of formulating a theory of explanation, which motivates covering law theorists, is that there be some objective way of determining what counts as a rationally acceptable explanation. Consequently, Dray's rehabilitation of the "empathy" position must meet the charge that rational explanation goes beyond the scope and control of empirical inquiry by introducing viciously subjective elements into historical explanations, thus making them not just "explanation sketches" but pseudo-explanations, and giving the historian "a license to eke out scanty evidence with imaginative filler." He must guard against the view that anything which

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[30] Dray, "The Historical Explanation of Actions Reconsidered," p. 107.

relieves perplexity can count as an adequate rational explanation of an action.

Dray of course acknowledges this danger. He nonetheless rejects the attempt to dismiss "empathy" as a mere psychological or methodological dodge, and to discount its counterpart "rational explanation" as a poorer method of obtaining the same results as can be achieved more reliably by subsuming actions under empirical laws. Moreover, he defends his model against the above charge by arguing that it does have "an inductive, empirical side, for we build up to explanatory equilibrium from the evidence," from historical documents, letters, speeches, rather than from scratch. In this way controls are placed on an inquirer who might let his imagination run riot. Hence, Dray avoids any metaphysical appeal to self-evidence for rational explanations, such as those usually associated with idealist pronouncements about intuitively understanding an action by "an immediate leap to the discovery of its 'inside', without the aid of any general laws"[31] or of any empirical reasoning at all. In fact, Dray readily concedes that mistakes are possible in the inductive reasoning of the calculation and that new information may be uncovered to upset the calculation. Nevertheless, he claims the procedure to be "self-corrective" and subject only to the "normal hazard of any empirical inquiry."[32]

Unfortunately, he fails to elaborate upon this empirically

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[31] Dray, Laws and Explanation in History, p. 129.

[32] Ibid., p. 130.

"self-corrective" theme. As a result it is unclear how he intends it, how it might be implemented, in what way it is scientific even in a broad sense of that term. Perhaps whatever empirically self-corrective aspects there might be to rational explanations are due to the degree it conforms to the covering law model. Dray fails to show, in other words, that his model is susceptible to empirical controls at the points where it diverges from the covering law model. For having denounced Hempel's view of rational explanations as incomplete sketches in need of filling in, yet still indicating the direction of a better, more completed historical explanation—he seems compelled to give up Hempel's method of confirmation. But then no other method of confirmation is discussed, much less opted for. Dray seems driven to the same position as Gardiner who claims that "the criteria for assessing the validity of any given explanation in history are, in general, different from those appropriate to the assessment of explanations as they occur in certain branches of scientific inquiry."[33] But this sharp contrast between criteria appropriate to scientific and historical explanations has not been established, largely because defenders of the Verstehen position uncritically accept the covering law theorists claim of value-neutrality in scientific inquiries. And it has been this error, I think, which has prevented Gardiner, Dray, Schutz and other defenders of Verstehen from sustaining a successful attack against the covering law model of explanation, and also from developing

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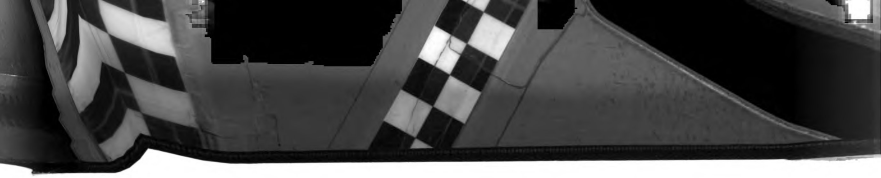
[33] Gardiner, op. cit., p. 95.



the important insight of Verstehen theorists.

Nevertheless, Dray may intend, as Gardiner does, that rational explanations require a "scientific" defense in a wide sense, viz. in requiring tests not limited to confirmation or evidential strength, in the same sense in which Lavine and Schutz earlier spoke of Verstehen. Gardiner mentions only one other method, that of practical success. One of his critics, Alan Donagan, promptly replies that such a test "may be employed in judging the assessments of generals and statesmen" but "plainly does not apply to those of historians," and hence that historical explanations as judgments or assessments are viciously subjective.[34]

Donagan's reply, however, is particularly harsh. Unless of course he assimilates all assessments and judgments to matters of personal taste, in which case he would clearly be correct, but at the cost of misinterpreting both Gardiner's "judgments" and Dray's "principles of action." Another approach will perhaps be more fruitful. Suppose, instead, we pursue further the assimilation of principles of action to empirically confirmable generalizations, which would alleviate some problems of testing principles. We will do this in more detail in the next section as a serious alternative and criticism of Dray's model (A). But for the moment let us pursue this assimilation only to elicit more clearly Dray's position, and to dispel some misunderstandings about it and the related topic of testability of principles.

Dray leaves no doubt that the employment of rational explanations

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[34] A. Donagan, "Explanation in History," in Gardiner, Theories of History, p. 432.

contains an element of implicit generality, that "reasons for acting" no less than "conditions for predicting" have a kind of generality or universality. Suppose, then, despite the distinction between reasons and causes, we still contend that a sound, acceptable or complete rational explanation must treat the data of the agent's calculation as antecedent conditions. As empirical data from which what was done could have been predicted, they are to be connected with what was done by a covering, empirically confirmable generalization. We might, in other words, concede a difference in content of "cause" and "reason" statements, yet argue for their similar logical function in explanatory arguments. If we said, for example, "Disraeli attacked Peel because Peel was ruining the landed class," we might mean, Dray agrees, that anyone sufficiently like Disraeli in relevant respects would have done the same thing in a situation sufficiently similar in relevant respects.[35] And, generally, "if Y is a good reason for A to do X, then Y would be a good reason for anyone sufficiently like A to do X under sufficiently similar circumstances."[36]

But Dray objects to this assimilation of principles of action to empirical generalizations on the grounds that the universality in the two cases is sufficiently different to make the assimilation methodologically hazardous for the historian. This kind of procedure would commit the historian who offers a rational explanation to the

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[35] Cf. the treatment of this locution by J. Hospers, Human Conduct (New York: Harcourt, Brace and World, 1961), pp. 320-22.

[36] Dray, Laws and Explanation in History, p. 132.

truth of a corresponding empirical law or generalization. And this is precisely what it takes, according to the covering law theorist, to make such explanations responsible to empirical evidence. But Dray insists they are responsible to evidence in different ways, since empirical laws are disconfirmable (falsifiable) while principles of action are not. If we found a negative instance of each, he maintains, "the law itself must be modified or rejected," while the principle of action "would not necessarily be falsified", and this because the former is descriptive and the latter prescriptive.[37]

This, at best, confuses a number of complex issues. On its face Dray's defense is clearly incorrect. Surely an empirical law, any more than a principle of action, need not be rejected or even modified on the basis of one negative instance. Surely the arguments of Duhem, Quine, Hempel and Popper concerning falsifiability deserve more consideration than this. In fact, Dray's case suffers generally from an insufficient analysis of empirical scientific methodology, of description and confirmation, and of the role of decisions and judgments in empirical inquiry. At any rate, if falsifiability on the basis of one negative instance is not sufficient to support the distinction between empirical laws and rational principles, Dray has only two alternatives. He can take principles to be unfalsifiable in principle or he can view them, along with laws, as partially falsifiable though in a more complex way.

But here he faces a simple dilemma. If his principles of action

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[37] Ibid., p. 132.

are unfalsifiable, then of course they cease to be empirically significant statements about the world, which he claims them to be, since they are no longer amenable to empirical control. But then there is no sense in which they are "self-corrective" short of some metaphysical or a priori procedure, and hence the fear of Gardiner and others would be borne out: they become viciously subjective or "self-evident." If, on the other hand, his principles are at least partially susceptible to empirical refutation, as are laws, then finding some large number of instances, *e.g.* finding that most people do not act in accordance with them, would at least create a strong presumption against the claim of the principles about the thing to do in the given situation.

But at this juncture Dray makes a peculiar move. Having correctly noted that no amount of empirical evidence compels the withdrawal of the principle, he then claims that "if it was not withdrawn, the explanatory value of the principle for those actions which *were* in accordance with it would remain."[38]

The consequence of this move is twofold. First, the principle of action loses its generality or universality since now applying only to those few cases to which it applies, *i.e.* it would not be "the thing to do" in the circumstances generally but only what some particular person or persons would do. Hence it seems more like a dispositional statement about the persons in question. Secondly, as the last comment indicates, the principle of action ceases to be

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[38] *Ibid.*, p. 132.

an appraisal or value judgment about what is rational in a given set of circumstances. Instead, it is transformed into a descriptive or predictive empirical statement about how some person or group will act in a given situation.

Dray states that "the connxion between a principle of action and the 'cases' falling under it is thus intentionally and peculiarly loose."[39] Perhaps so, but only because according to this dilemma his principles of action are either appraisals which are not susceptible to empirical control but are viciously subjective, or else they are not universalizable and not appraisals but limited dispositional descriptive statements which are falsifiable. And since the second alternative would be compatible with the covering law model, Dray seems led to the first in order to differentiate his model (A) from the covering law model and to defend SU.

Let me illustrate with a common garden-variety example offered by one of Dray's defenders, Kai Nielsen. "In 'Mrs. Finkbine had an abortion because she had good grounds for believing her baby would be deformed,' her warrant [for the explanatory force] for her action might have been 'Don't bear and rear deformed children'"[40] Put into the form of his model it appears as:

(A_1) (a) Mrs. Finkbine believed on good grounds that her baby would be deformed.

(b) Whenever there are good grounds for believing that a child will be born deformed, the thing to do is not to bear and rear the child but to have an abortion.

(c) Therefore Mrs. Finkbine had an abortion.

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39 Ibid., p. 133.

40 K. Nielsen, "Rational Explanations in History," in S. Hook, op. cit., p. 308.

Now, when such examples are spelled out, it appears, first of all, that the rational model (A_1) offers not so much the explanation of an action as the solution of an ethical problem. We seem puzzled why the conclusion (c) is descriptive rather than a prescriptive statement. We should expect from premises (a) and (b) the conclusion (c_1) to follow, viz. "Mrs. Finkbine ought to have an abortion," or "The thing for Mrs. Finkbine to do is to have an abortion." But when (c) is offered as the conclusion we begin to see more clearly some of the confusions surrounding what Dray is after, what he means by saying that rational explanations attempt to make sense of a person's action or to "understand why such a person should do such a thing," without trying to predict what he did. Surely from (a) and (b) we could not predict (c). So the question of importance is "In what sense do (a) and (b) explain (c)?" What, in particular, is the role and defense of principles of action such as (b)? We have already seen the dilemma to which Dray's use of "principles" leads him. Much of the difficulty turns on the ambiguity of "rational action" and "justification" implicit in the following passage.

> In the ordinary course of affairs, a demand for explanation is often recognized to be at the same time a challenge to the agent to produce either justification or excuse for what was done. In history, too, I want to argue, it will often be found impossible to bring out the point of what is offered as explanation unless the over-lapping of these notions, when it is human actions we are interested in, is explicitly recognized.[41]

Dray here, and Nielsen in the above example, assume that an action, when provided a warrant or rationale such as (b), qualifies as a

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[41] Dray, Laws and Explanation in History, p. 124.

rational action. But obviously we must distinguish between a rational act in the sense of a reasonable or acceptable action and in the sense of an act merely done for a reason (reasonable or not). And also between the justification of an act in the same two senses. Accordingly, three corresponding interpretations emerge in addition to that offered above in the example of Mrs. Finkbine. One stresses the ethical aspect of a reasonable and acceptable action, hence would really be an evaluative argument of the following kind:

(A_2) (a) Mrs. Finkbine believed on good grounds that her baby would be deformed

(b) Whenever there are good grounds for believing that a child will be born deformed, the thing *to do* is to have an abortion.

(c_1) Therefore Mrs. Finkbine *ought* to have an abortion

Another interpretation stresses, instead, the purely descriptive aspects of an act actually based on some reason, without appraising that reason, hence would take a different form:

(A_3) (a) Mrs. Finkbine believed on good grounds that her baby would be deformed.

(b_1) Whenever there are good grounds for believing that a child will be born deformed, Mrs. Finkbine *believes* that the thing to do is to have an abortion.

(c) Therefore Mrs. Finkbine had an abortion.

As one can readily see, the only differences in the three formulations are, first, that (A_2) contains an ethical conclusion (c_1) in place of (c) making it an ethical argument and committing its author to the truth or moral acceptability of the warranting principle (b). Since Dray clearly intends not to commit the historian or any other author of rational explanations to such a genuinely ethical

appraisal, which would be hazardous indeed, he would surely reject (A_2). But when we recall the other horn of our dilemma, derived from Dray's insistence upon the empirically falsifiable aspect of (b), it seems that he must reject (b) as well as (c_1), in which case he is left with model (A_3) which differs from (A_1) only by containing (b_1) in place of (b). For, you will recall, Dray wants to keep the explanatory value of principles like (b) even in cases where most people agreed it was not the thing to do, so long as there was some one case, say with Mrs. Finkbine, where it was believed to be the thing to do. In this situation the principle loses its universality and its appraisal quality, since limited to one case (or a few) and merely describes the agent's appraisal or belief. Nonetheless, this alternative frees the historian from any commitment to a principle of action, while still allowing him to explain an action as appropriate from the agent's point of view.

But it now begins to appear, if our appraisal of Dray's dilemma is correct, and if beliefs can be interpreted at least partially as dispositions, that reason explanations are perhaps dispositional in nature and have a form similar to (A_3). Especially since what is described by (c) seems related to what is described by (b_1) as an instance or manifestation. In this case they would be similar to Ryle's general analysis of "mental conduct concepts," motive explanations and "lawlike" statements in The Concept of Mind, and hence would still need modification to accord with the covering law model.

However, I do not think Dray would be happy with any of the alternatives we have so far suggested, i.e. (A_1), (A_2) or (A_3). Despite

the dilemma mentioned above Dray clearly wants to keep premises like (a) and (b), and hence would reject (A_3). But he would also reject both (c) and (c_1) as the proper conclusions. Since he is neither offering an ethical argument nor willing to countenance the structural symmetry of rational explanation and prediction, he more than likely would prefer a conclusion something like (c_2), "Having an abortion would be rationally possible or seem 'all right' to Mrs. Finkbine," and hence an overall argument such as:

(A_4) (a) Mrs. Finkbine believed on good grounds that her baby would be deformed.

(b) Whenever there are good grounds for believing that a child will be deformed, the thing to do is to have an abortion.

(c_2) Therefore having an abortion would be <u>rationally possible</u> for Mrs. Finkbine.

The main reason for rendering his analysis thusly turns on his persistent denial that rational explanations allow of predictions or proof that the action did in fact occur.

Let us now turn our attention to some of the objections brought against Dray's rational model of explanation. Our critique will lead us to consider some of the alternative models mentioned above, which serve to introduce Hempel's covering law, dispositional account of reason explanations. This, finally, will raise our two major inquiries: how reason explanations of purposive actions can be fit into the covering law model, and whether pragmatic appraisals or value judgments are essential in such a reconstruction.

Critique of the Rational Model

In this section we consider some objections raised against Dray's defense of the SU thesis, against his rational model of explanation and his attack on the CL model of Hempel and Popper. This critique will help us to see how, and with what modifications, the CL theory can account for explanations of historical actions and perhaps overcome some of the deficiencies of Dray's model. Having already alluded to some of these criticisms earlier in this chapter, discussion here is limited to five major points. But to facilitate and clarify the discussion, let me first introduce an illustration drawn from a recent symposium on related topics.

Consider, as an example of an historical explanation, Professor Gershoy's biographical account of a pivotal moment in the revolutionary career of Bertrand Barere, a little-known figure during the French Revolution. The action to be explained is why Barere, in the Thermidor crisis of July, 1794, alligned himself with a loose and unsavory coalition of anti-Robespierrists in what developed as a successful rebellion against the Incorruptible, even though he had earlier publicly praised Robespierre as "a great republican." The commonly accepted explanation of Barere's action held that he joined the plotters at the last moment, not because he shared their views, but only because of expediency since he realized they would be successful. This usual view, espoused first by Lord Macauley, connects the action with the reason by the more general interpretation of Barere as "a cowardly opportunist and a trimmer sucked into a fierce power struggle in which he displayed a skill amounting to genius in jumping unerringly on the bandwagon of the stronger." However, Gershoy, suspicious of this intemperate characterization yet convinced that such actions must be "interpreted and explained both in the context of his personality and the circumstances," offers a conflicting explanation of Barere as a moderate mediator forced to a decision of how best to serve the desirable objectives of the Revolution. "Before he took his final stand," Gershoy claims:

> Barere had tried to mediate a bitter dispute within the Committee of Public Safety in order to maintain its unity and effectiveness of action. He had also endeavored to ward off attacks that other avowed enemies of Robespierre outside the Committee were making on him. Reluctantly, because he convinced himself, little by little, that breaking Robespierre's hold over the Committee was more desirable than having the Incorruptible dominate it, he opted for the former. Once he made his decision, he joined with the opposition and played an active and important part in Robespierre's downfall.[42]

Now, to get the logic of the case clear let me formulate Gershoy's explanation, according to Dray's model (A), in the following way:

(1) (a) At time T Barere was (C_1) a moderate mediator who believed (C_2) that his attempt to mediate the dispute between Robespierre and other members of the Committee of Public Safety was futile, (C_3) that the Committee's unity and effectiveness of action were desirable goals, (C_4) that Robespierre had a strong hold on the Committee, resulting in unity but also in domination, (C_5) that breaking this dominating hold would produce more unity and effectiveness of action in the Committee, and (C_6) that actively joining the

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[42] L. Gershoy, "Some Problems of a Working Historian," in S. Hook, <u>op</u>. <u>cit</u>., p. 66.

opposition would break Robespierre's domination of the Committee. (Let us refer to (C_1) - (C_6) as (C_m).)

(b) In a situation of type (C_m), the appropriate thing to do is X, to actively join the opposition.

(c) Therefore after T Barere did X, i.e., he actively joined the opposition.

And let me also, for comparative purposes, formulate the commonly held explanation, suggested by Macauley, in the following sketchy way.

(2) (a) At time T Barere was (C_7) a cowardly opportunist who believed (C_8) that the only important goal was safety for himself, (C_9) that the plotters would be successful, and (C_{10}) that his safety was most reasonably assured by actively joining the opposition to Robespierre. (Let us refer to (C_7) - (C_{10}) as (C_o).)

(b) In a situation of type (C_o), the appropriate thing to do is X, to actively join the opposition.

(c) Therefore after T Barere did X, i.e., he actively joined the opposition.

Now it must be remarked, first of all, that Dray's case is at least incomplete in the sense that while he emphasizes the need to show that an act was "the thing to have done for the reasons given," that it was "the appropriate thing to do" in the situation, he nevertheless offers no normative criteria of appropriateness or rationality. His main thesis, of course, does not require that he do so. Yet in the absence of any consideration of what rationality might mean in the context of historical actions or even generally, Dray is led to an over-simplified and dubious twofold assumption. He misleadingly assumes both that there is some one clear and unequivocal sense in which an action, in a given set of circumstances,

is the appropriate or rational thing to do, and also that there is exactly one course of action in the given situation that is appropriate or rational or the best way of achieving a goal in this sense.

In cases (1) and (2), e.g., there is obviously not even an implicit criterion of appropriateness indicated by the historian. And this makes it appear, misleadingly, both that historians and others are agreed as to what "appropriate thing to do" means in situations like (C_m) and (C_o), and that in such situations there really is only one appropriate thing to do, viz., X. Surely, however, in situations like (C_o) it might be just as appropriate not merely to join the opposition but to lead it, so as eventually to replace Robespierre. And, more generally, as Hempel indicates in a brief review of recent developments in the mathematical theory of decisions, these assumptions clearly do not hold.

> For, first, even when the decision situation is of a kind for which one definite criterion of rational choice may be assumed to be available and agreed upon--e.g., the principle of maximizing expected utility--then that criterion may qualify several different courses of action as equally rational. Secondly, there are various kinds of decisions...for which there is not even agreement on a criterion of rationality, where maximin opposes maximax, and both are opposed by various alternative rules.[43]

These various conflicting criteria of rationality, it should also be noted, reflect not merely differences in the evaluation of the goals available, but rather different inductive attitudes

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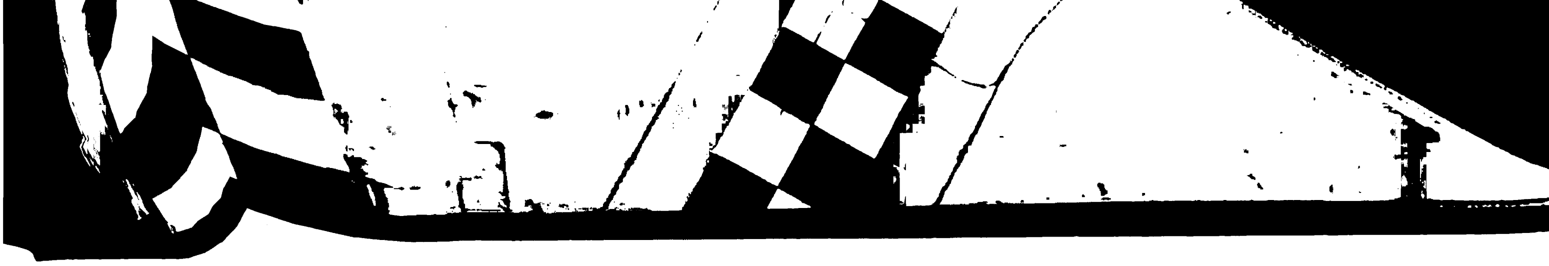
[43] Hempel, "Rational Action," Proceedings and Addresses of the American Philosophical Association, Vol. XXXV (Yellow Springs, Ohio: Antioch Press, 1962), p. 10.



toward the world, attitudes for example of optimism or pessimism, of venturesomeness or caution. We will have occasion to elaborate in more detail upon this point in our last chapter since the notion of "inductive attitude" bears heavily on Hempel's probabilistic model (P) and on his defense of Weber's value-neutrality thesis. It suffices for the present simply to suggest that Dray's account of rational explanation needs a good deal of bolstering from recent developments in decision theory, in order to avoid any rationalist myth of some one thing to do in any given circumstances as rationally necessitated.

A second objection relates to the fact that in many historical actions there is no conscious deliberation or rational calculation leading to the agent's decision. As a result, Nowell-Smith argues,[44] any rational explanation in Dray's sense would be falsified in cases where the agent was found not to go through the relevant calculation. As a result, Dray's model is overly intellectualistic by making human actions appear more rational than they are. Dray concedes that not all actions are performed deliberately in the sense required by his model. Yet he resists the temptation to say that in such cases there is no calculation to be reconstructed by the historian. He contends that "in so far as we say an action is purposive at all, no matter at what level of conscious deliberation, there is a calculation which could be constructed for it: the one the agent would have gone through if he had had

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[44] Nowell-Smith, "Review," <u>Philosophy</u> (April, 1959), pp. 170-2.

time, if he had not seen what to do in a flash...etc. And it is by eliciting some such calculation that we explain the action."[45]

Doubtless there is a calculation that could be constructed. But the point at issue concerns the explanatory significance of such a fictitious set of reasons or rationale. The question here is not even the more complicated one, indicated in an earlier section and to be considered later, of whether or not the agent's reason was causally operative in the action. Instead, it is how or in what sense a fictitious calculation can be said to explain an action. This seems dubious at best, since if the agent did not calculate his decision, then considerations of rationality or appropriateness clearly were of no force in his decision. To explain his action by reconstructing what he might have, but didn't, calculate clearly runs the danger of over-intellectualization. One could not on this basis, for instance, distinguish between a deliberate and nondeliberate action. A calculation could in all cases be supplied: in deliberate and nondeliberate cases alike. It is all too easy to construct a pattern where there is none and hence to distort rationalistically an historical agent's actions.

Dray[46] and Nielsen[47] reply to this line of criticism by admitting that such rationalistic distortions can, and do, creep

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[45] Dray, Laws and Explanation in History, p. 123.

[46] Dray, "The Historical Explanation of Actions Reconsidered," p. 111.

[47] Nielsen, op. cit., p. 310.

into narrative history written on this model, yet they insist that this difficulty pertains only to the particular content of an explanatory argument and in no way affects the correct form of a rational explanation. His account is a philosophic explication, Dray insists, of what makes an action understandable rationally. And this understanding results not just from a set of propositions which an agent consciously calculated or recited to himself, but from our perception of a rational connection between an action and the agent's beliefs and motives, from the ordering or reconstruction of such ingredients in the form of a practical calculation.

This reply, however, fails to recognize that the objection does relate to the form of rational explanations and not just to their content. It is a question of whether goals, reasons or principles can be invoked in such cases at all, whatever these reasons might be; not a question of distorting or mistaking the particular goals or principles, and whether or not there are empirical checks on possible mistakes. The point remains that our perception of such a logical connection between explanans and explanandum cannot explain why an agent committed the explanandum-action in cases when the agent took no account of such a connection. Moreover, this objection is not adequately countered by E. Barker's suggestion that actions of "historical significance" are often undertaken by an agent with "an awareness that he may have to explain his conduct to some audience."[48] For, in this

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[48] E. Barker, "Rational Explanations in History," in S. Hook, op. cit., p. 183.

case, the agent may have acted with the goal of satisfying some audience, as when a Congressman votes for the federal allocation of funds for his hometown power plant, in which case this was his reason. That is, he had a reason, and hence the objection is no longer applicable. But if the historical agent had no reason, "historical significance" of his act notwithstanding, no constructed reason can explain his action.

In fact, this kind of a defense of Dray's model points to a related difficulty. For with historically significant actions the historian, no less than the historical agent, is likely to produce by his construction of the reasons mere rationalization. As Passmore observes, the explanations an individual gives of his own conduct to a public or private audience are often "hollow-sounding... as if they were constructed to satisfy our audiences rather than as explanations of our action."[49] There seems little doubt, *e.g.*, that were Barere to explain his own action X to some public audience at the time, he would obviously explain it in terms of (1) rather than of (2), even if (2) were the correct explanation. In a sense, however, Dray's reply does successfully counter this difficulty, since this is a question of content, of the hazards of any search for a particular reasons. Surely Dray's model does not imply that whenever we think we know an agent's reasons we actually do know them. To uncover his "real," as opposed to "good," reasons is indeed a difficult task. But it is not peculiar to Dray's proposal

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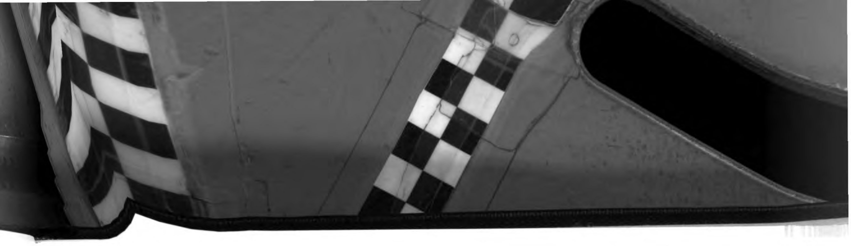
[49] J. Passmore, "Review," Australian Journal of Politics and History, (1958). pp. 269 ff.



which, as he claims, concerns only the form not the content of rational explanations.

Despite the seriousness of Nowell-Smith's objection, one qualification seems worthy of comment. For some actions that are decided upon "in a flash" and without conscious calculation or deliberation seem similar to actions that are deliberated upon.[50] Consider for example the many complex maneuvers involved in performing a tonsillectomy. No doubt at first a doctor learns to perform such an operation only by "painful" calculation and deliberation, but eventually the appropriate procedure becomes automatic and he can perform it "in a flash," routinely, with no conscious thought of the complex maneuvers involved.

Perhaps this is what E. Barker intended in the example of an historical agent being aware of the possibility of having to explain his action to an audience. Consider here the skilled politician who has learned how to "explain" his actions depending on the audience he is addressing, or the one who has learned how (by careful calculation at first but later by "instinct") to defend his conflicting actions to bothersome newsmen or Committee members.

In such cases as these the habit pattern thus acquired could be interpreted not as fictitious but as "consisting in a set of dispositions to react in certain appropriate ways in various situations." However, following Hempel's suggestion, "a particular act of this kind might then be explained, not by a recon-

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[50] Hempel, "Reasons and Covering Laws in Historical Explanations," in S. Hook, op. cit., p. 160.

structed calculation or deliberation which the agent did not in fact perform, ... but by presenting it as a manifestation of a general behavior pattern that the agent had learned."[51] But in this case the appropriate action can be explained by appeal to dispositions, such as cowardice or moderation, and perhaps to covering laws in accord with the CL model. This, of course, indicates the direction of Hempel's alternative version of "rational explanations" to be considered in the next chapter.

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[51] Ibid., p. 160

Further Objections to Dray's Rational Model

The third objection turns on the ambiguity mentioned earlier in this chapter, the ambiguity of "rational action," which makes it unclear whether Dray opts for model (A_1), (A_2), (A_3), or (A_4), i.e. to what extent Dray is proposing an explanatory model in the form of an ethical solution or an evaluative argument. Dray offers an important contribution in his reaffirmation of the claim that understanding an historical action often depends upon our discovering the agent's reasons or rationale for acting, as in examples (1) and (2). In the sense of being done for some reason, an action can be considered "rational." But this is a long way from, and surely does not entail, appraising the normative rationality of the act in the sense of endorsing the agent's reasons as "good reasons" or the action as "the thing to do." Strawson, for instance, objects that a view "which goes as far as this makes history impossible."[52] Surely no reputable historian would want, *qua* historian, to appraise abortion or joining the opposition to Robespierre as the rational or moral thing to do as such.

Now Dray does not want to go this far. He refuses to commit the historian to the truth or moral acceptability of a principle of action in the way that an argument like (A_2) with an ethical conclusion would do. He stresses the fact that the agent's reasons must be good ones only "from the agent's point of view," and that "the appropriateness of his act is to be assessed only in relation

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[52] Strawson, "Review," *Mind* (April, 1959), p. 268.

to the circumstances as he envisaged them."[53] But even this is not sufficient, for it contains a third meaning of "rational action." It still commits the historian to the truth of a normative principle of action of the form (b), "when in a situation of type C, the thing to do is X." Though now the principle is relative to the agent's beliefs and goals. But, Strawson persists, this does not go far enough since there are "differences of intelligence, temperament, ability and character"[54] as well as of goals and beliefs, and human actions involve faulty judgment as well as faulty information.

Dray's reply to this objection is, I must confess, baffling. He retreats by removing the sting from his earlier attack on the CL model. He claimed in Laws and Explanation in History, you will recall, that the CL model was "peculiarly inept" in accounting for typical historical explanations and that the latter required a peculiarly different kind of "logic." In his latest account of the rational model, he concedes that his model with its accompanying criterion of understanding and appraisal of the thing to do "cannot be the only one, even in history."[55] This reopens the possibility that the CL model might also be applicable in history, though perhaps in a different context.

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[53] Dray, "The Historical Explanation of Actions Reconsidered," p. 112; and Laws and Explanation in History, p. 126.

[54] Strawson, op. cit., p. 268.

[55] Dray, The Historical Explanation of Actions Reconsidered," p. 113.

The concession occurs as part of his reply to Strawson's objection. He agrees, first, that human actions can fall short of an ideal of rationality because of faulty judgment. And in light of his earlier persistent belief that non-deliberate actions could still have a rationale constructed for them, one would expect him to claim in this case that actions falling short of an ideal of rationality in this sense could also be explained in accord with his model. For, it seems we can understand the reasons or rationale of a faulty inference or judgment, as well as of a false belief, without of course endorsing it as a sound inference, but only as the appropriate one from the agent's view of the circumstances.

However, instead of continuing this use of "rational action" which allows one to understand the rationale of an act without endorsing it as completely rational, Dray switches meanings. He thus limits the applicability of his rational model, concerning inferences, to only those which are valid. "For it is obvious," he asserts, "that we cannot claim rational understanding of the making of a logical error.... And one cannot re-think a practical argument one knows to be invalid." In other words, his claim for the criterion of rational appropriateness is limited to "actions not judged to be defective in various ways."[56] But such statements clearly suggest that "rational action" is now being used in such a way that an historian or logician would have to endorse the agent's inference as rationally sound in order to understand its rationale.

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[56]*Ibid.*, p. 113.

I find Dray's retreat extremely baffling. But apparently no more than do some of his own defenders. Scriven, e.g., quite well recognizes that the soundness of an inference is clearly not necessary in order to understand the agent's rationale in making the inference. Surely "if the historian can play the role of judging reasonableness with respect to beliefs he knows to be false, he can equally well do it with respect to inferences which he knows are logically unsound."[57] Doubtless most logic professors can, sometimes at least, "rethink" a student's logic exercise which one knows to be invalid. Just as one can also "rethink" one's own bad arguments in order to improve them. Scriven consequently suggests, in a more consistent manner than Dray, that all reason-explanations are essentially similar. And the way in which they are similar reverts back to Dray's earlier analysis, since "we may understand why X did Z, namely because he thought it would achieve D, without at all thinking his judgment was defensible."[58]

In spite of Scriven's consistent defense of the rational model, however, his proposal fails to eliminate a fourth difficulty often raised against Dray's model. In fact, his interpretation of a rational action as one the agent did in order to achieve some goal, D, seems to vary considerably from Dray's.

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[57] Scriven, "New Issues in the Logic of Explanation," p. 349.

[58] Ibid., pp. 349-50.

It contains no appraisal or principle of action corresponding to (b) in (1) and (2) or (A_4) above. Instead, Scriven appeals to a descriptive premise of the form (b_1), "when in a situation of type C, the agent believes the thing to do is X," in order to avoid the kind of objection brought forth by Stawson, viz that understanding an agent's reasons for acting is different from and not dependent upon appraising the rationality of the action. In so doing he gives up (A_4) in favor of (A_3) as the most adequate defense of the rational model of explanation. But if this is the case, Scriven would apparently agree, at least in part as shown in the next chapter, with Hempel's major objection to Dray's model. Let us consider this fourth line of criticism in some detail since it elicits the essentials of the issue and prepares the way for Hempel's dispositional analysis of reason-explanations.

In chapter two we described Hempel's two ideal models of explanation and a number of requirements which he considers to be necessary though not sufficient conditions for the soundness or adequacy of any explanation of a given event. One of the most important conditions stipulated that "any rationally acceptable answer to a question of the type 'why did X occur?' must provide information which constitutes good grounds for the belief that X did in fact occur."[59] And this, for CL theorists, means either deductively or inductively inferring the actual occurrence of the

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[59] Hempel, "Reasons and Covering Laws in Historical Explanation," p. 146.

event from a set of laws and antecedent conditions in accord with either model (D) or (P).

Now Dray's rational model, read as either of the form (A_1) or (A_4), differs from the CL account in two ways: by replacing the laws with a generalized normative principle of action, and consequently by loosening the connecting link between explanans and explanandum from deductive or inductive implication to something like "rational necessity or coherence." The main objection to Dray's model, then, is that to the extent it differs in these two features from the CL model, to that extent it fails to satisfy the condition of adequacy just mentioned. Hence it cannot explain its explanandum-event as described in (c), say, why "Barere actively joined the opposition."

Moreover, in order to satisfy this condition, to provide "good grounds" for the explanandum (c), a rational explanation would have to replace the principle of action (b) with both a descriptive generalization about what a rational agent might do in the circumstances, and a statement that the agent was rational at T. In this case it would cease to be an appraisal and would become a CL explanation. For example, in case (1) we would have to replace (b) with the following two descriptive statements: (b_2) "Barere was a rational agent at T," and (b_3) " rational agent, in a situation like (Cm), will with high probability do X." Thus if rational explanations are to explain adequately why some act occurred, they must be reinforced by a probabilistic covering law, the truth of which the historian is then committed to. And if such descriptive generalizations are missing from an historian's account of an action, the account is best viewed as enthymematic, as an explanatory-sketch or perhaps a partial explanation. But in any event not as a complete explanation of its own kind.

Of course, information of the kind suggested by (a) and (b) of model (A) - that an agent was in a given situation and that in such a situation X is the appropriate or rational thing for the agent to do - does provide ample grounds for believing a conclusion such as (c_2), that it would have been appropriate or rational for Barere to actively join the opposition forces. But this information clearly does not afford good grounds for conclusion (c) of (A), that Barere actually did X. The historian needs to discover, in other words, the agent's actual reasons, not merely what good reasons there might be, for acting as he did. And this requires-- as Hempel, Passmore, Nagel, Scriven and Nowell-Smith all agree-- showing more than that the action made good sense from the agent's point of view. It requires showing "that it was <u>because</u> it made good sense that he did it: otherwise it isn't an explanation. It may have made good sense from his point of view, but he may have done it out of spite...."[60]

We might note here that even Gershoy interprets the "logic" of his inquiry to be "not fundamentally different from that of the scientist", since his explanation follows the basic requirements of the CL model by relying on "singular statements" and "empirical regularities" which "could be tested like any scientific

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[60] Scriven, "New Issues in the Logic of Explanation," p.340

explanation for accuracy, cogency and logic."[61] However, he nowhere offers any evidence to justify his own explanation, (1), or why he takes it to be better than the commonly accepted one, (2). And without such evidential support it would fail to satisfy Hempel's epistemic condition of adequacy (being true or highly confirmed). Hence it would be at best a potential explanation, as discussed in chapter two. For Barere might well have had both dispositions. He might have been a cowardly opportunist and a moderate mediator at the same time, without any incompatibility.

Moreover, either disposition is often manifested in behavior similar to Barere's actual action X, and either set of reasons or calculations, (Cm) or (Co), are rationally possible and "make sense" of his action. Thus, citing either set of reasons, say (Cm), by itself will not sufficie to explain adequately why Barere did X. Gershoy can only establish this claim, as Nagel convincingly argues,[62] by showing not merely that Barere could rationally have acted as a mediator, but that this disposition actually did operate as a causal determinant in his actual action X. It may have made good sense from his point of view, yet he might still have done it for opportunistic reasons. Gershoy must show that Barere did X because his mediator-calculation made sense.

But to show this is to show that the agent was acting on reason at the time, and hence to appeal to some generalization

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61 Gershoy, "Some Problems of a Working Historian," p. 67.

62 Nagel, "Relativism and Some Problems of Working Historians," in S. Hook, op. cit., p. 89.

showing that an agent with such actual and good reasons will usually act as this agent did. Only in this way can we provide good grounds for believing that the agent's good reasons influenced his action or were causally operative. Thus, the basic logical defect in Dray's rational model: while it helps to render an agent's action intelligible by showing how he could plausibly have done it and even how he would have done it if he were rational and acted accordingly, it nevertheless does not explain why he in fact did act as he did. The good reasons still need to be linked to the explanandum-action by descriptive covering laws, not by normative principles of action.

Despite this apparently decisive criticism, Dray and Nielsen persist in denying its force, though for different reasons. We will pursue each case in turn. It is clear, of course, that to rebut this criticism one must argue either that Hempel's condition of adequacy is not a necessary condition of sound explanations of why some event or action did in fact occur; or that the rational model offered by Dray is not intended to codify this kind of explanation, and hence is more properly construed as, say, of the form (A_4) instead of (A_1). In this case the conclusion of (1) and (2) would be (c_1)"Therefore actively joining the opposition would be rationally possible for Barere." Since the first alternative does seem rather futile, both Dray and Nielsen resort to the second. That is, they in effect surrender most of the charges against the CL model, in order to preserve the SU thesis in the sense of a special kind of explanation which requires

an appraisal element but no empirical covering laws. Since Dray's argument is two-pronged, the first part of which Nielsen finds defective and the second part of which he develops in more detail, let us consider briefly each part in turn.

Dray's first argument turns on two premises. First, he claims that one means by such statements as "He did X because he thought R" something logically parallel to what one means by, "I did X because I thought R". Secondly, in the latter self-applied case we can distinguish, he maintains, between reasons which would justify our action and our actual reasons for so acting, without knowing any laws from which we can infer, deductively or inductively, such knowledge. If these assumptions are acceptable, it follows that we can also know the actual reasons of other agents even though we know no appropriate generalizations about how all or most people would behave in such circumstances. However, Hempel challenges the second premise and indicates that it too can be accounted for on a dispositional model.

Hempel correctly concedes that the two cases, knowing our own actual reasons and knowing those of others, are logically on a par. But he denies that explaining one's own actions by reference to the actual reasons for which they were done can be accomplished except on assumptions like (b_2) and (b_3), i.e. generalizations and statements of dispositions to act rationally in the given situation. In other words, unlike Dray, he sees both cases as amenable to treatment in accordance with the CL model when viewed something like (A_3), i.e. dispositionally. Further, although explanation and justification are often inextricably fused in self-applied cases, we do nonetheless usually distinguish in such cases between a mere rationalization, say (2), and a genuine explanation, e.g. (1). Such a distinction is surely no more difficult on this analysis than on Dray's. Since we will pursue Hempel's dispositional analysis of rationality in more detail in the next chapter, let us turn to Dray's second front of defense.

Dray opts for the second alternative mentioned above by simply denying that his rational model was ever intended to be an explication of historical explanations of why an agent actually did a given act, or even why they did the rational thing, i.e. why they acted in accord with the good reasons they had. Instead, he merely intended to explicate the form of explanations showing that the thing done was appropriate or rational for a person so situated. Thus, he feels unscathed by Hempel's objection, since these are simply two distinct kinds or senses of explanation, and rational explanation "can be complete of its kind or at its own level,"[63] not just an explanatory-sketch requiring covering laws to be filled in. In this Dray concurs with the mediating view of Nowell-Smith that "ultimately the dispute may amount to no more than a trivial verbal dispute about the meaning of 'explain'",[64] that in some contexts one concept of explanation might be appli-

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[63]Dray, "The Historical Explanation of Actions Reconsidered," p. 115.

[64]Nowell-Smith, op. cit., p. 162

cable while in other contexts another might be. Nielsen also agrees with the mediators, since this view seems to establish the frequent ordinary usage of Dray's concept of rational explanation in a way that does not presuppose its conjunction with a covering law. This is enough, he believes, for Dray's central thesis.

But it seems highly unlikely that a philosophic issue that has generated so much argumentation is really resolvable as a "trivial verbal dispute". It seems much more likely to me that this grossly over-simplifies the matter. To rest the issue at this point, as Dray and Nielsen desire since it at least "ropes off" a proper domain for rational explanations of form (A_4), would be most unfortunate. It still leaves unclarified a number of important aspects of the issue. And these bring us to our final criticism of Dray's model.

For one thing, Dray recognizes the philosophic task to be one of explication or "rational reconstruction," rather than mere description, of ordinary and historical explanatory practice. Explication as discussed in Chapter II, may of course properly deviate from such practice. His complaint, accordingly, unlike that of Nielsen and others, is that the CL model is "the wrong <u>kind</u> of reconstruction,"[65] that it makes "a claim in the wrong 'universe of discourse' for the answering of typical 'why' questions in history."[66] That is, it does "not coincide conceptually" with

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[65] Dray, "The Historical Explanation of Actions Reconsidered," p. 108

[66] <u>Ibid</u>., p. 130

what historians usually mean when they explain human actions; it lacks "sensitivity to the concept of explanation historians normally employ" by forcing historical explanations onto the Procrustean bed of a preconceived general schema. As aresult the CL model is, he claims, simply an inadequate explication of historical practice.

Now, such charges suggest that Dray has some clear idea as to what constitutes an adequate codification of ordinary historical explanatory practice, that he has some way of determining just what concept of explanation historians do normally employ, that there is in fact one "normally employed" or "usually meant" concept of explanation in historical practice, and finally that his model adequately codifies it. But these suggestions surely need a great deal more analysis than Dray gives them.

More importantly, it is still not clear how Dray's reconstruction of the SU thesis or the "empathy" position avoids the Nagel-Abel-Hempel criticism of Verstehen or Dray's "seeing the appropriateness of an action." Neither seems necessary or sufficient for rationally acceptable explanations of human actions. They seem at best heuristic devices for discovering possible explanatory hypotheses, or possible reasons for action, such as (Cm) or (Co). For when interpreted as (A_3), rational explanations only make sense of an agent's action by showing that it might have been done for some appropriate or good reasons, that some possible reasons were available to the agent, regardless of whether or not he actually did act on such reasons. The impor-

tant questions seem to be, as with _Verstehen_: of what significance is such a model? Is this model autonomous and complete of its kind or can it be explicated as a sub-part of the CL theory? Is the CL explication of rational explanations acceptable? And finally, has Dray successfully routed the value-neutrality thesis?

I submit that Dray's model is drastically over-simplified, that when elaborated it is better analyzed as a sketch needing completion, and that our analysis of explication reveals why this is the case. For what purpose, we might ask, might an historian choose to uncover the appropriateness or rationality of an agent's action, _e.g._ of Louis' calculation to decide whether or not to withdraw military pressure from Holland, or Barere's decision to join the opposition to Robespierre? Would this information be significant to him in itself or only because it might be useful for other wider purposes? Would the historian find it of historical importance, _e.g._, even if other information indicated that Barere was not disposed to act rationally at the given time (_i.e._ if Barere was known to be under severe emotional strain, the influence of drugs or even extremely fatigued)? It seems to me that he would not, that he would display the customary historian's bias against such counterfactual conditionals. But in this case not without good reason.

Even Dray admits that his model fails to apply to "defective" action. But, as one historian comments, "most historians today find their problems precisely in the gaps or flaws of rationality perpetrated by historical agents."[67] Moreover, if Dray's model applies only to non-defective or rational cases, it would seem to presuppose some assumption to this effect, e.g.(b_3), that Barere was disposed to act rationally at the time, in accord with the standard of rationality invoked, and that circumstances did not prevent him from so acting. In fact, Dray at one point even concedes that in history there operates as a "standing presumption" the "general belief that people act for sufficient reason."[68] However trivial it may be, the important question is whether it is logically required or presupposed in Dray's model of rational explanation.

It or some more restricted law is required, if Dray's model is to be of use to the historian. Otherwise the historian engages in sheer speculation about how an act might have made sense or how the agent had to act lest he have no reason, since its sense is no longer applicable to why the action was actually performed. The fact remains that he might have had no reason. We are left with, say, Gershoy's potential explanation, or even Macauley's explanation of Barere's action as due to his opportunistic calculation.

This latter case is a particularly good example. For Macauley's "natural" or "familiar" rationale of his action makes it highly in-

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[67] Krieger, op. cit., p. 137.

[68] Dray, "The Historical Explanation of Actions Reconsidered," p. 115

telligible why Barere might have so acted. And no doubt with the customary paucity of historical evidence, an historian, e.g. Macauley, might readily grasp at the apparent opportunism-theme as a plausible sketch of an explanation. It is this aspect that Dray wants to isolate from the context of historical practice and codify as a peculiarly different kind of explanation, complete of its own kind.

But herein lies the very danger to which many CL theorists point. For, having abstracted this aspect of an historian's work from the wider context of his explanatory practice, Dray seems to imply that the historian may or may not need to proceed to the wider context, to fulfill other purposes, such as explaining why A actually did X. And since the historian's eventual goal is the latter, Dray's proposal has the tendency to lull the historian into the deceptive belief that by showing one plausible rationale of A's action, he has explained why A actually did X, and hence that he need not consider alternative rationales, such as Gershoy's. But this procedure conduces to the practice of exalting easy, familiar or intelligible accounts into actual explanations.

P. K. Feyerabend, in rather harsh terms, deplores such a proposal as conformism and as conducive to taking a subject where pseudo-explanations and non-sequiturs abound and arguing that it has a "logic of its own" according to which it must be judged. "What we need," he suggests, and what Dray's proposal does not provide, "is to improve and to extend our knowledge of the world by improving scientific procedures and eliminating unsatisfactory developments."[69] This can only be accomplished by a normative interpretation of scientific method and explanation.

We must conclude then that Dray's model inadequately explicates actual historical practice, however well it might accurately describe some such practice. In his eagerness to avoid forcing explanations onto some Procrustean bed, he has misrepresented both the purposes of a philosophic explication and of historical explanatory practice. At the very least, his model requires some assumption to the effect that the agent was acting "on reason" or disposed to do so.

We must also note, however, that Dray later retracts his declaration of the presumption of rationality as "incautiously strong",[70] on the grounds that any such presumption does not preclude the occurrence of many cases to the contrary. In history, unlike the physical sciences, there is no assumption that everything is explicable, and hence any such presumption of rationality must be logically different from a covering empirical law.

Unfortunately, he does not explicate just how it differs logically or what its logical status is. But we need not pursue

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[69] P. Feyerabend, "Comments," in Feigl and Maxwell (eds.), *Current Issues in the Philosophy of Science* (N.Y.: Holt, Rinehart and Winston, 1961), p. 279

[70] Dray, "The Historical Explanation of Actions Reconsidered," p. 115.

this point, for of more importance than the universal presumption that all acts are rational is the application of this methodological rule to the particular cases under investigation by the practicing historian. Such cases need merely the presumption that the given agent was disposed to act rationally at the time in question and that circumstances did not prevent his so acting. Only then, it seems, could we be sure that Gershoy, *e.g.*, had reconstructed "Barere's sense" or rationale, and had not just constructed a fictitious one in an irresponsibly speculative manner.

But since in ordinary practice this presumption will normally be taken for granted, it is only when departures from rationality are considered that the historian feels the need to specify disturbing factors. Such an elliptical formulation may indeed, as Dray suggests, be adequate for practical purposes, *i.e.* in its psycho-pragmatic context of explanations, yet it obscures the logic of explanatory argument, and makes it appear to be of a different form and to serve a different purpose. Surely an explication, such as Hempel's, which explicitly formulates what was implicit in the historian's explanation does not force the latter onto a Procrustean bed.[71]

Since Dray's attempt to explain why some action occurred has been effectively met by the above criticisms, we might pause to consider why he so persistently resists the CL proposals. One answer, I think, turns on his belief that if appraisals or value

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[71] *Ibid.*, pp. 132-3.

judgments do not constitute the explanatory force of such arguments, then Weber's positivist insistence on the value-neutrality thesis will go unchallenged, except in an unimportant presystematic sense. Dray, following the earlier idealists, resists the value-neutrality thesis and hopes to establish, as they did not, the essential logical role of pragmatic appraisals in a rational reconstruction of rational explanations. Having failed in this, successive concessions are made, until finally he ends with little more than the earlier idealist insistence on possible explanations and on the importance of humanism, as opposed to science.

This helps to account for his most recent maneuver. "Even if a 'science' of history employing CL explanations were well advanced," he claims, there would still be good reason for keeping explanations of the form (A_4). They satisfy our "humane curiosity: an interest in discovering and imaginatively reconstructing the life of people at other times and places"[72] by viewing them "from the inside," "from the standpoint of agency." Apparently, such explanations are not so important for the science of society, for explaining why men do what they do, but are a part of history as "a branch of the humanities." Hence, Dray's "main complaint" against extending the CL model to history is "that it sets up a kind of <u>conceptual</u> <u>barrier</u> to a humanistically oriented historiography."[73]

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[72] Ibid

[73] Ibid

Instead of pursuing further what Dray means by either a "condeptual barrier" or a "humanistically-oriented historiography," I want to suggest that if the latter involves the making of value-judgments, as I think it does, then his defense of SU and his subsequent charge of the CL theory being a barrier to such an endeavor is based on his acceptance of Hempel's belief that the CL model required the value-neutrality thesis. In the final chapter, after examining Hempel's model of rational explanations, I hope to show to the contrary that the CL model on its probabilistic side actually requires the explainer to make value judgments. Hence, rather than a barrier to the making of such judgments and to a humanistic orientation, the CL theory actually requires them in order to reconstruct rational or other statistical explanations. Consequently, Dray's resistance to the CL model and his dogged clinging to possible explanations, wherein appraisals or behaviour maxims have explanatory force, is not necessary to maintain the importance of a "humanistically-oriented historiography," at least in the minimal sense of including the making of value judgments or appraisals.

CHAPTER V

C. HEMPEL'S VERSION OF RATIONAL EXPLANATION

Rational Explanation and the Covering Law Model

Let us turn now to consider Hempel's alternative explication or theory of rational explanation. We have already seen some of the difficulties attending Dray's attempt to make principles of action serve an explanatory function, especially the problem of explaining why the explanandum-action actually was done. Accordingly, Hempel proposes to replace the evaluative principle of action in, say, (1) by an empirical generalization describing how rational agents act in situations like Cm. To avoid Dray's difficulties concerning fictitious calculations, as well as to offer good grounds for believing Barere actually did X, he explicitly formulates an additional assumption to the effect that Barere was disposed to act rationally at T, i.e., that he was a rational agent disposed to do what was appropriate in the situation. When so modified, the schema and our example take the following form:

(R) (a) At T Barere was in a situation of kind Cm.

(b) At T Barere was a rational agent.

(c) In a situation of the type Cm, a rational agent will do X.

(d) Therefore Barere did X at T.

Not only is the result a CL explanation, but any critical appraisal or evaluation contained within it appears irrelevant to the explanatory force of the argument. The explanans also performs double duty by logically implying the explanandum-action and hence showing it to be both rationally possible and actually done.

That is, Hempel's model seems to accomplish all that Dray's does, without the latter's defects. The only element unaccounted for is Dray's evaluative principle of action. But its absence allows Hempel to preserve his value-neutrality thesis. Our further task, as a result, will be to consider the adequacy of (R) with special emphasis on the value-neutrality issue. We have seen how Hempel successfully meets Dray's challenge to make value judgments serve as the explanatory force of rational explanations. We need now consider how well (R) serves to explicate such explanations, and whether or not fulfillment of this goal can be accomplished without committing the historian to some decision or appraisal. Can an acceptable rational reconstruction of such explanations be achieved without disturbing the serenity of the value-neutrality thesis?

Hempel, of course, believes that it can, since he interprets the notion of rational agent invoked in (R) as a "descriptive-psychological" concept whose normative connotations are irrelevant for the explanatory force of the argument.

> To be sure, normative preconceptions as to how a truly rational person ought to behave may well influence the choice of descriptive criteria for a rational agent--just as the construction of tests providing objective criteria of intelligence...will be influenced by presystematic conceptions and norms. But the descriptive-psychological use of the term 'rational agent'...must then be governed by the objective empirical rules of application that have been adopted, irrespective of whether this or that person...happens to find those objective rules in accord with his own standards of rationality.[1]

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[1] Hempel, "Rational Action," p. 13.

In these terms, we must determine whether this descriptive-psychological concept of rational agent serves the intended explanatory role and whether all normative considerations can be relegated to a "presystematic" position as inessential for the completed reconstruction. To carry out these two tasks we will consider, in this and the next chapter, two important features of Hempel's model (R). One concerns the fact that the generalization, (c), which provides the explanatory force to the argument, will in most cases be at best a probabilistic assertion as to what a rational agent in circumstances (Cm) tends or is likely to do. Such an explanation is not of the deductive form (D) but of form (P), since the explanans offer only high confirmation for the explanandum (d). This point, to be discussed in Chapter VI, raises the normative question in the context not of the logical but of the epistemic condition of adequacy for explanations.

The other relates to the fact that in explaining an action by reference to the agent's rationality and his reasons, thus presenting the action as an instance or manifestation of some general tendency, Hempel construes reason or motive explanations as containing general laws. Yet they are also akin to Ryle's dispositional explanations as characterized in The Concept of Mind under the general rubric of "mental-conduct concepts." "Rationality in the descriptive-psychological sense," Hempel refers to as a "broadly dispositional trait" since to characterize an agent as rational is "to attribute to him, by implication, a complex bundle of dispositions, each of them a tendency to behave in characteristic ways in certain kinds of situation."[2]

It will be helpful to begin by looking at Ryle's analysis of dispositional explanations, since it has influenced more moderate critics of the CL model to deny requirement R_2, that the explanans of an adequate explanation need contain general laws. This issue further clarifies Hempel's dispositional version of rationality and of reason-explanations. We have already noted Gardiner's concern with the looseness of general laws and his reliance on dispositions in an attempt to explain actions as instances of the actor's normal behavior. J. W. N. Watkins, though a general adherent of the CL model, concurs in this view that some historical explanations are "explanations in detail." Unlike "explanation in principle" which embodies fully general laws, they employ as explanans the specific beliefs and goals of actual people.[3] And these dispositions, he believes, following Ryle, are sufficient to explain human actions without further appeal to fully general laws.

Alan Donagan joins Gardiner and Watkins in utilizing Ryle's analysis of dispositions as lawlike statements instead of laws. This enables him to modify the CL model so that it avoids general platitudes about men in general. Instead he concentrates on the

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[2] *Ibid.*, p. 13.

[3] J. Watkins, "Ideal Types and Historical Explanation," in Feigl and Brodbeck (eds.), *Readings in the Philosophy of Science* (New York: Appleton-Century-Crofts, 1953), pp. 733-4.

particular dispositional complex which constitutes the given agent's character, and allows the historian to "know his man."[4] None of these more moderate critics of the CL model, however, deny R_1; they agree that an adequate explanation must take the form of a deductive argument. All would reject any proposed loosening of the deductive bond between explanans and explanandum. They prefer loosening the explanans itself from a strictly universal law to a lawlike dispositional assertion mentioning particular individuals. And all do so by an appeal to Ryle's logical analysis of "mental-conduct concepts," to which we therefore turn.

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[4] A. Donagan, "Explanation in History," pp. 428-43.

Rylean Dispositional Explanations

According to Ryle, the "official doctrine" of mind--which postulates such mental states as conscious thoughts, feelings, beliefs, sensations and desires, and thus constructs a bifurcation between these unwitnessable mental causes and their observable physical effects in order to explain human behavior--constitutes a philosophical myth. He describes it as "the dogma of the Ghost in the Machine," a category mistake of misrepresenting the logical type of the facts of mental life as if they were mental happenings, and somehow mysteriously "in the mind." That motives for action, say, are mental causes or antecedent conditions of actions, Ryle repudiates as an appeal to occult qualities, to metaphysical fictions, and generally as a "logical howler." Hence, since motives or beliefs are not mental events, they cannot be causally connected with actions by general laws. His alternative analysis interprets motives, beliefs and thoughts as dispositions, and the agent's action as a manifestation of the given disposition. In opposition to the above dogma, Ryle believes that "the sense in which we 'explain' /an agent's/ actions is not that we infer to occult causes, but that we subsume under hypothetical and semi-hypothetical propositions." Consequently, such explanations are not of the causal type "the glass broke because a stone hit it," but of the different dispositional type "the glass broke when the stone hit it, because it was brittle."[5]

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[5] G. Ryle, The Concept of Mind (New York: Barnes and Noble, 1949), pp. 50 and 88ff.

There are then, for Ryle, two quite different senses of explaining some given occurrence. We can explain some occurrence by citing some antecedent condition, event or cause of the occurrence, *e.g.*, the stone hitting the glass, and by connecting this event with the occurrence by a covering law. Or we can do so by citing some dispositional property of the glass such as its brittleness. This is to assert some general hypothetical or lawlike proposition about the glass, *i.e.*, to give the "reason" why the glass broke when struck. Explanations of motivated human actions are assimilated to the second type. Accordingly, "he boasted from vanity" is analyzed as "he boasted on meeting the stranger and his doing so satisfies the lawlike proposition that whenever he finds a chance of securing the admiration and envy of others, he does whatever he thinks will produce this admiration and envy."[6] Hence, the logical force of motivation explanations, or in Gardiner's words the "function of the 'because'" in such cases, lies in the deductive subsumption of the particular action under the general pattern of behavior of which it is an instance or a manifestation.

As we saw in the last chapter, there is indeed a difference between causes and reasons, however difficult drawing a clear-cut distinction between them might be. That motives cannot be causes, however, is quite another issue. Moreover, Donagan and Gardiner also argue that while causal explanation does fit the CL deductive

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6 *Ibid.*, p. 89.

model, dispositional explanation "differs from anything recognized in the Hempelian theory, which presupposes that the only way of deriving the statement that certain windows broke from the statement that they were stoned is by the allegedly buried general law 'All windows break when stoned.'"[7] But the grounds of this claim, based on the distinction between covering law statements and lawlike dispositional propositions or "inference licenses," deserve closer scrutiny. For if the distinction withstands criticism, it would serve to undermine the view of CL theorists that historical explanations are not fundamentally different from those in natural science, and that there is a methodological unity in the empirical sciences.

A general law, according to Ryle, is a hypothetical statement which is "open" in the sense of not mentioning any individuals, *i.e.*, a statement of which "the protasis can embody at least one expression like 'any' or 'whenever.'"[8] In its simplest form it would appear as, "If anything is A it is B," *e.g.*, "If anything is a window, it breaks when stoned." Lawlike statements, on the other hand, are "closed" in that they do mention individuals, even though they are also partly hypothetical in what they imply and can be satisfied by a wide range of behavior. For example, "Those windows were brittle" implies that if sharply struck, they would

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[7] Donagan, *op. cit.*, p. 435.

[8] Ryle, *op. cit.*, p. 120.

break or shatter. In mentioning certain individual windows it is closed and of the form, "if these individuals were A they would be B."

Further, Ryle considers both kinds of explanations, causal and dispositional, as complete of their kind though they appear to be enthymemes. For instance, both "The window broke because stoned" and "The window broke because brittle" are complete. But Donagan, as we have seen, takes only the causal explanation, "The window broke because stoned," to fit the CL model. It presupposes the general law, "All windows break when stoned," while the dispositional explanation presupposes no law or antecedent condition but contains a lawlike statement, hence is complete and non-enthymematic as it stands.

There are, however, a number of difficulties with this line of reasoning. Those to be considered suffice, I think, to cast serious doubt on the application of Ryle's distinction between laws and lawlike statements to historical explanations and accordingly on Donagan's moderate criticism of the CL model. First of all, Professor Brodbeck[9] makes clear that the spelling out of the two enthymemes, "Broke because stoned," and, "Broke because brittle," reveals their similarity in mentioning some antecedent or simultaneous condition or happening. In the first case, the law is implicit while the happening, being stoned, is explicitly mentioned. In the second, the lawlike hypothetical defining the

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[9] M. Brodbeck, "Explanation, Prediction and 'Imperfect' Knowledge," in Minnesota Studies in the Philosophy of Science, Vol. III, p. 266.

disposition "brittle" is explicitly asserted, while the individual statement of fact, the instance that the window was struck, is implicit. Hence, contrary to Ryle's claim, neither is really complete. Both are instead enthymematic since each contains an implicit premise, the law in one case and the individual fact in the other. Both cases derive their explanatory force from subsuming the explanandum-event under general statements. The major difference lies in the nature of the general statement, for in one case it is a law and in the other it is a lawlike statement. The outstanding question is whether or not this difference suffices to preclude dispositional explanations from falling under the CL model, as Hempel attempts to do.

The second difficulty for the Ryle-Donagan position, then, concerns Donagan's misleading and inaccurate view of just what law or laws are presupposed in the "Broke because stoned" case. For this enthymeme to fit the CL model, he claims, it must presuppose the law "All windows break when stoned." This case represents a great many used by other writers to show the futility of applying the CL model to historical explanations. For any laws appealed to must be, as this one, trivial and uninteresting. Their criticism of the CL model would be successful if this were the case. But indeed it is not. Surely the available laws are not limited to mere summative generalizations such as this one, _i.e._, laws generalized from the particular event to be explained. This is of special interest in Gardiner's case because he holds motive-explanations to be complete and not in need of further explaining, since the lawlike statements contained in them are not "generalizable" or derivable from higher laws. But this argument utilizes an over-simple law as the implicit premise in the explanans. No doubt we could explain someone's boasting on the basis of his vanity in accord with the law "All men are vain."

But just as this is an inadequate premise, so is it not the only law we could use, a fact which Ryle, Dray, Gardiner, Scriven and other critics of the CL model seem to overlook. Obviously what holds for some one individual or even one group of individuals is not generalizable to all men in this sense. But Donagan uses this point to prove that there are no adequate laws to cover such cases. He claims that "The Norsemen and Danes who sailed south to the Irish Sea and to the shores of the English channel were plunderers first and settlers by an afterthought" would require, to be an adequate CL explanation, the buried assumption "All men, or all Norsemen and Danes, and perhaps Anglo-Saxons too, are plunderers first and settlers by afterthought." Yet, as most CL theorists anxiously reply, this is clearly not the correct implicit law. Surely all we need in this case is, following Professor Brodbeck, "that anyone's being a plunderer first and a settler by afterthought implies 'If he has opportunities of sufficient plunder in a territory, he will not settle in it.' This is not the same as saying that everyone is a plunderer. The implied general statement is the definition, in whole or in part, of the disposition term 'plunderer.'"[10] Hence, Donagan's

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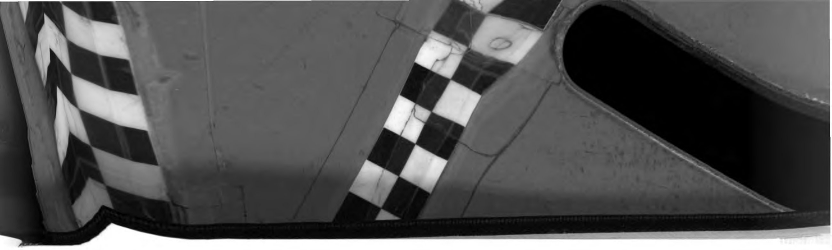
[10] Ibid., p. 268.

rejection of general statements about human dispositions, and his belief that historical explanations must contain only lawlike statements, is seen to be quite mistaken. For human actions can be explained by general premises about how people with the given motives can be expected to behave in given circumstances, without presupposing any general laws about all men. Thus can we avoid trivial and uninteresting general platitudes about men in general and also concentrate on the particular agent's character which allows the historian to "know his man." And we can do both within the scope of the CL model.

Moreover, the objection that this procedure merely allows the historian to know how the agent is similar to others, not to know his particular man, overlooks Weber's oft-repeated warning that to know any event or action in its absolute uniqueness is impossible. Surely if, say, Barere were a cowardly opportunist, we would not have expected him to act differently from the way other cowardly opportunists would have acted in his situation. In fact, would we not generalize, even in Dray's terms, from the act's being appropriate for him to its being appropriate for all men with sufficiently similar dispositions in similar situations? If so, the connection between law-covered and dispositional explanations is much closer than any of the moderate critics of the CL model suggest, for lawlike dispositional statements about human agents are inferrable from, or directly presuppose, general laws. As a result, CL theorists are not forced to deny the important function of dispositions or trait-ascriptions, along with that of



general laws, in such explanations.

This reply, however, has not gone unnoticed by the moderate critics. Ryle, Donagan and Dray offer explicit rebuttal, since, if the reply is correct, their painfully elaborated criticism of the CL model would indeed be idle. Their official answer assumes erroneously that if a dispositional statement is inferrable from or presupposes laws, then the laws must be learned first. "But," answers Ryle, "in general the learning process goes the other way. We learn to make a number of dispositional statements about individuals before we learn laws stating some general correlations between such statements."[11] And Donagan, taking this to be a decisive confutation, adds that one may know his windows to be brittle without knowing from what laws, in conjunction with other relevant information, such lawlike knowledge may be deduced.[12]

Hardly final or decisive, this answer fails even to be relevant. Suppose we grant Ryle's point that we learn particular disposition statements before we learn the related laws. His argument concedes, first, that such laws are sometimes available to be learned. And, secondly, it turns on a confusion between a question in the logic of discovery or learning and one in the logic of justification or explanation. For the CL claim is that lawlike dispositionals can be inferred from general laws is clearly a question of the logic of proof and explanation, not of

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[11] Ryle, op. cit., p. 124.

[12] Donagan, op. cit., p. 438.

learning. The fact that we learn about some particular glass being brittle before we learn that all glass is brittle, if indeed it be a fact, would not preclude the possibility of inferring the particular case from the general. But thirdly, the case of the glass breaking because it was brittle serves as an unfortunately poor example. For in this case, since the manifestation of the disposition involves the destruction of the glass and thus precludes experimenting with this particular glass, we would generally know that this particular glass broke because it was brittle only on the basis of our general knowledge about glass.[13]

However, Donagan's own examples about plunderers and other human dispositions are attempts to show, as we have seen, some logical peculiarity about human dispositions. But this requires a different argument and, hopefully, one based on more cogent grounds than Ryle's erroneous assumption about our learning behavior. Ryle obliges in this case as well with a distinction between what he calls single-track and multiple-track dispositions. The former are simple, specific or determinate in the sense of being manifested in only one uniform way; the latter are complex, generic or determinable since they can be manifested in a wide and even unlimited variety of ways. While there is only one sort of behavior we expect of a ruminant cow, an habitual

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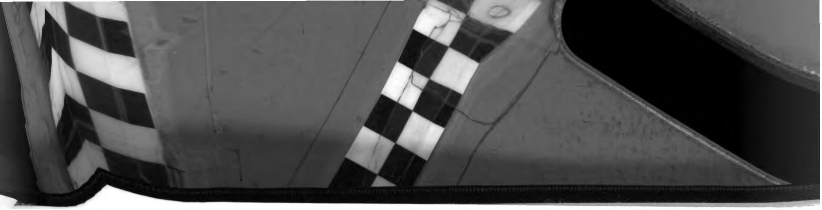
13 Cf. Dray's analysis of this issue in "The Historical Explanation of Actions Reconsidered," pp. 125-7.



smoker or brittle glass, we usually expect many different kinds of behavior from a vain, greedy, proud or ambitious man.

Human dispositions, then, being determinable rather than determinate, are such that our knowledge of them does not necessarily allow us to predict the particular manifestation or what the given person will do in a certain situation. Thus though the distinction may not be as clear-cut as Ryle, Dray and Donagan suggest, the main point remains that we cannot deductively predict in detail, say, that Disraeli attacked Peel in 1846 because he was ambitious. At most we could predict from Disraeli's ambitious character that the attack was one of a number of actions which the disposition allows us to infer, *i.e.*, that the disposition would be manifested in some way or other and that attacking Peel was one of these ways.

But again, this distinction fails to establish the conclusion that human dispositions cannot be derived from laws about human behavior. At best it argues for the practical difficulties in establishing such laws. Surely not an insignificant conclusion in itself, but not the conclusion at issue. Further, this very complexity and determinableness of human dispositions can be fashioned into an argument for the position that historians do sometimes actually defend their dispositional explanations by presupposing general laws, universal or probabilistic. Jonathan Cohen,[14] *e.g.*, has argued that if, say, Barere's opportunism allows us to infer

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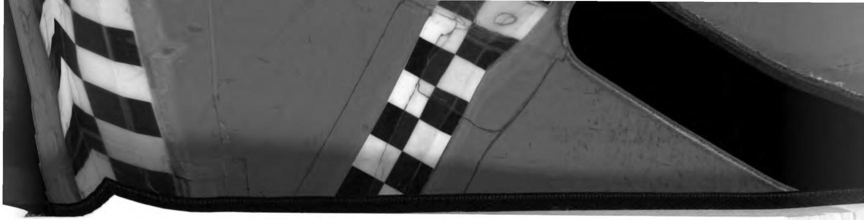
[14] J. Cohen, "Review," *Philosophical Quarterly* (1960), p. 192.

only its manifestation in some way or other and not specifically his joining the opposition to Robespierre, then in order to explain this act we must also know that Barere was disposed to act opportunistically in this particular way, as well as merely in some way or other. This suggests to Cohen that the historian here presupposes the generalization that a person who is disposed to act opportunistically in some ways will more than likely also be disposed to do so in other ways. This would be one way of defending the connection between dispositionals and general laws.

Consequently, the case of the moderate critics against the CL logical requirement of general laws to provide the explanatory force of arguments proves to be unsuccessful. The presence of dispositionals has not been shown to preclude general laws. Ryle's analysis of dispositional statements nonetheless establishes some important distinctions exhibiting the status of dispositional explanations, however little they weigh against the claims of such CL theorists as Hempel, Nagel or Popper. Ryle's claim that motives are not occult happenings but rather determinable dispositions, and thus not causes, for instance, in no way argues against the CL theory, so long as dispositions or psychological trait-ascriptions can be connected with other properties or actions by general laws. At best, Ryle's analysis challenges the hope of some CL theorists of finding some clear criteria for characterizing general laws as completely unrestricted or open in scope, *i.e.*, as lacking any essential reference to



individuals.[15] But since (R_2) merely assumes the notion of general law and is neutral concerning alternative analyses of it, the objection is to the hope of finding such criteria, not to the CL model itself.

Let me add one further comment about the role of general laws in historical explanations before returning to Hempel's dispositional construal of "rational action" and its contrast with Ryle's analysis. Most CL theorists have tried to eliminate the misconception that what historians, or natural scientists for that matter, try to explain are individual actions or events in all their peculiarity and uniqueness. This, as we have seen, because it leads to paradoxes and entails an infinite amount of information and subsequent selectivity, is impossible to accomplish. Nevertheless, because laws of any kind do not fully characterize specific, concrete, individual instances but only classes or kinds of instances, humanist critics of varied stripe insist that laws, and hence the subsumption of particulars under them, play no significant role in peculiarly historical explanations. Thus freed of the terrible scientific burdens of discovering, verifying and modifying general hypotheses or laws, they turn to more humanistic aspects of explanation such as judgments, insight and empathy.

It is important to recognize, however, that CL theorists

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[15] Cf. the accounts of Popper, The Logic of Scientific Discovery, sections 13-15; Hempel and Oppenheim, "Studies in the Logic of Explanation," p. 155; and Scheffler, Anatomy of Inquiry, pp. 86-8.

have not denied the need for such insight, judgment and empathy in the scientific quest for explanation. Their claim instead is that such elements properly belong to the psychology or the logic of discovery not to the logic of proof or explanation. Accordingly, they have consistently agreed with the premise that single laws do not completely explain individual events, yet dispute the conclusion drawn from it. What the uniqueness and unlimited aspects of individual occurrences argue for is neither the quest for ultimate metaphysical laws of history à la Marx, Toynbee or Spengler nor the rejection of the significant role of any laws à la Dray, Donagan or Collingwood, but rather a large network of general laws which in their intersection can begin to characterize adequately the complexity of the unique event. No doubt most people would concede that we know very few completely general historical laws. But, as with the beginning psychology student who delights in hypnotizing and analysing his "former" friends, our need in history is not for less knowledge or for peculiarly different knowledge but for more knowledge. We need more fully articulated and substantiated points of intersection, i.e., more general laws, the absence of which forces the historian to vague explanatory sketches or even to pure narrative and chronicle.[16]

Donagan and Dray may be correct in their chastisement of any proposed science of history as "at present purely visionary," and of any attempt to abandon traditional "antinomian" social inquiry

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[16] Cf. M. Bunge, Causality (New York: World Publishing Co., 1962), pp. 271-2.

on the mere sketch and promise of something better as foolish. Yet their arguments, far from refuting the possibility of such a program, fail to establish it even as a poor methodological strategy in the long run. Even the recognition of the statistical nature of historical events and actions, which requires historical laws to be construed as tendencies or trends, when added to this ideal or unattainable nature of unique events, ought not to force us to the futile humanistic strategy of postulating historical individual events as lawless in order to defend a libertarian metaphysical position.

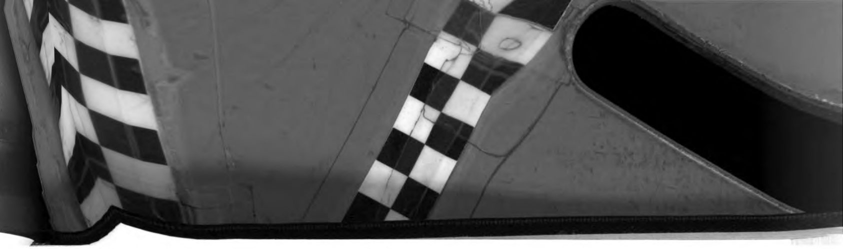
Such strategy might well succeed in depicting history as "a branch of the humanities." But only at the price of abandoning history as a branch of the science of society. Considered by itself, this would be a price high enough to constitute a poor methodological gambit. How unnecessary such a price is will be seen in the next chapter. There it will be argued that the presence of historical trends or tendencies as premises of CL explanations presupposes, not precludes, the humanistic and purposive element of judgment and insight, if not of empathy.

Now, having considered some aspects of Rylean lawlike dispositions and the unsuccessful use of them to modify the CL model, let us see how Hempel extends this model to historical, rational explanations. In particular, we will examine his defense of the requirement of strictly general laws so as to permit dispositional explanations as a species of causal explanations.

Hempelian Analysis of Dispositions

As noted in the first section, Hempel views explanations by motivating reasons as "broadly dispositional," and hence as conforming to the requirements of the CL model since they explain by subsuming the explanandum-action under covering general laws. In light of the Rylean claim that these two views are incompatible and that subsequently we need relax the requirement of strictly general laws to permit dispositionals as lawlike statements, we will consider how Hempel's version of dispositions differs from Ryle's and how he connects dispositionals with laws without conjuring up again the ghost in the machine. Let us begin by examining rationality along "broadly dispositional" lines.

Ryle's distinction between determinate and determinable dispositional predicates, between, *e.g.*, "brittle" and "ambitious," does not adequately account for what is implied by such psychological concepts as rational agent. Even if the distinction is loosened to one of degree, so that determinate predicates are not absolutely simple or single-track but only less complex then determinables, the former still differ from names of psychological personality traits. For they refer to dispositions to respond to specific external stimuli by certain characteristic overt behavior. For example, to say of a piece of glass that it is brittle implies that when stoned it will break. But, on the other hand, to call someone ambitious or a rational agent is not merely to say he also will respond to specific external stimuli, though in a more complex variety of ways. We do of course imply



this. Yet the situation is complex in still another way which Ryle's distinction fails to take into account.

The circumstances of a rational agent's behavior cannot be adequately described by reference merely to environmental conditions and external stimuli. As Weber, Schutz and Dray emphasized, they include the agent's goals, purposes, capabilities and beliefs, which are also dispositions of the person. Hempel marks this difference by suggesting "that the dispositions implied by attributing rationality to a person are higher-order dispositions."[17] These beliefs and objectives, in response to which a rational agent characteristically acts, are themselves not merely manifest external stimuli but are also broadly dispositional features of the agent. Since, in other words, attributing a particular belief or objective to someone implies that he will in certain circumstances tend to behave in ways symptomatic of his belief or goal, to further attribute rationality to him is to talk of a disposition about a disposition. It is a second-order disposition to respond, which therefore includes reference to first-order dispositions. This second-order aspect of psychological trait-ascriptions is what Ryle's interpretation of them as simply bundles of dispositions fails to capture.

Further, Ryle's analysis suggests that both determinate and determinable dispositional predicates are definable by lawlike subjunctive conditionals, or are fully specified by the latter

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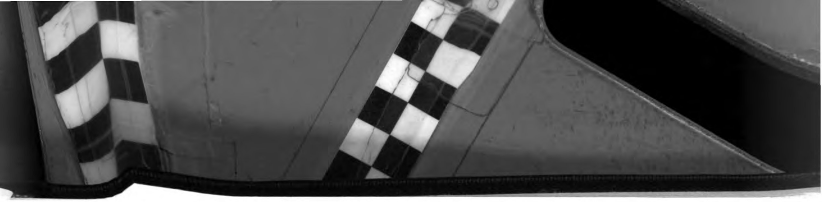
17 Hempel, "Rational Action," p. 14.



implications which provide operational criteria for their application. But Hempel's broadly-dispositional label indicates that a statement expressing such an ascription as rationality "may *imply, but is not tantamount to*, a set of other statements which attribute to the person certain clusters of dispositions."[18] These dispositions constitute at most symptoms or indices of the agent's rationality, not fully specified definitions.

Consider, for example, R. B. Brandt's sketch of a possible explanation of why Barere voted for the guillotining of the king. Besides premises citing Barere's normal state of mind and general situation at t_1, Brandt replaces any strict general law by the following two premises:

> "Premise 2a: At t_1 Barere was a cowardly man, and therefore by definition,
>
> Premise 2b: At t_1 Barere would be motivated to do any action A, which he regarded as the only one which would provide reasonable security for his personal safety, more strongly than to do any conflicting action non-A, provided he were in a normal frame of mind."[19]

In this case Premise 2b is not a general empirical law but a Rylean lawlike conditional statement about the individual Barere, having the status of a partial meaning of Premise 2a, " as a definition of 'cowardly' or as an analytic truth." Hempel's broadly dispositional analysis of "cowardly," on the other hand,

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18 *Ibid.*, p. 14.

19 R. Brandt, "Personality Traits as Causal Explanations in Biography," in S. Hook (ed.), *Philosophy and History*, p. 198.

would require dropping the phrase "and therefore, by definition" from Premise 2a and rewriting Premise 2b as a general law about cowardly people instead of about Barere, so that the explanation conforms to the form of (R).[20]

Hempel's criticism of the Rylean analysis, as seen in our example, is directed against the suggestion that Barere's dispositions to respond in characteristic ways in the situation exhaust the meaning of asserting he was cowardly. Instead, Hempel takes such trait-ascriptions to be governed by a network of "quasi-theoretical connections" or principles which interconnect the complex interdependencies of the many psychological concepts in question. These principles also conjointly "determine an infinite set of empirical consequences, among them various dispositional statements which provide operational criteria" for ascertaining when an individual is cowardly. This parallels the physical case of saying a body is electrically charged or magnetic. In these cases as well the important point is that

> the underlying theoretical assumptions contribute essentially to what is being asserted by the attribution of those physical properties. Indeed, it is only in conjunction with such theoretical background assumptions that a statement attributing an electric charge to a given body implies a set of dispositional statements; whereas the whole set of dispositional statements does not imply the charge, let alone the theoretical background principles.[21]

Even though the names of psychological trait-ascriptions

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20 Ibid., p. 201.

21 Hempel, "Rational Action," p. 15.

obviously do not occur in theoretical networks with anything like the scope and explicitness of physical theories, for which reason Hempel calls them "quasi theoretical connections," they nevertheless do presuppose these similar connections. We no doubt assume, for instance, that Barere's overt behavior of voting for the king's death as a means of pursuing his goal of personal safety depends on the interdependencies of his many other beliefs and objectives. With this goal, we would expect him to so vote only if, say, he believed that the vote would carry and be actionable, that there was no better way to achieve his goal, and that the achievement of his safety would not seriously conflict with his other objectives, especially ones he considered more important. In this way, attributions of goals to an agent imply certain specific overt behavior only when conjoined with appropriate hypotheses about his beliefs, and conversely. The agent's behavior cannot therefore be used to test these attributions of goals or beliefs separately, but only together as part of a network of hypotheses and assumptions. Hence, "belief attributions and goal attributions are <u>epistemically interdependent</u>."[22]

But this indicates that often we cannot decide, without good antecedent information, whether the agent believes an act will efficiently produce one of his goals or whether he simply is uninterested in that particular goal. In other words, we must use economic criteria of efficiency concerning an agent's values

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22 <u>Ibid</u>., p. 16.

and goals to determine whether his conduct is a manifestation of his belief or merely of his change of interest. Otherwise, by failing to recognize this interdependence, we run the risk of falsely accusing historical agents of stupidity when the error is ours in failing to understand what the agent actually valued and intended to do.[23] This very consideration, this interdependence of values, goals and beliefs, constitutes the most important aspect of the notion of Verstehen and the various recent reconstructions of it, including Dray's.

But, as Hempel's treatment of rationality shows, this point in no way conflicts with the CL theory of explanation. The belief that it does stems largely from associating the CL theory of explanation with earlier positivist pronouncements on narrow operationalist criteria for testing, *e.g.*, belief-ascriptions. By advocating this epistemic interdependence, Hempel clearly avoids such associations. He thus not only eliminates most of the conundrums which plague any narrow behavioristic construal of belief, but also enlarges the analysis of trait-explanations so as to preserve a close parallel with explanations in the natural sciences.

However, this interdependence-thesis has one additional consequence which Hempel has so far been unwilling to incorporate into the CL theory. If an adequate explanation requires true or

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[23] Cf. C. W. Churchman, *Prediction and Optimal Decision* (Englewood Cliffs, N.J.: Prentice-Hall, 1961), pp. 268-91.

well-confirmed laws as premises, then it would seem that since confirmation of any hypothesis depends on the relevant purposes or goals and on the cost of applying them, in addition to other empirical evidence, then confirmation must also depend essentially on economic-decision criteria. Hence the value-neutrality thesis will have to be rejected. We will pursue this extension of the interdependence-thesis in more detail in the next chapter, so for the moment let us continue with other criticisms of Hempel's analysis of rationality.

Rejecting the Rylean analysis of psychological trait-ascriptions and instead emphasizing the essential role of quasi-theoretical assumptions connecting goals and beliefs results in a quite different interpretation of trait-ascriptions. They are taken, in Hempel's analysis, as theoretical constructs. As such, they function as undefined predicates in a network of theories. They are given meaning by a "partial interpretation" of the laws and correspondence rules, i.e., of the quasi-theoretical connections, which interconnect them with other theoretical constructs, dispositionals and observational or manifest predicates. The meaning of "rational agent" or "cowardly," for instance, would be anchored in the entire interdependent cluster or network of theories. It would not be given to each concept separately. Thus premise (C) indicated in (R), "In a situation of the type C, a rational agent will do X," can be properly regarded as an empirical covering law which connects the attribution to the agent of certain beliefs, objectives and values with a description

of his external circumstances and his expected conduct X. This of course assumes premise (b): that he is disposed to act rationally at t_1.

Critique of Hempel's Rational Model: Rationality and Tautologies

Further clarification of the network-like character of these quasi-theoretical connections and the epistemic interdependence of goal-attributions might be achieved by considering some possible objections to Hempel's model of rational explanations as represented by (R). It might seem, first of all, that just because of this epistemic interdependence Dray is correct in not requiring premise (b), that the agent in question was a rational agent at t_1, as an explanatory hypothesis. For the criterion needed to test the hypothesis, that the agent wants to attain a given goal, by observing his actions and assuming we know his beliefs, would already presuppose that he is rational. That is, that he will choose an action that is rational relative to his goals and beliefs or one which offers him the best chance of success. In other words, since the very criterion we use--to determine which actions are implied by, and hence serve as evidence of, the goal-attribution--presupposes the agent's rationality; premise (b) occurs vacuously as an inviolate analytic truth. Any apparent violation, as Dray argued in dismissing this premise from rational explanations, would only show that either our goal-attributions or our belief-attributions were erroneous.

This line of reasoning, you will recall, led Dray to the unfortunate position of "constructing" fictitious calculations which the agent might have gone through but didn't. It led also to his claim that we cannot rationally understand a logical error, and hence that his model applied only to rational actions "not judged to be defective in various ways." Indeed, if one assumes beforehand that every action explained in accordance with the model is a rational one, then naturally it follows that the agent was rational at the time of the action and thus that premise (b) is unnecessary. But Hempel adds premise (b) in order to avoid just this kind of predicament.

Instead of restricting rational explanations, a la Dray, in such a way that fictitious constructions are allowed while logical errors and actual actions remain inexplicable, Hempel wisely leaves open the possibility that "there are various kinds of circumstances in which we might well leave our belief- and goal-attributions unchanged and abandon instead the assumption of rationality."[24] Since, then, the agent might have made a logical error in his calculation or overlooked some relevant items of information he believes to be true or even have been under emotional strain, the assumption of his rationality at the time of the action remains a corrigible empirical hypothesis. It thus functions as an essential premise in the explanation of why he actually did the act, not as a vacuously occurring analytic truth.

The provision that the ascription of a rational disposition to the agent be corrigible, along with those of beliefs and goals, serves also to emphasize the complex way in which clusters or networks of theories are testable or confirmable. A counter-

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24 Hempel, "Rational Action," p. 17.

example, i.e., a predicted action which failed to occur, would not necessarily refute any particular hypothesis in the network, even though all are corrigible. For the unit of empirical test is not each isolated hypothesis or theoretical connection but, in Quine's metaphor, the "corporate body" of the whole contextual network, if not of the whole of science.

One might, however, concede the nonanalyticity of premise (b) in Hempel's model, but still object on the related grounds that the analyticity occurs in premise (c), "In a situation of the type C, a rational agent will do X." This purportedly covering law about what rational persons will do, R. B. Brandt contends,[25] is not really a corrigible, empirical law at all. It is instead an analytic statement "true by definition," since it expresses part of the meaning of 'rational agent.' If so, Hempel's broadly dispositional analysis of rationality and other trait ascriptions would consequently presuppose no general laws. Hence (R) would not be a model of testable nomological explanations. This objection raises once again the question of how best to include the element of rationality when explaining why some historical agent acted as he did.

We have so far rejected two alternatives: that rationality enters as a lawlike dispositional and that it does so as a normative standard or principle of action. We will shortly discuss another alternative, a more novel one suggested by Scriven. But

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[25] Brandt, op. cit., p. 203.

the issue at present is whether rationality can be brought into rational explanations as partially defined by a general empirical law or whether interpreting it as a theoretical construct makes it part of an analytical definition.

Brandt opts for the latter position. He regards saying "that a rational person will do what he takes to be the course of action which will probably maximize utility in his situation" as an analytical assertion. He also takes Hempel's premise (b) to be saying that the agent in question thinks action X will maximize expected utility in the circumstances.[26] If so, we would then need no further premise telling us that a rational agent will perform action X. Brandt, in other words, seems to find it paradoxical to say both that the "law" gives the partial meaning of 'rationality' and that it is a corrigible, synthetic, empirical statement.

Now this objection elicits another important logical feature of such theoretical constructs as 'rationality' which are embedded in a cluster of interconnecting theories. It should be noted that Brandt's notion of analyticity itself raises a great many philosophical problems since it suggests that a clear distinction can or has been cogently drawn between analytic and synthetic statements. Since the issues involved are much too complex to be considered here, we will merely assume such a distinction in order to pursue Brandt's main point.

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[26] Ibid., p. 203.

Hempel's reply to such a criticism consists in pointing out, first, that such concepts are governed by a large network of general "symptom statements" connecting the disposition of rationality with various manifestations of symptoms of its presence, of which (C) is a part. And since, further, the whole cluster or totality of these symptom statements for the disposition of rationality have implications which are plainly not analytic but empirical, "it would be arbitrary to attribute to some of them--*e.g.*, the one invoked in our *explanans*--the analytic character of partial definitions and to construe only the remaining ones as having empirical import."[27] That is, since, as with Carnap's reduction sentences for a given dispositional concept, this network of theories taken as a totality implies some nonanalytic consequences with the status of general empirical laws, at least some part of the network must be empirical. Not all the statements in the network can be analytic, although any particular one could be.

Consequently, Brandt's paradox seems merely apparent. The general law can indeed, because of its role in the network, give partial meaning to the concept of rational action and yet be a synthetic empirical assertion. For the connection between the disposition of rationality and its specific manifestations is not merely a direct empirical one, not merely an instance of this

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27 Hempel, "Reasons and Covering Laws in Historical Explanations," in S. Hook (ed.), *Philosophy and History*, p. 156.

disposition taken separately. Rather is it a more complex empirical one conditioned by the interdependence of the various concepts, laws and rules embedded in the network. This points to the fact that rational action has of yet no precise, fixed meaning but undergoes change with the progress in our psychological knowledge.

Perhaps it is this changing, unfixed, nonsystematic aspect of trait-ascriptions that Brandt, after all, intends to emphasize. Many philosophers complain of such networks of theoretical constructs that they do not square with the facts of our common-sense experiences, since we clearly do understand trait-ascriptions outside a reconstructed interpretative system of laws and correspondence rules. Brandt remarks that we have "a quite definite understanding of terms like 'cowardly' by themselves."[28] This fact he takes to be denied by Hempel's analysis of these terms as theoretical constructs. Somehow this view implies, for Brandt, that trait-names are not understandable outside such a systematic network.

I submit, however, that this general complaint, and _a fortiori_ Brandt's particular version, rests on an ambiguity concerning "understandable." Obviously, a trait-name can be said to be understood in many ways, some of which are more important scientifically than others. If Brandt means, _e.g._, that 'cowardly' can be understood in any of the varied presystematic ways without benefit of a reconstructed system, the claim amounts to little more

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[28] Brandt, _op. cit._, p. 202.

than a harmless truism. For Hempel's analysis conflicts with such understanding only as an attempt to reconstruct or explicate it. If, on the other hand, Brandt means that such names can be understood in the sense that they can be systematically explicated in a different way, *e.g.*, as subjunctive conditionals rather than as theoretical constructs, then such an explication needs be offered and its merits compared with those of Hempel's explication. For clearly this is the important issue raised by the complaint. But however difficult this issue may be to resolve, only confusion results from the misleading complaint that a philosopher's explication does not square with the fact that we understand on a presystematic level the term being systematically explicated.

Critique of Hempel's Rational Model: Rationality and Evaluations

One further criticism and accompanying modification of Hempel's model (R) remains to be considered. Unlike those just discussed, this one bears not only on the Hempelian notion of rational action but also on the statistical nature of premise (c), and hence on the subsequent probabilistic character of model (R) to be considered in the next chapter. Pertinent to the topic of this section, however, Scriven's alternative proposal to Hempel's CL model arises from his concurring reply to Brandt's charge that (C) is analytically true. He quite agrees that "In a situation of the type C, a rational agent will do X" is not a tautology. But for quite novel and suggestive reasons, which lead him to offer another still different modification of Hempel's model. Scriven's reply to Brandt turns on the complex way that the disposition of rationality is connected to, "includes," or implies the performance of act X. No doubt this connection looks like a tautology. But Scriven follows Hempel in pointing out that "rationality only makes its manifestations probable,"[29] that the connection is usually along the lines suggested in our earlier discussion of partial explanations.

Unless the complete situation C and action X were so fully specified as to make premise (C) automatically provable, an unachievable ideal, (C) would be informative about the agent's

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29 Scriven, "New Issues in the Logic of Explanation," p. 342.

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situation, about his other beliefs and goals which are required by (C) but not included in it. However, Scriven refuses to follow Hempel to the conclusion that the connection and hence premise (C) is of the ordinary descriptive, statistical kind. He takes, rather, the intermediate position that action X is "not only an example of, but a *quasi-necessary consequence* of rationality."[30] No specific acts are necessary conditions or consequences of rationality, since the failure to so act on one occasion surely does not make one irrational. Yet Scriven maintains that the connection between rationality and its specific manifestations is more than a merely empirical or statistical one. Thus he refers to it as quasi-necessary since "it is logically impossible that someone be rational and exhibit *none* of these manifestations." He further explicates it so that the "consequence of being a rational agent is that he will do A in C at T, where A appears to him to maximize his expectations of his desired goals D. This is what he will *normally* do but special circumstances may lead him to do A' without destroying his claim to be rational."[31]

Precisely how this account differs from Hempel's statistical construal, however, still remains obscure. Scriven offers some help by suggesting that premise (C) looks tautological because, and here he follows Dray, it is an evaluative proposition, a prudential maxim or rule "informative as to what *should* (rationally)

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30 *Ibid.*, p. 341.

31 *Ibid.*, pp. 343-4.

be done." He also regards it as a "tautology-sketch" since supplementing it with the detailed facts makes it into a tautology. That is, if we were given all the relevant facts, then "the conditional with all the facts in the antecedent and 'the rational man does A' as the consequence is a tautology."[32] In an earlier essay he characterized these substitutes for general laws in historical explanations as "truisms" or "normic statements," as a kind of logical hybrid having some universal and some statistical features. They alone are capable of providing the grounds for acceptable explanations of individual events, because of their crucial role as "norm-defining" giving them a "<u>selective immunity</u> to apparent counter-examples." And the study of such statements he regards as the "logic of guarded generalizations,"[33] since though they are not definitionally true, they still cannot be falsified in the relatively direct way that simple empirical generalizations can. Unlike statistical statements also, they are not restricted to saying about the things to which they refer that some do and some do not fall into a given category. Rather, normic statements say that "<u>everything</u> falls into a certain category except those to which <u>certain special conditions</u> apply."[34] Hence they claim a preferred status for some particular kind of

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32 <u>Ibid</u>., p. 342.

33 Scriven, "Truisms as the Grounds for Historical Explanations," in Gardiner (ed.), <u>Theories of History</u>, pp. 464-5.

34 <u>Ibid</u>., p. 466.

behavior, since on theoretical grounds deviations from this behavior can be attributed to the operation of interference factors.

Thus, in his example of explaining that William the Conqueror did not invade Scotland because he had no desire for the additional lands of the Scottish nobles, the implicit generality and ground of the explanation would be some such normic truism as, "Rulers who are satisfied with what they have do not normally or usually invade neighboring territories."[35] By replacing Hempel's general laws, universal or statistical, by such normic generalizations, Scriven claims to have captured the explanatory force of a law, yet without sacrificing the hold on the particular case. While not ruling out all cases to the contrary, unlike universal laws, they still inform us of more than that rulers sometimes or seldom act in this way, unlike statistical generalizations. By thus saying something weaker than one kind of law and stronger than another, because of representing the normal, proper or standard case, normic truisms purportedly overcome the basic weakness of statistical statements. They neither abandon the hold on the particular case, nor allow the particular case to rattle around inside a network of statistical laws, since it can now be located in the normic network. Such statements locate the particular action or event by telling us what had to happen in this specific case, unless of course certain exceptional

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[35] Ibid., pp. 444 and 467.

circumstances obtained. In this way we can be sure that C, e.g., explains X even though we lack exact laws.

The problem, however, as Scriven quickly recognizes, is just how to interpret the notion of "exceptional conditions," so that we can cogently distinguish between counterexamples which actually do falsify these normic truisms and those cases which are merely apparent counterexamples. Unless Scriven can clarify this point, his hybrid modification of the CL theory along with his criticism of Brandt will go the way of most empathy theories by being untestable in principle.

But here Dray's perceptive remark seems most appropriate: when pressed on this point, Scriven's normic generalizations are in danger of being assimilated either to Hempel's ordinary descriptive statistical laws or to Dray's normative principles of action.[36] Scriven, of course, claims to have captured the advantages of each without their accompanying weaknesses. His support for this claim, nevertheless, remains unconvincing. And largely because he fails, along with the others we have considered, to locate properly the role of normative judgments and decisions in explanatory arguments. This failure leads him to support one version of SU by denying the cogency of the CL theory, and to consider history "the mother subject for explanations" on the grounds that historical explanations are logically no different from common-sense ones.[37]

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[36] Dray, "The Historical Explanation of Actions Reconsidered," p. 121.

[37] Scriven, "Truisms as the Grounds for Historical Explanations," p. 462.

But to elicit the nature and import of this failure, we must show how his proposal can be assimilated either to that of Dray or of Hempel.

Scriven's comments about the "norm-defining" role of normic truisms and their consequent immunity to any direct or straightforward falsification lead one to suspect that they are, after all, merely what Dray called "principles of action." This suspicion seems further supported by the fact that, following Dray's account, this normative element purportedly provides the explanatory force of reason explanations. In fact, I think Dray correct in remarking that such truisms as, "Rulers who are satisfied with what they have do not normally invade neighboring territories," can be easily interpreted as norms reminding us of what is appropriate or reasonable to do in the given circumstances.[38] If we simply substitute "appropriately" for "normally," we are reminded of what rulers will do, unless of course they are acting in unreasonable ways: arbitrarily, emotionally or ignorantly. In this way, the explanatory function of his normic statement would indeed be to provide the rationale of William's noninvasion of Scotland. Thus would Scriven's modification of the CL model be assimilated to Dray's rational model.

Moreover, by stressing the difference between the way normic and statistical statements handle exceptional conditions, Scriven develops Gardiner's emphasis on the historian's need to make

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[38] Dray, "The Historical Explanation of Actions Reconsidered," p. 122.

appraising judgments in applying his generalizations. Unlike Gardiner and Dray, however, Scriven recognizes the same use of judgment in applying physical laws, even though it plays a weightier role in history. Hence he refuses to concur with their rash claims about the disunity of science and history. He argues, *e.g.*, that the physicist, as well as the historian, judges that his causal explanations are the right ones, and that this judgment is "unformalizable," aided by "empathy" and dependent on contextual considerations.[39] Since the importance of normic truisms, for Scriven, lies in their knowability and utility even when all the special conditions on which their truth depends are impossible to state, they function as tactical rules in bridge or chess. "Second hand plays low," and "Deploy your pawns early," for instance, parallel "Power corrupts." And since such rules are clearly not exceptionless, the historian, as the skillful and experienced player, develops his trained judgment and capacity to handle these normic generalizations. He recognizes the network of exceptions, realizes the limits of their applicability and the degree of their acceptability, even when unable to formally state or articulate these matters.

Yet, even granting Scriven the complex relationship between dispositions like rationality and their quasi-necessary manifestations or consequences, the selective immunity to apparent counterexamples of explanatory generalizations, and the important role

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[39] Scriven, "Truisms as the Grounds for Historical Explanations," p. 462.

of judgment and contextual considerations in applying these generalizations--even so, it would still be feasible to assimilate his normic truisms to statistical generalizations as required by Hempel's model (P). That is, it is not at all clear what logical novelty Scriven's truisms have which make their explanatory force significantly different from that of statistical statements. Hempel's premise (C) will most often, of course, be such a nonuniversal statement as, "In a situation of type C, a rational agent will probably (more than likely, usually) do X," thus meeting the complaint that the original universal statement was too strong.

In addition, it also counters Scriven's objection that the particular case X "rattles around" inside a network of statistical laws which loses hold of this case, by providing as equally direct relevance to the particular case as normic generalizations. But to do so involves invoking the "requirement of total evidence" as part of the statistical model (P). By thus requiring that the total evidence available be used as a basis for determining the acceptability or degree of confirmation of an explanatory hypothesis, Hempel provides a sound basis for assimilating normic statements to statistical generalizations. Not only are they comparable in explanatory force, but the latter, when supported by the requirement of total evidence, provide the basis for a more extensive understanding of the particular case. This satisfies Scriven's further requirement of "informative relevance," which he no longer has to misconstrue as a replacement for inferrability, since statistical generalizations also warrant the inference of the particular case.[40]

Further, if our argument is cogent, the residue of Scriven's objection to model (R) lies in its statistical provisions. In particular, his case rests finally on the claim that the needed methodological requirement of total evidence has "the drawback of being virtually unattainable,"[41] and hence that Hempel's model needs modification to include the element of judgment in order to be informatively relevant. This brings us, then, to the question: what role to assign to what Scriven correctly takes to be "the great truth in the Verstehen theories." That is, to their "recognition of the indispensability and efficiency of the historian's capacity to respond to the cues in a well-described situation, so that he may with justifiable confidence accept or propose a particular reasons-explanation as correct."[42]

As the next chapter will show, even Hempel concedes the inadequacy of the requirement of total evidence, and for a number of reasons. Thus it needs be replaced by some more manageable substitute, as well as to be reinforced by further requirements which explanations must satisfy if they are to qualify as rationally acceptable. But if so, then we must ask whether reinforcing the requirement of total evidence entails any serious logical or epistemological modification of the CL theory of explanation.

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40 Scriven, "New Issues in the Logic of Explanation," p. 357.

41 Ibid., p. 345.

42 Ibid., pp. 358-9.

Scriven advises of course that it does. He modifies it by replacing the covering statistical or probabilistic laws with normic truisms, because only the latter cogently provide the informative relevance of explanations to the particular case. My contention, on the other hand, is that Hempel's theory can accomplish this task without such logical modification. But it can do so only by conceding one of the major epistemological theses advocated by many recent Verstehen theorists, including Scriven, Dray, Gardiner and Lavine. It must surrender the value-neutrality thesis. In the next chapter we will argue not only that this modification would improve the CL theory, but that an adequate reconstruction of probabilistic explanations, as explicated by Hempel according to model (P), actually entails this epistemic thesis. The explainer must logically consider contextual and purposive matters, and make cost or value judgments, in order for his explanation to be rationally acceptable. This requires, as we attempt to show, interpreting scientific knowledge along the pragmatic lines suggested by Dray and Scriven but rejected by Hempel as presystematic.

Their analysis, however, locates the pragmatic element in the generalization providing an argument with explanatory force (either principles of action or normic truisms), and hence supports SU by modifying model (R) accordingly. Instead, we take Hempel's model to be free of the purported logical difficulties. The major modification needed concerns not the laws providing explanatory force but the epistemic requirement that the explanans-hypotheses be at least well-confirmed. This, we will claim, suffices to locate and account for the important pragmatic element of the historian's capacity to judge exceptions to laws and, more generally, to judge the acceptability of these laws or generalizations.

Moreover, by locating the pragmatic value judgments in the role of appraising and selecting explanatory generalizations rather than in the generalizations themselves, we can avoid another of Scriven's charges, which otherwise might prove embarrassing to Hempel. It occurs as a dilemma which would, if cogent, "mark the end of any 'model.'" Either, Scriven argues, Hempel would treat the informality or looseness of many explanatory generalizations "as a sign of imperfection and reject them," or he would not. Now to reject them, I agree, would be to make a mistake analogous to the one Hempel seems originally to have made in taking the deductive model to be more scientifically adequate and complete than the probabilistic model. But not to reject them is to make the probabilistic model "largely a matter of good, nonformalizable judgment about the 'weight of the evidence.'" This is not, as many suppose, a sign of unscientific explanation, but rather "a sign that much of scientific argument conforms more closely to legal and historical argument than it does to mathematical inference."[43] In either case the CL theory appears to be in trouble.

The usual counter to this kind of charge, already noted in the replies to the early idealists and to Schutz and Dray, grants

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[43] Scriven, "Explanations, Predictions and Laws," in Minnesota Studies, Vol. III, pp. 227-8.

the first horn of the dilemma. But it denies the second on the grounds that while such looseness entails pragmatic judgments about the weight of evidence, it does so only in some presystematic or psychological sense. Hence the logic of explanation remains free of such loose matters. In other words, CL theorists reply to such charges by the claim that the choice of premises or their acceptability as explanans "is not in the strict sense a logical matter and does not affect the validity of a deduction."[44] Here Hempel and Nagel concur with Brodbeck's counter to Scriven.

Yet this seems an obvious but irrelevant response. Is it not, in fact, a retreat to the first horn of the dilemma? For Brodbeck, yes; but not for either Hempel or Nagel. Both resist the temptation to assimilate all probabilistic explanations to the deductive model, but instead consider model (P) to be an independent ideal with peculiar epistemic requirements and difficulties of its own not encountered by the deductive model. One of these epistemic requirements of an adequate explanation concerns the very point at issue, which Brodbeck consequently ignores: the confirmation and acceptability of the premises in an adequate probabilistic explanation.

It seems, then, that the force of the 'empathy' position, especially as put by Scriven, has not been appreciated by CL theorists because of the overemphasis placed upon the logical requirements of the deductive model, and because of the little

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[44] Brodbeck, op. cit., p. 242.

attention paid to the intricacies and difficulties of model (P). The important question remains: must the pragmatic elements of judgment and acceptability of explanatory hypotheses be included in an adequate reconstruction of scientific probabilistic explanations? Part of the point of Scriven's dilemma is to force an affirmative answer, with which I concur.

However, I take issue with the conclusion he draws from this thesis, viz., that to allow these pragmatic ingredients into statistical explanations is "to mark the end of any 'model.'"[45] For this would follow only if the pragmatic elements were unformalizable in principle and were part of the logical explanatory force of the model, e.g., if they required general laws to be replaced by normic truisms or principles of action. Since, instead, the judgments are more properly taken as epistemic weights for appraising the acceptability of such explanatory generalizations, the model remains logically intact, unscathed by the challenge.

But to show why this is so requires us to turn, in the next chapter, to a more detailed consideration of model (P) than has heretofore been offered. In particular, we want to see what specific difficulties give rise to the requirement of total evidence, why it is not by itself sufficient as a criterion of evidential adequacy or rational credibility for probabilistic hypotheses and, most importantly, why and how pragmatic utilities are needed to supplement purely epistemic utilities as criteria

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[45] Scriven, "Explanations, Predictions and Laws," p. 228.

of such rational credibility. In sum, our thesis is that Hempel's CL theory survives the varied logical criticisms of 'empathy' theorists, but only on condition that the value-neutrality thesis be rejected, that pragmatic elements be included essentially in a logical reconstruction of probabilistic explanations, and that the structural emphasis on explanations be extended to include a purposive ingredient.

If this case can be made successfully, the insistence of Dray and Scriven on the "irreducible pragmatic dimension to explanation" will have been supported against Hempel's claims to the contrary. Supported, of course, by redirecting or relocating the pragmatic dimension in such a way as to preserve the cogency of Hempel's logical model of explanation against their other charges. But supported nonetheless. Hence, Hempel's reply--that the pragmatic concept of explanation, however important, can claim only psychological or genetic priority over the theoretical CL ideal; and that the latter "is objective in the sense that its implications and its evidential support do not depend essentially on the individuals who happen to apply or to test them"[46]--must be viewed with skepticism. Whether a given set of explanatory premises, containing nonvacuously occurring statistical hypotheses, adequately explains a certain action or event to a given person will indeed depend partially on just such "subjective,"

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[46] Hempel, "Explanation and Prediction by Covering Laws," B. Baumrin (ed.), Delaware Seminar in Philosophy of Science, Vol. I (New York: Wiley and Sons, 1963), p. 130.

purposive and contextual features as interests, attitudes, judgments and values. But it will depend on these features in a systematically epistemological, not just psychological, way.

Still, whether the introduction of these pragmatic features makes the logic of explanation essentially subjective or non-formalizable in a vicious sense, as Hempel and Scriven seem to believe, is another and quite independent question. My suspicion, however, is that it does not. Yet this belief, that value considerations are not amenable to objective treatment, seems partly responsible for the general failure among CL theorists to recognize the extent to which the probabilistic model and the entry of stochastic laws into the sciences have, in Rescher's phrase,[47] revolutionized not only our concept of nature and man but also our concept of scientific explanation. They force a fundamental re-examination of the very meaning of this concept along pragmatic and purposive lines. For this reason we turn, finally, to a more detailed analysis of Hempel's inductive systematization of nomological explanations.

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[47] N. Rescher, "Fundamental Problems in the Theory of Scientific Explanation," ibid., Vol. II, p. 41.

CHAPTER VI

THE PRAGMATIC DIMENSION OF EXPLANATION AND THE VN THESIS

The Probabilistic Model and VN

In the preceding chapter we suggested that an adequate analysis of Hempel's extended version of the CL model, covering teleological as well as causal explanations, requires investigating the use of the probabilistic model (P) as the more appropriate form for rational explanations. Only thus can we determine to what extent the CL theory can be defended against the SU thesis, and in what way it entails a denial of the VN thesis. That is, our twofold contention--that empirical generalizations are necessary to provide the inferential explanatory force of rational explanations, and that the epistemic condition (R_4') needs be expanded to include inductive criteria other than confirmatory strength--requires a defense provided only by a detailed analysis of Hempel's recent statistical systematization of explanatory arguments. To this task we now turn.

Since the CL theory interprets explanation as an inferential relationship, deductive or inductive, between a set of laws conjoined with antecedent conditions and the description of some event or action, the main difficulties for the historical use of model (P) turn on requirement (R_4'), that an acceptable explanation must contain hypotheses or theories with a high degree of confirmation. This requirement in turn raises most of the problems of induction generally, especially the problem of providing adequate criteria of good evidence and reasonable belief. For, unlike deductive inference, statistical or inductive inference contains an essential gap between explanans and explanandum. It contains premises which evidence or support but do not prove the conclusion. The latter, by outstripping or asserting more than the premises, is not "contained in" them. Thus the ensuing difficulties concern "filling in" or bridging the gap, finding good reasons to support the conclusion, recognizing the degrees of reasonableness, and determining what degree of evidence suffices to warrant the rational acceptance of our explanations.

It is generally agreed, I think, that the amount of evidence required to warrant any particular belief is not fixed but a function of our varied purposes. That sufficient evidence also depends on such pragmatic factors as the cost concerning the significance of making a mistake when acting on the belief in question, however, seems less generally recognized. This latter thesis bears heavily on historical explanation and the invidious comparisons often made between the kinds of explanations employed in various disciplines. For it appears that these disciplines differ not in the logical kinds of explanations offered, as indicated by the SU thesis, but rather in the different emphasis placed on just this pragmatic factor. The amount and kind of evidence currently available in the given field, the number and quality of competing beliefs, and the importance of accepting a false belief or rejecting a true one all seem central.

The emphasis of most empathy theorists on subjective judgment properly concerns not the explanatory force of an argument but the acceptability of the explanans hypotheses. When these hypotheses are statistical in nature, their acceptability requires a consideration of pragmatic criteria and hence a denial of the VN thesis. Further, when they occur in fields such as history where the amount of data is often negligible and where plausible competing hypotheses abound, the requirements of acceptable evidence must be loosened considerably. In other words, the degree of rigidity of our requirements for confirming evidence depends on these very cost factors, and hence tends to distinguish the various sciences. But this contention makes one important assumption that will have to be defended later: that the goal of science is not merely truth for its own sake but truth as modified by other criteria. We will pursue this general contention in the context of certain aspects of model (P) alluded to in the last chapter, viz., the peculiar ambiguity of statistical inference, the requirement of total evidence and the criteria of rational credibility of statistical generalizations.

As already noted, much criticism of the CL model as an explication of the logical structure of historical explanations turns on Hempel's early emphasis upon the deductive pattern and his stress on covering laws as universal generalizations of the form "All x is y." Most actual historical generalizations, notoriously, describe not invariant relations, as the deductive model requires, but at best only correlations of varying degrees of frequency.

Thus historians usually feel unaffected by the presentation of negative evidence or counterexamples to their generalizations, making refutation of their hypotheses extremely difficult. But such a situation suggests to some philosophers of history a peculiar paradox for the CL deductive model. Scriven, for instance, notes the apparent inconsistency between the existence of good explanations in historical practice and the nonexistence of comparably good laws and predictions. For, "some historical explanations appear to be so well supported by the evidence that we cannot reasonably doubt them." Yet the CL theory postulates, as a necessary condition for an explanation to be beyond reasonable doubt, that we have general laws which are also beyond doubt and license the deduction. But this condition is not met since "historians are not in possession of such general laws."[1] The paradox of the CL model, then, lies in the fact that good explanations are explicated as deriving logically from dubious general laws, from laws in which we have less confidence than the explanation they are used to support.

No doubt such a situation is largely responsible for the various moderate criticisms of the CL model, some of which were reviewed in the last chapter. But it is important to note again that not all CL theorists cling to the deductive model and its requirement of universal laws as the explanatory force of historical explanations. Hempel and Nagel, in particular, recognize in

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[1] M. Scriven, "Truisms as the Grounds for Historical Explanations," pp. 443-4.

all of their writings on this subject the need for loosening the model to allow generalizations of a statistical nature and an inductive relationship between explanans and explanandum. And Hempel, in recent writings, contributes a logical analysis of these matters by his model (P) which, you will recall, has equal status with model (D) as an irreducibly complete idealization.

The requirement of general laws is loosened to include those of the form "Almost all instances of x are instances of y" or more precisely, "The probability of an x being a y is r," i.e., $p(y, x) = r$.

Such a view clearly dulls the edge of the apparent paradox. It accounts for the historian's reluctance to reject his generalizations on the basis of a few negative instances, as well as for the difficulties in refuting such hypotheses. Statistical laws are much less dubious than universal laws, since not claiming unexceptionable or invariant relations but only correlations allowing of many exceptions. Yet for this very reason they raise serious problems concerning the epistemological notion of rejecting or accepting an hypothesis.

Moreover, this extension of the CL theory to include model (P) seems too great a concession to many who were attracted to the deductive model because of its logical elegance and its "hold" on the particular event to be explained. Scriven, you will recall, objected to model (P) on the grounds that statistical generalizations lose this hold on the particular case since the latter "rattles around" inside a network of statistical laws which fail to provide any informative relevance, any extensive understanding, for the particular case. The fundamental charge leveled against the probabilistic model rests on the fact that statistical laws, unlike deterministic ones, are compatible with both the occurrence and nonoccurrence of their explanandum-event. They fail to rule out the nonoccurrence of e, and hence fail to explain why e, in particular, occurred rather than something else. For this reason Scriven proposed his novel normic truisms as a replacement for the laws of the CL model. As a kind of logical hybrid they purportedly possess the important advantages of both universal and statistical laws, along with those of Dray's rational principles of action.

Our immediate task is to review this aspect of the situation regarding statistical laws as explanatory hypotheses. We must see how and why they lose their hold on the particular explanandum-event by their compatibility with both e and non-e, and also determine what additional requirements need be imposed on model (P) to eliminate this clearly objectionable feature. We will then be in a better position to examine the value-neutrality thesis.

Inductive Ambiguity and Total Evidence

Unlike the nomological statements adduced in the explanans of a deductive explanation, statistical nomological statements express correlations between certain attributes or properties as a specified long-range frequency. Such statements assert that if certain specified conditions are realized, say A, then an occurrence of a given kind, say B, will come about with a certain long-run relative frequency, r. If, in other words, A and B are attributes, then such a statement will take the following form: "The probability for an instance of A to be an instance of B is r," or symbolically, "p(B, A) = r." For example, the probability of the toss of a fair coin being heads is 1/2. It is also to be noted that the probability r refers not to the class of all actual instances of B, *i.e.*, to a finite class, but instead to the class of all potential instances of B. That is, the probability statement ascribes a certain disposition to the coin, *viz.*, that of yielding a head in one out of two tosses in the long run, not just in all the actual tosses of the coin.

Now it might appear as if the CL models, (D) and (P), are exactly parallel except for the different kinds of laws used in the two cases, strictly universal and statistical laws. The following two arguments, for instance, appear similar in this way.

(D_1) (a) All patients who suffer from a virus infection and are treated with penicillin are helped by penicillin.

(b) Patient x suffers from a virus infection and was treated with penicillin.

(c) Therefore, x will be helped.

(P_1) (a') The probability of being helped by penicillin when suffering from a virus infection is .9.

(b) Patient x suffers from a virus infection and was treated with penicillin.

(c) Therefore, x will be helped.

As Scriven and others point out, the truth of (a) and (b) is incompatible with the nonoccurrence of what is described by (c), since argument (D_1) is deductive. And by so ruling out the nonoccurrence of what is described by (c), (a) keeps its hold on this occurrence and hence explains why it in particular occurred rather than something else. But the truth of (a'), on the other hand, is quite compatible with the falsity of (c), since there may be another reference class, say A, to which x belongs which makes it highly probable that x will not be helped by the penicillin. If, for example, x was allergic to penicillin and if the probability of being helped by penicillin in such circumstances was extremely low, we would obviously not expect x to be helped. In such a case, the argument would have the following form.

(P_2) (a'') The probability of a patient who is allergic to penicillin being helped by penicillin is .1.

(b') Patient x was allergic to penicillin.

($\sim$c) Therefore, x will not be helped by penicillin.

Hence, since (a') is compatible with both (c) and ($\sim$c), it clearly loses its hold on the particular case and fails to explain why x was in fact helped by the penicillin. This merely notes that the improbable may be actual.

The peculiar phenomenon just illustrated, whereby model (P) allows of interpretations (P_1) and (P_2), both of which contain true premises yet the conclusions of which are inconsistent, Hempel labels the "inconsistency" or "ambiguity of statistical explanation."[2] Generally, for any argument of the form (P) with true premises, there is a competing argument of the same form also with true premises but whose conclusion is incompatible with that of the first argument. The ambiguity in question lies, of course, in the different reference classes to which our patient was assigned, and relative to which he was assigned inconsistent properties.

Furthermore, this difficulty seems to be absent from model (D) or deductive explanations. For incompatible conclusions, as (c) and ($\sim$c), can be derived only from incompatible premise-sets, and true premise-sets containing strictly universal laws can never be incompatible. That is, for model (D) to allow of interpretations parallel to (P_1) and (P_2), at least one of the universal laws, either (a) or its competitor, would have to be false. Hence, the choice of premise sets in this case is obvious: choose the true explanans and thereby eliminate the problem. But in the case of (P_1) and (P_2) the choice is not at all obvious, since both sets of premises are true. The problem and ambiguity thus remain. Little wonder then that Scriven and others find Hempel's model (P) suspect on the grounds that statistical laws lose their

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[2] C. Hempel, "Deductive-Nomological vs. Statistical Explanations," p. 127. Cf. also his "Inductive Inconsistencies," pp. 128-132; along with S. Barker, Induction and Hypothesis (Ithaca, N.Y.: Cornell University Press, 1957), pp. 75-8.

hold on the particular explanandum-event, even though such laws are widely invoked for scientific explanatory and predictive purposes.

The trouble might be thought to stem from the attempt to apply probability statements to individual events or persons, as with our patient x. For such an application is thought by some to have no meaning at all. Professor Brodbeck, following C. S. Peirce and more recently von Mises, suggests that

> From a deterministic law, given the initial conditions, we can predict an individual event. From a statistical law and its initial conditions...we can predict only a so-called mass event, that is, the frequency with which an attribute will be distributed in the given class.... From a statistical law, then, nothing can be predicted [nor explained] about an individual event.[3]

The statistical frequency of some property B in a reference class so restricted as to contain x as its only member would of course be of no use in judging the hypothetical probability that x had B. But it does not follow that the latter judgment cannot be made according to a more appropriate reference class. Moreover, as Hempel and others have argued, "there is only a difference in degree between a sample consisting of just one case and a sample consisting of many cases. And, indeed, the problem of ambiguity recurs when probability statements are used to account for the frequency with which a specified kind G of result occurs in

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[3] M. Brodbeck, "Explanation, Prediction and 'Imperfect' Knowledge," pp. 247-8.

finite samples, no matter how large."[4]

The troublesome ambiguity seems to arise then not from applying probability statements to individual cases, but instead from two different sources. One such source consists in the misleading assimilation of such arguments as (P_1) and (P_2) to deductive ones as (D_1), on the basis of the analogous construal of strictly universal and statistical laws. For such an assimilation tends to make us overlook the important fact that both 'probability' and 'certainty' are relative terms. That is, the conclusion (c) is certain only relative to the premises of (D_1) not in itself, and it is likewise highly probable (inductively) only relative to those of (P_1) and improbable only to those of (P_2), not in itself. In each case (c) is merely an elliptical formulation of a relational statement. Once this is seen, the impression that statistical explanations or arguments warrant, on the basis of true premises, the acceptance of such incompatible conclusions as (c) and $(\sim c)$ vanishes. This impression trades on interpreting the incompatible conclusions as nonrelational. The incompatibility or ambiguity dissolves since (c) is warranted by a different set of premises, those of (P_1), than those warranting $(\sim c)$, those of (P_2).

Nevertheless, even when clarifying this confusion, even when the apparent incompatibility is seen to result from relating the

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[4] Hempel, "Deductive-Nomological vs. Statistical Explanation," p. 132. Cf. also W. Rozebloom, "Comments" in _Current Issues in the Philosophy of Science_, pp. 237-41; and A. Pap, _An Introduction to the Philosophy of Science_, pp. 186-9.

conclusions to different premises, these premises remain true. Hence the problem now changes to choosing between them. We emphasized in Chapter II that a rationally acceptable explanation, deductive or probabilistic, must be one whose explanans warrants the belief that the explanandum-event did occur. In the case of deductive explanations no such problem emerges, since if incompatible conclusions are warranted by two different premise-sets, then at least one must be false and our decision obviously ought to be for the true set, if any. But, since in probabilistic explanations both sets of warranting premise-sets can be true, which of the alternative sets to choose as a rationally acceptable scientific explanation and prediction becomes a critical matter.

In such a case it is clear, as Hempel and Oppenheim recognize, that the four conditions of adequacy laid down by them are surely not sufficient conditions for an explanation to be rationally acceptable. Indeed, two sets of premises might easily meet conditions (R_1'), (R_2), (R_3) and even (R_4) (i.e., contain empirically testable and true statistical laws which inductively imply their respective conclusions), but yet imply incompatible conclusions, in which case at least one such set must be ruled rationally unacceptable. Hence, Hempel's distinction between potential and true or well-confirmed explanations, discussed in Chapter II, clearly does not suffice to mark the difference between a merely genuine and a rationally acceptable explanation. How precisely to mark this distinction, however, remains one of the most baffling and perplexing of epistemological problems.

Nevertheless, this problem of specifying criteria for rationally acceptable explanations seems, contrary to advocates of the SU thesis, in no way to affect the first three conditions. A serious problem for scientific explanations as well as for historical or social ones, it concerns basically (R_4'). Since we can never be assured of the truth of statistical laws, the question of importance is how, given two conflicting sets of empirically testable premises, to decide what credence to give to each set. How much evidence is required or sufficient to warrant a set of premises as a rationally acceptable explanation? And can such a decision be made adequately within the confines of the value-neutrality thesis? More precisely, would an adequate explication of the notion of a rationally acceptable scientific or historical explanation, and hence of such a decision to accept or reject explanatory hypotheses, require the inquirer to make value judgments?

To make a start in this direction we note first what is by now an obvious yet still important additional criterion adduced by numerous philosophers: the requirement of total evidence. Such a criterion helps to decide our residual problem concerning the ambiguity or inconsistency of statistical explanations. For, even when given the premises of (P_1) and (P_2) as true, neither would be adjudged acceptable without considering further relevant evidence. Otherwise, we run the risk of selecting only those true or well-confirmed statements as explanatory hypotheses which favor one's biases or desired conclusions.

To control such bias Carnap, among others, proposes that "in the application of inductive logic to a given knowledge situation, the total evidence available must be taken as basis for determining the degree of confirmation."[5] Hempel adds the modification that a smaller part, e_1, of the total evidence may be used if the remaining part, e_2, of the total evidence is inductively irrelevant to the explanatory hypothesis h whose confirmation is to be determined. Using Carnap's notion of inductive probability, "the irrelevance of e_2 for h relative to e_1 can be expressed by the condition that $c(h, e_1 \cdot e_2) = c(h, e_1)$."[6] Hence, our residual problem dissolves since, *e.g.*, (P_1) and (P_2) cannot both satisfy the requirement of total evidence. For the total evidence, as one body of consistent evidence, cannot confer high probabilities on two contradictory statements, as (c) and (~c), since the two probabilities add up to 1. But just this would be the consequence if both (P_1) and (P_2) satisfied the requirement.[7]

At this juncture it will be helpful to consider the nature of this requirement of total evidence, its epistemological status and also the kind of support that can be adduced for it. Especially instructive is the criticism of this principle recently leveled by A. J. Ayer as part of an attack against Carnap's notion of inductive

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[5] R. Carnap, *Logical Foundations of Probability* (Chicago: University of Chicago Press, 1950), p. 211. Cf. also A. Pap, *op. cit.*, pp. 187-9; R. Chisholm, *Perceiving: A Philosophical Study* (Ithaca, N.Y.: Cornell University Press, 1957), pp. 25-27; and S. Barker, *op. cit.*, pp. 76-78.

[6] Hempel, "Deductive-Nomological vs. Statistical Explanation," p. 138.

[7] Hempel, "Inductive Inconsistencies," p. 141.

or logical probability. But for our purposes we will limit the discussion to Ayer's challenging question "Why *have* we to take as evidence the total evidence available to us...?"[8] Ayer intends not to deny the legitimacy of the principle but only to deny its justifiability on the basis of Carnap's principles of inductive logic, and hence to question the relevance of logical probability as the "guide of life" or as a basis for action.

Now in Chapter II, we referred to Carnap's distinction between internal and external questions, between questions which occur within a linguistic framework and presuppose the framework or categorial principles and questions about the acceptability of this categorial framework. Put into this context, Ayer seems to imply that a categorial principle, *viz.*, the principle of total evidence, cannot be justified within the system. And indeed he is correct. But the import of his implication is negligible, since based on a confusion of an external question with an internal one. For clearly the principle of total evidence constitutes part of the framework of inductive probability, and as such, in Carnap's words, "is not a rule of inductive logic, but of the methodology of induction."[9] Hempel further clarifies the external nature of the principle by taking it as "a partial explication of the conditions governing rational belief and rational choice." Such an

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8 A. J. Ayer, "The Conception of Probability as a Logical Relation," in E. Madden (ed.), *The Structure of Scientific Thought* (Boston: Houghton, Mifflin, 1960), p. 281.

9 Carnap, *Logical Foundations of Probability*, p. 211.

explication "specifies a necessary, though not sufficient, condition for the rationality of inductive beliefs and decisions." Its acceptance is, as noted earlier, "pragmatic in character and cannot be defined in terms of the concepts of formal (deductive or inductive) logical theory."[10]

Notice in particular that if CL theorists were merely explicating the notion of formally sound potential or genuine explanations, the principle of total evidence would be unnecessary, along with the pragmatic dimension of explanation. But once they open the question to rationally acceptable explanations, to the acceptability of statistical explanatory hypotheses, then the principle of total evidence or some substitute must be invoked. With it comes a serious challenge to the value-neutrality thesis. For an adequate explication must now include an analysis of the acceptability of explanatory hypotheses, hence cannot exclude the pragmatic dimension as merely psychological or presystematic or merely extra-logical. Such a dimension is of course not purely formal. But this only concedes that the required explication is not of a purely formal notion. The explicandum, 'scientific explanation,' is essentially a pragmatic and methodological as well as a logical term. Hence any adequate explicatum must account for, not merely explain away, the fact that some extra-logical dictate of purpose or interest is required to direct our scientific acceptance or rejection of hypotheses.

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10 Hempel, "Inductive Inconsistencies," pp. 142-3.

Moreover, it is but a step, though perhaps a long and difficult one, from the inclusion of pragmatic elements in an adequate explication of rationally acceptable explanation to the denial of the value-neutrality thesis as an essential part of such an explication. An indication of things to come can be gleaned in C. I. Lewis' version of the principle of total evidence.

> No inductive conclusion is well taken and justly credible unless the obligation to muster all the given and available evidence which is relevant to this conclusion has been met....Indeed this principle of the required completeness of available and relevant evidence for the justified credibility of inductive conclusions, has a character which is plainly akin to the moral. It is unlike the textbook rules...and has instead the character of a maxim.[11]

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11 C. I. Lewis, The Ground and Nature of the Right (New York: Columbia University Press, 1955), pp. 32-33.

Rational Credibility and Utilities

Lest we slide too quickly to our conclusion without the proper support, however, let me hark back to Scriven's objection to the requirement of total evidence, mentioned in the last chapter. His criticism rests with the claim that the principle has "the drawback of being virtually unattainable." From this claim he infers correctly that the CL theory needs modification to include the pragmatic element of judgment. With this claim and inference we can but concur. But we shall argue that such modification necessitates only the denial of the value-neutrality thesis, not, as he further concludes, a revamping of (R_2) by replacing empirical covering laws with normic truisms or principles of action.

Now, Scriven's objection to the principle of total evidence is conceded by Hempel, Carnap, Barker and others. It points to the unavailability, at least at present, of an appropriate general system of inductive logic whose rules would enable us to show that the part, e_2, of our total evidence going beyond our premises is inductively irrelevant to our conclusion. Barker,[12] in particular, shows that all attempts to utilize the principle in appraising the rational acceptability of statistical arguments like (P_1) and (P_2) meet with serious difficulties. More generally, even if we adopted any straightforward principle of induction by simple enumeration, we would still be plagued by inductive inconsistencies, since we

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12 S. Barker, Induction and Hypothesis, pp. 76-78.

possess no clear criterion for deciding upon the most appropriate reference class. Thus Scriven seems to be correct in charging that the principle is "virtually unattainable," and also in concluding that the CL theory needs a more manageable substitute for the principle, and reinforcement by additional conditions for rationally acceptable explanations.

Any such substitute and reinforcement should of course be applicable to simple statistical arguments like (P_1) and (P_2). Hempel has recently proposed a rough substitute for the principle of total evidence as a criterion of evidential adequacy which, though it avoids many of the embarrassments created by the principle, must be used with caution, qualification and discretion. His modified version requires two conditions to be met in order for statistical systematizations containing, say, the premises 'Fb' and 'p(G, F) = r' to meet the principle of total evidence.

> (i) the total evidence e contains (i.e., explicitly states or deductively implies) these two premises; (ii) e implies that F is a subclass of any class F* for which e contains the statement F*b and in addition a statistical law (which must not be simply a theorem of formal probability theory) stating the value of the probability p(GF*) [13]

However, even if this admittedly rough criterion were an adequate substitute for the requirement of total evidence, other conditions would still have to be satisfied by a rationally acceptable explanation. Such reinforcement also accords with Scriven's criticism of the principle. But the central question

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13 Hempel, "Deductive-Nomological vs. Statistical Explanation," pp. 148-9.

here is whether such reinforcement entails any serious logical or epistemological modifications of the CL theory of explanation. Hempel argues for a negative answer; Scriven for an affirmative one. So, having reviewed Scriven's unsuccessful defense in the last chapter, we turn now to examine Hempel's case. We want to see in particular the grounds for the CL defense of the value-neutrality thesis. We also want to determine whether covering statistical laws can provide the informative relevance of explanations to the particular case, without conceding any major point of the SU thesis.

What other conditions, then, besides the requirement of total evidence must be satisfied by a rationally acceptable explanation? And do such residual conditions merely determine the degree of confirmation of explanatory hypotheses? Or do they, instead, authorize the provisional acceptance of such hypotheses on a given body of evidence which supports but does not logically prove them, where the acceptance of an hypothesis is taken as a case of a purely epistemic or theoretical choice between competing scientific hypotheses? Or, finally, do they authorize acceptance of such hypotheses in a sense which amounts to the adoption of a certain course of action? The grounds for the latter alternative might be that accepting an hypothesis in an open-ended situation, where no course of action is specified, makes no clear sense, and hence must be viewed as a pragmatic notion.

If either of the first two alternatives can be successfully defended, the value-neutrality thesis might survive. But if the

third alternative is cogent, as we hope to show, it argues strongly for the view that an adequate explication of the CL theory entails a denial of the thesis. For on this alternative other conditions than evidential strength are necessary for an explanation to be rationally acceptable. Let us look then at Hempel's most recent analysis of this situation, and specifically at his account of criteria of rational credibility and rules for the inductive acceptance of explanatory hypotheses.

Following Carnap, he takes our question to concern the methodology of induction, "the application of inductive logic to the formation of rational belief."[14] In so doing he interprets scientific knowledge along the lines of an "accepted-information model" or schematization, and hence establishes rules of acceptance or rejection regulating membership in the body of scientific knowledge at any given time. Taking K to represent the total body of scientific statements accepted as true by scientists at a given time (whether or not these statements are true), he proposes three general necessary conditions or rules of inferential acceptance regulating membership in K:

> (CR_1) Any logical consequence of a set of accepted statements is likewise an accepted statement; or, K contains all logical consequences of any of its subclasses.

> (CR_2) The set K of accepted statements is logically consistent.

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14 Hempel, "Inductive Inconsistencies," p. 151.

(CR_3) The inferential acceptance of any statement L into K is decided on by reference to the total system K (or by reference to a subset K' of it whose complement is irrelevant to L relative to K').[15]

More specific rules are then established by rendering the problem as a special case of formulating rules for rational decision between several alternatives. It becomes a case of theoretical choice between accepting a new hypothesis h into K, rejecting it in the sense of accepting -h, or suspending judgment by accepting neither h nor -h. The problem of rational choice, of specifying rules of decision, is posed in the following schematic fashion:

> An agent X has to choose one out of n courses of action, $A_1, A_2, \ldots, A_n$, which, on his total evidence e, logically exclude each other and jointly exhaust all the possibilities open to him. The agent contemplates a set $O_1, O_2, \ldots, O_m$ of different possible 'outcomes' which, on e, are mutually exclusive and jointly exhaustive....Then for any one of those actions, say A_j, and any one of those outcomes, say O_k, the given system of inductive logic demands a probability for the hypothesis that, given e, A_j will lead to the outcome O_k. Indeed, if aj and ok are statements describing A_j and O_k, respectively, that probability is given by $C(O_k, e \cdot aj)$.[16]

Nevertheless, Hempel and Carnap clearly recognize the inadequacy of the system of inductive logic to determine a rational course of action for X. Rationality, as a relative concept, depends on the agent's goals or objectives, on the value or utility he attaches to the outcomes which might result from his action. Carnap therefore assumes that these values can be

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15 Hempel, "Deductive-Nomological vs. Statistical Explanation," pp. 150-1.

16 Hempel, "Inductive Inconsistencies," p. 152.

represented by a quantitative notion of utility. The value of each outcome $O_1, O_2, \ldots, O_n$ for X is assigned a real number, say uk, as the utility of, say O_k, for X at the time in question. Carnap then proposes a general decision rule to determine which of the available courses of action is rational to choose in the circumstances: the action maximizing or offering him the highest expectation value of the utility attached to action Aj for X: "$u'(Aj, e) = C(o_1, e \cdot aj) \cdot u_1 + \ldots + C(o_m, e \cdot aj) \cdot um$."[17]

Applying this schema and maxim to the problem of establishing acceptance rules for scientific explanatory hypotheses, Hempel views the decision to accept, reject or suspend judgment on an hypothesis as a special kind of scientific choice. Such choice has three possible outcomes: K enlarged by the contemplated hypothesis h; K enlarged by the contradictory of h; K unchanged. The problem, accordingly, is which scientific hypothesis to accept and thus add to the body of scientific knowledge. In particular, the issue concerns what utilities to assign to these outcomes and, more importantly, what kinds of values the assigned utilities are to represent. Are they to concern merely the quest of truth for its own sake, of accepting as new information into K only true statements; or are such other aspects of science as simplicity and explanatory power also to be considered? Or are they to concern, in addition to these purely scientific or epistemic utilities, other cost factors and pragmatic gains and losses, i.e.,

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[17] Ibid., p. 153; cf. also Carnap, Logical Foundations of Probability, p. 269.

utilities resulting from acting on the basis of the hypotheses in question? We will pursue these questions in detail in the remaining sections of this chapter. Regardless of the outcome of this discussion, one fact of importance emerges already. Once the notion of explanation is expanded to include the distinction between genuine and rationally acceptable explanations, as CL theorists do, the entire theory of explanatory inference is left in a highly unsatisfactory and incomplete state. That is, unless we can formulate conditions under which an explanation satisfies certain purposes and does so with maximum efficiency.

Such conditions seem, generally, to cast a peculiar light on the degree of evidential strength or confirmation required to make an explanatory hypothesis acceptable. For to make such an appraisal, we would have to know both how efficiently the hypothesis satisfied our goals or purposes and also how important our varied goals or objectives are. But in this last consideration lies what I take to be the essential defect of the value-neutrality thesis, as advocated by Weber and CL theorists. In order to elicit the issue more clearly, let me consider in detail the kind of argument I find most compelling as an attack on the thesis in question. And in so doing, it will be necessary to contrast it with some weaker arguments used to derive the same conclusion, the denial of VN. But Weber and CL theorists successfully counter these weaker arguments. For they usually locate the value element in the context of discovering or imaginatively constructing plausible explanatory hypotheses. Since the VN thesis concerns only the justification,

corroboration, confirmation or rational acceptability of explanatory hypotheses, these usual arguments miss their mark. Accordingly, any argument successfully opposing the VN thesis must locate the value element in the very logic of explanation. Let us look then at one such recent argument.

Explanation and Value-Neutrality

The value-neutrality thesis, you will recall from the first chapter, maintains that the scientist's value scheme be logically divorced from scientific standards of explanatory validity and reasonable or warranted belief. It requires scientific inquiry to be objective in the sense that the scientist remain evaluatively neutral when appraising the acceptability or rational correctness of his explanations, and also in the sense of yielding the same conclusion for all competent inquirers, independently of variable personal interests, attitudes or values. Now, to oppose this thesis, it must be shown that two scientific inquirers with the same evidence and the same probability assignments might nonetheless disagree about the acceptability of a given explanatory hypothesis, and do so on rational grounds. Only in this way will one's values emerge as a basis for logically appraising the evidence, rather than merely for choosing problems to investigate or for even having a science at all. Value decisions in the latter roles can and have been, by CL theorists, relegated to the psychology or sociology of science, hence keeping science proper untainted, *i.e.*, objective and value-free. In other words, it must be shown that the scientist *qua scientist* makes value decisions and judgments, if we are to challenge the tenability of the VN thesis.

Consequently, the case seems best argued on the basis of recent work in statistical analysis concerned with the problem of rational decision-making in the face of uncertainty. This type of argument has been advocated by such experimentalist philosophers as Braithwaite, Churchman, Frank and Rudner.[18] It builds on the earlier statistical work of Neyman, Savage and Wald,[19] especially in application to problems of quality control and more generally to problems of nondeductive inferences. These innovations in the logic of statistical inference--based upon experimentally controllable teleological concepts and hence capable of supplying a model broad enough to confront the issue of a theory of experimental action--seem a much more profitable point of departure for methodology than the largely speculative position of Hume and J. S. Mill.

Applications of statistical procedures are to be found, of course, in physics, genetics and the social sciences where a major goal is to decide what to believe. But they are also used in determining insurance rates and in market research where the goal is to determine how to act. Hence, the basic problem of statistics, rational decision-making in the face of uncertainty, suggests a structural similarity between these different kinds of problems: how to act and what to believe in the face of uncertainty. Accordingly, R. Rudner, summarizing much of this work, formulates

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[18] R. Braithwaite, Scientific Explanation, Chapter VII; C. W. Churchman, Theory of Experimental Inference, Chapters 14-15; C. W. Churchman, "Statistics, Pragmatics, Induction;" P. Frank, Philosophy of Science, Chapter 15; R. Rudner, "The Scientist Qua Scientist Makes Value Judgments."

[19] J. Neyman, A First Course in Probability and Statistics (New York: Holt and Co., 1950); L. Savage, The Foundations of Statistics (New York: Wiley and Sons, 1954); A. Wald, On the Principles of Statistical Inference (South Bend, Ind., Notre Dame University Press, 1942).

in the following passage what I take to be the most explicit and compelling attack against the value-neutrality thesis.

> Now I take it that no analysis of what constitutes the method of science would be satisfactory unless it comprised some assertion to the effect that the scientist as scientist accepts or rejects hypotheses.
>
> But if this is so then clearly the scientist as scientist does make value judgments. For, since no scientific hypothesis is ever completely verified, in accepting a hypothesis the scientist must make the decision that the evidence is *sufficiently* strong or that the probability is *sufficiently* high to warrant the acceptance of the hypothesis. Obviously our decision regarding the evidence and respecting how strong is 'strong enough,' is going to be a function of the *importance*, in the typically ethical sense, of making a mistake in accepting or rejecting the hypothesis....*How sure we need to be before we accept a hypothesis will depend on how serious a mistake would be.*[20]

But since Rudner's case is incomplete as it stands and has subsequently been subjected to sustained counterattack in the recent literature by I. Levi,[21] a spokesman of the Hempel-Carnap view, it will perhaps be instructive to unpack the argument in some detail. We will then be in a better position to assess Hempel's and Levi's defense of a value-free science by locating the issues more clearly.

In the above-quoted passage four statements are explicitly noted as premises from which the conclusion, that the scientist *qua* scientist must make value judgments, is claimed to follow. Let us list these as follows:

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[20] Rudner, *op. cit.*, p. 2.

[21] I. Levi, "Must the Scientist Make Value Judgments?," *Journal of Philosophy*, Vol. LVII (1960); "Decision Theory and Confirmation," *Journal of Philosophy*, Vol. VIII (1961); and "On the Seriousness of Mistakes," *Philosophy of Science*, Vol. XXIX (1962).

(1) "The scientist as scientist accepts or rejects hypotheses."

(2) "No scientific hypothesis is ever completely verified."

(3) Therefore, "the scientist must make the decision that the evidence is sufficiently strong or that the probability is sufficiently high to warrant the acceptance of the hypothesis."

(4) The "decision regarding the evidence and respecting how strong is 'strong enough' is going to be a function of the importance, in the typically ethical sense, of making a mistake in accepting or rejecting the hypothesis."

(5) Therefore, "the scientist as scientist does make value judgments."

Now to some elaboration. Premise (2) states a central tenet of empiricism to which, as we saw in Chapter II, both Popper and Hempel, along with all other CL theorists, subscribe. In accord with Hempel's requirement (R_3), it is but one version of the fallibilist claim that all scientific explanatory hypotheses are empirical and hence corrigible. No such hypothesis is ever without risk or ever completely confirmed by any amount of evidence, but can instead at best be rendered more or less probable. With (2) undisputed by CL theorists, the acceptability of (3) depends only upon that of (1).

Moreover, if corrigibility or the chance of making an error were the only relevant consideration for the decision required by (3), the scientist would never reach any decision. He would merely keep increasing the amount of evidence indefinitely, i.e., until per impossible by (2) he attained certainty, before deciding to accept or reject the hypothesis. But if (1) is true, if the scientist must decide in the face of uncertainty, then some additional factor must be considered in order for a decision to be made. And since it seems, at least prima facie, unreasonable to decide arbitrarily on the basis of sheer convention to accept only hypotheses with, say, more than 0.5 degrees of probability, the statistician's suggestion that the additional factor be some measure of utility gains support. Since Hempel and Carnap concede this point, as indicated in the last section, we can for our purposes take it as established that the decision required by (3) is a function of some kind of utility. That is, the scientist must make a utility judgment or decision, granted of course that (1) holds.

How then to defend (1)? The issue here turns on how to interpret the function of the scientist. For to defend the value-neutrality thesis, i.e., to deny (5), by rejecting (1), that the scientist accepts or rejects hypotheses, commits one to the "guidance-counselor" view of the scientist. This in fact is the view advocated by both Carnap and Hempel.[22] Instead of accepting or rejecting hypotheses, the scientist, on this version, simply assigns degrees of confirmation to them relative to the total available evidence, and hence serves only as an advisor to policy makers who might want to apply such information to practical affairs. Carnap even defines logical or inductive inference in just this way, so that all probabilistic explanatory inferences

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[22] Carnap, Logical Foundations of Probability, pp. 241-270; Hempel, "Review" of Churchman's Theory of Experimental Inference, in Journal of Philosophy, Vol. XLVI (1949), p. 560.

in science consist not in an attempt to replace doubt by true or reasonable belief, but in assigning degrees of confirmation to hypotheses relative to the given evidence. Hence, if this analysis of the scientific task is convincing, the objection to (1), and thus to (3), concedes that the scientist qua policy maker must make the decision required by (3), yet insists that the scientist qua scientist, i.e., qua guidance-counselor, merely determines the degree of probability for an hypothesis. The value-neutrality thesis would thereby be defended at the price of giving up (1). Determining how costly this price is and whether we would be wise to pay such a price become the immediate problems.

No doubt the issue as to the aim and function of science is embarrassingly complex and difficult. At the present time we have no adequate answer. However, two considerations seem to argue against the Carnap-Hempel denial of (1). First, Rudner suggests that even the assignment of probabilities to an hypothesis is in effect the acceptance of an hypothesis, albeit of a different one. That is, "the determination that the degree of confirmation is say, p,...which is on this view being held to be the indispensable task of the scientist qua scientist, is clearly nothing more than the acceptance by the scientist of the hypothesis that the degree of confidence is p...."[23] So, even if the task of the scientist is to assign degrees of confirmation, this requires him to accept some hypothesis and hence to make the kind of decision involved in (3).

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[23] Rudner, op. cit., p. 4.

But, secondly, it has already been shown that Hempel's analysis of explanation itself entails that the scientist do more than assign degrees of confirmation. For in our earlier discussion (Chapter II, Section 3) we had occasion to ask whether Hempel intended his theory of explanation as an explication of 'an explanation' or of a 'rationally acceptable explanation.' The result of that examination showed that Hempel clearly intends the latter, especially when he distinguishes between potential explanations which satisfy conditions (R_1) - (R_3) on the one hand, and well-confirmed or true explanations which satisfy in addition either (R_4') or (R_4) on the other. This is further evidenced when he invokes as a general and necessary condition of adequacy for any rationally acceptable explanation of a given event x that the explanation "must provide information which constitutes good grounds for the belief that x did in fact occur."[24] Hempel has never been satisfied to analyse the notion of explanation merely in terms of a valid argument containing empirical laws among its explanans. Instead, he also insists that the explanans be acceptable in order to provide good grounds for the explanandum, and hence to be a "correct" explanation.

Further, when attempting to eliminate the inconsistency of inductive inference, Hempel again acknowledges the inadequacy of (R_1) - (R_4) as sufficient conditions of rationally acceptable explanations, and supplements them with the principle of total

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[24] Hempel, "Reasons and Covering Laws in Historical Explanation," p. 146.

evidence. This immediately goes beyond the scope of inductive logic proper to the methodology of induction and to general pragmatic considerations. Now, if his task were just to explicate the notion of a formally sound (deductively or inductively) potential explanation, then pragmatic and methodological matters would be irrelevant. The scientist might escape by simply assigning degrees of confirmation to explanatory hypotheses. But since he intends to explicate the notion of an acceptable explanation, which requires acceptable statistical hypotheses as premises, the principle of total evidence and other methodological principles must be invoked.

To agree therefore that these are not purely formal logical principles is only to concede that the explicandum in question is essentially a pragmatic as well as a syntactical and semantical term. And this amounts to Rudner's claim in premise (1), that an adequate explication or rational reconstruction of scientific procedures must include a statement to the effect that the scientist as scientist accepts or rejects hypotheses. The price of giving up (1), then, seems to be restricting the CL theory to genuine or even formally valid explanatory arguments, a price Hempel wisely refuses to pay.

If these considerations are cogent, (3) follows as a consequence. That is, we seem committed to viewing the scientist not just as a counselor of policy makers, but as himself a decision maker. The scientist qua scientist has then as one of his major goals to replace doubt by true or reasonable belief, and to do this by deciding in the face of uncertainty when the evidence for an explanatory hypothesis suffices to warrant his belief in that hypothesis. Further, since (5) obviously follows from (4) and with (3) already established, the tenability of Rudner's argument rests with the move from (3) to (4), from the fact that scientists must make decisions to the fact that these decisions are a function of the seriousness of error.

Unfortunately, Rudner offers no argument in the article cited for this central connection. Instead, he fills the gap by appealing to examples from quality control with some of the guiding theories of statistical inference. "If," to take one of his cases,

> the hypothesis under consideration were to the effect that a toxic ingredient of a drug was not present in lethal quantity, we would require a relatively high degree of confirmation or confidence before accepting the hypothesis--for the consequences of making a mistake here are exceedingly grave by our moral standards. On the other hand, if say, our hypothesis stated that, on the basis of a sample, a certain lot of machine stamped belt buckles was not defective, the degree of confidence we should require would be relatively not so high.[25]

Thus, taking such an example as a paradigm case of scientific inquiry, he assimilates cases of deciding what to believe to cases of deciding how to act in the face of uncertainty, and concludes that the seriousness of making a mistake, the cost or ethical factor, must be considered in any scientific assessment of statistical explanatory hypotheses.

What is unfortunate in the appeal to examples at this crucial stage in the argument, however, is not that there are no better

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[25] Rudner, op. cit., p. 2.

grounds for moving from (3) to (4). For indeed there are. Rather, the suppression of the needed assumptions has led defenders of the VN thesis to serious misunderstandings. As a result they reconstruct Rudner's argument by appealing to altogether unnecessary premises in order to fill the gap, and then proceed to challenge these very premises. Professor I. Levi, for instance, clearly sees the required connection between believing or accepting an hypothesis and acting on the basis of the hypothesis relative to some objective or goal. This stresses again the need to interpret explanatory hypotheses, at least in part, methodologically as means or instruments adopted in order to achieve some specified objectives. Rudner's argument assumes, and Hempel concurs, that scientific questions regarding explanation are raised for some purpose, that all problem-solving is an aspect of purposive behavior, and that assessing the rational acceptability of explanatory hypotheses requires weighing cost or utility factors in addition to degrees of probability or confirmation. Hence the relevance of decision theory to the logic of inductive inference rests on its applicability to inductive behavior.

But when Levi states explicitly the assumptions necessary to move from premise (3) to (4) and hence to the denial of the value-neutrality thesis (5), he commits Rudner and Churchman to two additional premises, one of which is unduly and unnecessarily strong, but which is nevertheless made the center of controversy. The two additional assumptions cited by Levi are as follows:

(6) "To choose to accept a hypothesis H as true (or to believe that H is true) is equivalent to choosing to act on the basis of H relative to some specific objective P."

(7) "The degree of confirmation that a hypothesis H must have before one is warranted in choosing to act on the basis of H relative to an objective P is a function of the seriousness of the error relative to P resulting from basing the action on the wrong hypothesis."[26]

Now (7) he accepts on the authority of the statistical theories of such statisticians as Pearson, Neyman and Wald. But since (7) without (6) fails to yield (5), the tenability of Rudner's case for (5) is made to rest on (6), on an equivalence between believing an hypothesis and acting on the basis of it relative to some objective P.

But whether or not Rudner or Churchman would accept (6) as an adequate formulation of their suppressed premise is simply not the question at issue. For a weaker statement will suffice. Hence Levi raises a false issue and thereby confuses what I think are the important issues. Let me clarify. Levi assumes that the only way to derive the desired conclusion (5) is on the basis of a "behavioralist" analysis of belief, whereby "accepting a proposition H as true" is synonymous with, equivalent to, equated with, or reducible to "acting on the basis of H relative to a practical objective P."[27]

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[26] Levi, "Must the Scientist Make Value Judgments?", p. 348.

[27] Levi, "Decision Theory and Confirmation," p. 615; and "On the Seriousness of Mistakes," pp. 48 and 50.

Yet such an assumption is not essential in order to yield (5). It is unnecessary, in other words, to take a behavioralist view of belief. The only condition required for Rudner's argument is that belief be tied to action in such a way that beliefs can be justified by practical considerations of action, and that different mistakes in action be taken with different degrees of seriousness, so that the acceptance of beliefs is not merely the result of a quest for truth and nothing but the truth. In place of the equivalence condition in (6) an implication suffices:

> (6') To choose to accept a hypothesis H as true (or to believe that H is true) implies as a necessary (but not necessarily a sufficient) condition the disposition to act on the basis of H relative to some specific objective P.

Once the substitution of (6') for (6) is made, the issue no longer turns on a behavioralist reduction of belief to action, but clearly focuses on the question posed by Hempel's attempt to reinforce the principle of total evidence by formulating acceptance rules for rational belief, *viz.*, what kinds of values the assigned utilities are to represent. Three different kinds of values seem relevant: truth for its own sake, truth tempered by such other theoretical concerns as simplicity and explanatory power, and truth tempered by these theoretical concerns in addition to such pragmatic and practical concerns as the seriousness for action in making mistakes.

Once again, however, confusion threatens. For Rudner's premise (4), which invokes the notion of the seriousness or importance of error, does not require pragmatic costs or utilities to replace truth, high confirmation, simplicity or even explanatory power as appropriate criteria for appraising the rational acceptability of hypotheses. Nor does it entail defining any of these properties in terms of pragmatic utility. In fact, in another article Rudner explicitly refers to three independent weights necessary for an adequate theory of inductive inference and for the assessment of rational choice among explanatory hypotheses: evidential strength, simplicity and pragmatic utility.[28]

Having warded off the false issues concerning the move from (3) to (4)--a behavioralist reduction of belief to action and a pragmatic reduction of truth to utility--let us return to the main issue at hand, to our incompleted discussion in the last section of Hempel's "accepted-information model" and his notion of "epistemic utility." Rudner's argument entails that scientists do not rationally accept hypotheses as an outcome of a quest for truth and nothing but the truth, untempered by such other factors as simplicity, cost of explanatory power. Since Hempel's "accepted-information model" assumes they can, this conflict serves as a firm basis from which to begin a defense of (4).

Suppose, then, an inquirer to be seeking the truth and nothing but the truth, to be concerned only with adding true or false statements into the body of knowledge K, or, in Levi's phrase, to be "concerned to select from a given list the one and only one proposition that is true regardless of any other properties

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[28] Rudner, "An Introduction to Simplicity," Philosophy of Science, Vol. XXVIII (1961), p. 110.

that it might have."[29] To be rational in this case, the inquirer seems committed to weighing each possible mistake he might make as equally serious. But such a commitment, on Rudner's argument, cannot be satisfied since different mistakes have different degrees of seriousness or importance. It will be helpful at this juncture to see how Hempel develops his notion of epistemic utility, in order to exhibit the relative weakness of his position and the strength of Rudner's.

In the last section we noted that the problem of establishing acceptance rules for scientific explanatory hypotheses, of deciding which scientific hypotheses to accept and thus add to the body of scientific knowledge, turns on what kinds of values the utilities assigned to outcomes are to represent. Hempel's answer is clear:

> the utilities should reflect the value or disvalue which the different outcomes have from the point of view of pure scientific research rather than the practical advantages or disadvantages that might result from the application of an accepted hypothesis, according as the latter is true or false. Let me refer to the kind of utilities thus vaguely characterized as *purely scientific, or epistemic, utilities*.[30]

The problem then concerns finding a measure of the epistemic utility of adding an hypothesis h to the previously established system of knowledge K.

But Hempel immediately recognizes the need to make such utilities depend on how much of what h asserts is new information

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[29] Levi, "On the Seriousness of Mistakes," pp. 49 and 51.

[30] Hempel, "Inductive Inconsistencies," p. 154.

not already contained in K. Taking k as a sentence with the same informational content as K, the common content of h and K is given by $h \vee k$. Thus, since h is equivalent to $(h \vee k) . (h \vee -k)$, the content of h which goes beyond that of K is expressed by $(h \vee -k)$. Hempel then introduces the notion of a "content measure function for a [suitably formalized] language L," *i.e.*, a function m assigning to every sentence s of L a number m (s) such that "(i) m (s) is a number in the interval from 0 to 1, inclusive of the endpoints; (ii) m (s) = o just in case s is a logical truth; (iii) if s_1 and s_2 have no common content--*i.e.*, if the sentence $s_1 \vee s_2$, which expresses their common content, is a logical truth--then $m(s_1 \cdot s_2) = m(s_1) + m(s_2)$."[31]

If m is then used as a content measure for a language suited to the purposes of empirical science, the utility of adding h to K is given as the

> Tentative measure of epistemic utility: the epistemic utility of accepting a hypothesis h into the set K of previously accepted scientific statements is $m(h \vee -k)$ if h is true, and $-m(h \vee -k)$ if h is false; the utility of leaving h in suspense, and thus leaving K unchanged, is 0.[32]

Finally, Carnap's rule of maximizing estimated utility, mentioned earlier, warrants the acceptance or rejection of h as epistemically rational in accordance with the following "Tentative rule for inductive acceptance: Accept or reject h, given K, according

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[31] *Ibid.*, p. 154; and also Hempel, "Deductive-Nomological vs. Statistical Explanation," p. 154.

[32] *Ibid.*, p. 154.

as $c(h, k) > 1/2$ or $c(h, k) < 1/2$; when $c(h, k) = 1/2$ h may be accepted, rejected, or left in suspense."[33] Since on this account a scientist is interested only in accepting h when it is true, the possible correct answers of accepting h when true and rejecting h when false are to be considered equally desirable and the corresponding mistakes equally undesirable. Hence, we are led to the unsettling recommendation of accepting, i.e., acting on the basis of, h when its degree of confirmation or probability is merely 0.51.

Both Hempel and Levi readily concede the difficulty with this account: the rule is much too liberal or lenient to be suitable for even pure scientific procedure. Further, Levi points to the same problem when other methods are used instead of Carnap's maximizing utility, e.g., Bayes method and the method of significance testing proposed by Neyman and Pearson. He also acknowledges that accepting h "when its degree of confirmation is low does not seem reasonable unless this acceptance is reduced to action undertaken to realize some objective other than seeking the truth and nothing but the truth,"[34] e.g., formal or structural simplicity and explanatory power. With this Hempel concurs. Levi, however, pursues the third option, suspended judgment or remaining in doubt, as a way of avoiding the difficulty and hence of defending the view that scientists accept or reject hypotheses in quest of the

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33 Ibid., p. 155.

34 Levi, "On the Seriousness of Mistakes," p. 55.

truth and nothing but the truth. In this sense, the scientist is to suspend judgment on h when he feels the total available evidence warrants neither the acceptance nor rejection of h.

Consider one of Levi's examples of the Bayes method applied to the problem of replacing doubt by true belief. Suppose an experimental psychologist wants to determine whether or not some person has extrasensory perception. By assuming that the subject has ESP if he guesses correctly the colors of cards drawn randomly with a frequency greater than .6, and otherwise not, the psychologist must decide on the basis of a sample of guesses whether the long-range frequency of correct guesses is .6 or less (H_1), or is greater than .6 (H_2), when A_1 is the act of accepting hypothesis H_1.

> Since the experimenter presumably is interested only in accepting that hypothesis as true which is true, he should consider the possible correct answers o_{11} (accepting H_1 when it is true) and o_{22} (accepting H_2 when it is true) as equally desirable and the corresponding 'mistakes' or 'errors' o_{12} and o_{21} as equally undesirable....Consequently, the matrix for this problem will be as follows:
>
> | | H_1 | H_2 |
> |---|---|---|
> | A_1 | 1 | 0 |
> | A_2 | 0 | 1 |
>
> The Bayes method recommends adopting A_1 if the probability of H_1 is greater than 1/2 and A_2 if the probability of H_1 is less than 1/2. Consequently if the probability of H_2... is .51, we would be warranted in accepting H_2 (adopting A_2).[35]

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[35] *Ibid.*, p. 54.

Now, to avoid this unsettling and unreasonable consequence and yet preserve application of the method to cases where the goal is to seek the truth and nothing but the truth, Levi revises the problem by adding a third option: the act S of suspending judgment or remaining in doubt. In this case the matrix changes to the following:

	H_1	H_2
A_1	1	0
A_2	0	1
S	k	k

where the utility k is the value of act S when the hypothesis H_1 is true. But the price of thus avoiding the unsettling consequence, by assigning a sufficiently high value to k so that a high degree of confirmation is required to warrant the acceptance of any hypothesis, consists in selecting arbitrarily the value assigned to k.

However, this arbitrary assignment brings us back to the essential point of Rudner's move from premise (3) to (4), from the fact that scientists must make decisions (which Levi concedes) to the fact that these decisions are a function of the seriousness of error. For Rudner's argument hinges on the assumption that an arbitrary selection of a value to be assigned to k in such cases as the above is unreasonable, especially if there are grounds for making such assignments of values. This is why premise (6'), which connects beliefs or the acceptance of hypotheses with acting on the basis of them relative to some goal, provides the crucial link

between the decision to accept in (3) and the different degrees of seriousness of error in (4). Instead of assigning a value to k arbitrarily or on the basis of sheer convention, we take our cue from (6'), from the fact that belief implies a disposition to act. Our choice of a value for k then depends upon how serious a mistake would be if we acted upon the hypothesis, i.e., if we accepted a false hypothesis or rejected a true one. This in turn depends upon how useful the hypothesis in question is to us. So, while on purely logical grounds the value assigned to k is arbitrary, still a rational choice of assignments can and must be made on extralogical grounds, viz., on the basis of pragmatic utilities: the purposes for which we intend to use the hypothesis and the relative advantages and disadvantages of acting on the basis of it relative to these objectives.

It should be noted here that Levi is not unaware of this type of consideration. After noting the unhappy result of his revision of the ESP problem, i.e., the arbitrary assignment of a value for k, he offers further revision by introducing the notion of "degrees of caution."[36] In the case of his truth-seeking experimenter, who assigns a value to k high enough to assure that the Bayes method will recommend suspended judgment for some assignments of probability to H_1 and H_2, we have the case of a scientist who "takes mistakes more seriously in relation to eliminating doubt" than might some others. In contrast to a more tenacious scientist,

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[36] Ibid., p. 56.

he exercises a higher degree of caution, indicating "that to seek the truth and nothing but the truth is to aim at a complex objective. It is an attempt to eliminate doubt tempered by an interest in finding truth and avoiding error."[37] But this of course concedes the point at issue, i.e., the move from premise (3) to (4). It concedes, as Levi acknowledges, "that the choice of a problem whose goal is to replace doubt by true belief and, as a consequence, the choice of a degree of caution in realizing such a goal can be both influenced and justified by practical considerations."[38]

Moreover, the fact that this conclusion implies nothing about a behavioristic reduction of beliefs, acceptance of hypotheses or suspended judgments constitutes less solace for the value-neutrality thesis than Levi recognizes. For, as we argued earlier, Rudner's argument opposing the value-neutrality thesis depends not on (6) but on (6'), not on a reduction of belief to action but merely on an implication between them such that accepting h entails (but is not necessarily equivalent to) a disposition to act on the basis of h relative to some objective. Nevertheless, in concentrating his efforts against (6), Levi fails to see that by conceding the scientist's need to reckon with the seriousness of mistakes, even though not allowing a reduction of belief to action, he is forced to acknowledge that different mistakes be taken with different degrees of seriousness. In other words, he totally neglects (6')

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37 Ibid., p. 56.

38 Ibid., p. 57.

which suffices to get Rudner's argument to the conclusion (5), that scientists qua scientists make value judgments regarding the seriousness of errors in acting upon accepted or rejected hypothesis. For, the different purposes for which one acts clearly require different cost factors and, as a result, different degrees of seriousness.

Even when the use for which one intends to accept an hypothesis is purely intellectual or theoretical, "the measure of these losses will depend upon the difficulties of devising alternative theories, and the complications of accepting a wrong theory from the point of view of other fields of investigation."[39] So merely by granting (6'), that acceptance or belief entails a disposition to act, and that the seriousness of mistakes must be reckoned with in deciding to accept or reject hypotheses, Levi seems forced to concede, in addition, that different degrees of seriousness must be considered, since these degrees result from the different purposes upon which we act. Hence, Levi has in effect conceded (5), the denial of value-neutrality.

Clearly, if the preceding considerations are cogent, the scientist must take into account in his decision to accept or reject hypotheses (3), not only epistemic utilities but also pragmatic utilities. Consequently the decision will vary with the kind of action to be based upon the hypothesis. Consider again the example cited earlier regarding the toxic ingredient of a drug.

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[39] Churchman, Theory of Experimental Inference, p. 250.

Because of the different utilities involved, a decision rule might warrant, on the same evidence, that the hypothesis in question should be accepted if applied to experimental animals, but rejected if applied to humans where an error would obviously be more serious. Hence the decision to accept or reject hypotheses is in part instrumental to action; it is partially a decision to adopt one of alternative courses of action. And this decision can be rationally defended only by considering the pragmatic gains and losses attached to the possible outcomes of the actions. This amounts to resting the acceptance of an hypothesis upon whether or not it leads to successful action.

Having thus unpacked and defended the experimentalist argument opposing the value-neutrality thesis of CL theorists, let me now present it in full array.

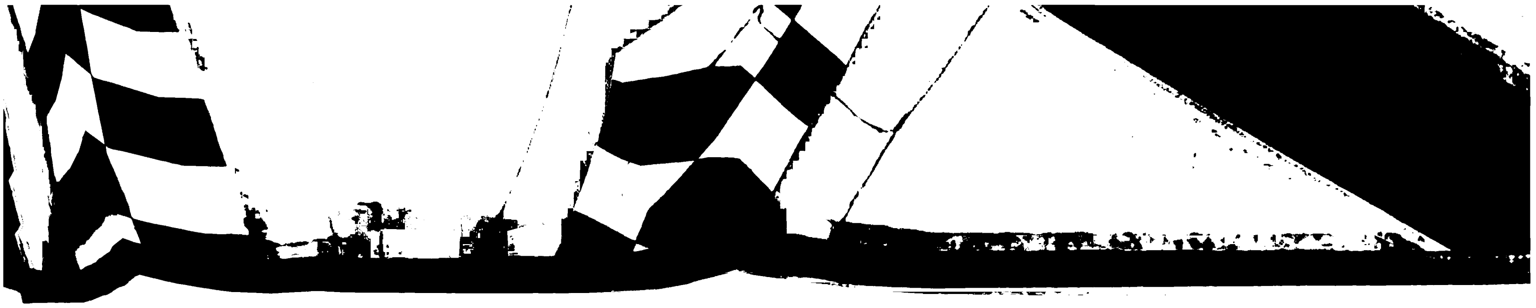
(1) The scientist as scientist accepts or rejects hypotheses.

(2) No scientific hypothesis is ever completely verified, but all are corrigible.

(3) Therefore, the scientist must make the decision that the evidence is sufficiently strong to warrant the acceptance of the hypothesis.

(6') To choose to accept a hypothesis H as true (or to believe that H is true) implies as a necessary (but not necessarily a sufficient) condition the disposition to act on the basis of H relative to some specific objective P.

(7) The degree of confirmation that a hypothesis H must have before one is warranted in choosing to act on the basis of H relative to an objective P is a function of the seriousness of the error relative to P resulting from basing the action on the wrong hypothesis.



(4) Therefore, the decision in (3) regarding the evidence and respecting how strong is "strong enough" to rationally accept H is going to be a function of the importance or seriousness, in the typically ethical sense, of making a mistake in accepting or rejecting the hypothesis.

(5) Hence, the scientist as scientist does make value judgments.

Value-Neutrality and Historical Explanation

In order to focus attention once again on historical explanation, let me consider briefly two final but important and related objections to the foregoing argument, objections posed most recently by Hempel, Nagel and Rescher but the roots of which lie deeply embedded in Aristotle's distinction between the theoretical and practical sciences. The import of the experimentalist argument, it would seem, is to force a reconsideration not only of the meaning of scientific and historical objectivity but also of the relations between the theoretical, technological and policy-making aspects of rational inquiry. Each of these issues is obviously too complex to be considered here in any detail. We will accordingly limit discussion to two related points.

First, though Hempel is unable to find any "satisfactory general way of resolving the issue between the two conceptions of science" (represented by the "accepted-information model" and the "pragmatist or instrumentalist model"), he still claims "that it would be pointless to formulate criteria of applicability by reference to pragmatic utilities; for we are concerned here with purely theoretical (in contrast to applied) explanatory and predictive statistical arguments."[40] Nagel follows suit by objecting to the experimentalist argument on the grounds that it misleadingly "suggests that alternative decisions between statistical hypotheses must invariably lead to alternative actions having

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40 Hempel, "Deductive-Nomological vs. Statistical Explanation," p. 162.

immediate practical consequences upon which different special values are placed."[41]

Beyond the controversial issues raised by these passages which have already been considered in our defense of Rudner's argument, this kind of objection forces one additional clarification. For the argument against the value-neutrality thesis in no way depends on considering, in the weighting of pragmatic utilities, only the "alternative actions having immediate practical consequences." No doubt some scientific hypotheses will be closely linked to such actions. But others, of a higher degree of "theoretical purity" will be more tenuously linked to such actions via the former ones. In fact, however, this linkage need be no more tenuous than the linkage of empirical significance between hypotheses containing theoretical constructs and relevant observational statements.

In other words, it seems to make as much sense to speak of degrees of practical consequences and pragmatic utilities, with a system or network of hypotheses linked at its edges to immediate practical consequences, as it does for Hempel to speak of degrees of testability or empirical significance, with a network of hypotheses linked at its edges to immediate observations. Surely nothing in either linkage precludes the other. Moreover, it should come as no surprise that the goals or objectives used to weigh the consequences of wrong estimations in the "purely"

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41 Nagel, The Structure of Science, p. 497.

theoretical sciences will be of a more complex nature and require more general criteria than those used in the more practical sciences, such as quality control and engineering. Hence, even if there are cases where the choice of policy for deciding to accept or reject hypotheses does not depend directly upon immediate practical consequences or costs, still the costs or pragmatic utilities are relevant in a more indirect manner.

In this sense, then, I think Hempel is correct in his charge that, on the basis of pragmatic utilities alone, "what is qualified as rational is, properly speaking, not the decision to believe h to be true, but the decision to act in the given context as if one believed h to be true...."[42] Yet, as with Levi's earlier objections, this damages Rudner's argument only if (6) is substituted for (6') and the behavioralist reduction fails. Without this unnecessary assumption, the correct residue of Hempel's point is that pragmatic utilities are relevant to the decision to accept or reject hypotheses only to the degree to which such hypotheses or beliefs are linked to actions.

The second point of interest brings us, finally, more directly to the application of the experimentalist argument to historical explanations. In a stimulating article under the joint authorship of Professors Rescher and Joynt, the close relationship between evidence in history and in the law is challenged. The grounds are that historical inquiry, unlike legal proceedings, involves no

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[42] Hempel, "Deductive-Nomological vs. Statistical Explanation," p. 162.

"stake," since "history has but one single purpose: the establishment of truth regarding the past...and the truth alone," while the law tempers this goal with "a view to the protection of individual rights."[43] Rescher and Joynt note some important differences between historical investigation and legal adjudication. For example, heresay evidence is not wholly inadmissable in history as in the courts. Nor can the historian afford the luxury of the high standard of evidential acceptability prevalent in the law: of automatically excluding certain witnesses, of admitting testimony only from witnesses who can be cross-examined, of imposing severe limitations on admissable evidence, and of imposing certain minima of evidence combined with rigid standards of proof.

Despite the importance of these differences, however, it is not at all clearly established that they stem from the same root, that the explanatory rationale accounting for such differences lies in the suggested different purposes of history and law. By assimilating legal procedures to scientific inquiry, with Bentham and J. H. Wigmore, yet divorcing both from historical investigation since the latter seeks the truth and nothing but the truth, the authors in effect argue that even if the experimentalist argument is cogent for scientific and legal inquiry, it nonetheless fails to apply to history.

Now, the main reason why we appealed to the denial of the value-neutrality thesis for scientific explanation in the first

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[43] N. Rescher and C. Joynt, "Evidence in History and in the Law," Journal of Philosophy, Vol. LVI (1959), pp. 567 and 577.

place was to show that the emphasis of empathy theorists on the subjective element of judgment was correct, but that it concerns the rational acceptability of the explanans hypotheses, not the logical explanatory force of an argument. We thus defended the CL theory against the SU thesis at the price of giving up value-neutrality. That is, we have tried to develop what seems to be the important insight of Verstehen theorists, and to show that the attempts to reconstruct the significant residue of Weber's SU thesis are well-intended though ill-enacted. The residual element is indeed overlooked by CL theorists in their attack on Verstehen. But the development of this element, along the lines suggested by Lavine, Natanson, Schutz, Dray, Donagan and Scriven, proves singularly ineffective against the general logical provisions of the CL theory. For the notion of Verstehen in no acceptable way argues against (R_1) - (R_4), inclusive of (R_1') and (R_4'). Instead, as we have shown in taking our cue from suggestions of Lavine, Dray and Scriven, the significant insight of Verstehen theorists on the need for the historian to make decisions and judgments in order to accept explanations as correct entails denying the value-neutrality thesis, not affirming the SU thesis.

Nevertheless, we also tried to support Dray's claim that this residual element, though clearly in the pragmatic dimension of explanation, is not merely a presystematic or psychological matter of discovering hypotheses as CL theorists suggest. Rather, it plays an essential role in any adequate philosophic explication or theory of rationally acceptable explanation. However, both Dray and Scriven overplay their hand. They substitute this pragmatic utility or value element, in the form of principles of action and normic truisms, for the explanatory role of empirical laws. Our argument, in other words, rests with the twofold claim that the denial of value-neutrality can be sustained, but that this in no way implies support for the SU thesis in any of the reconstructed versions considered herein. Consequently, we take the important residue of Weber's view as an additional pragmatic condition to be tacked on to the other provisions of the CL theory, rather than, as Dray suggests, a condition making the CL theory "peculiarly inept" in accounting for human actions.

We have shown, accordingly, that while Dray's argument does not support the conclusion he wants, it still suggests the criticism of the CL theory pursued in this chapter: that the historian and scientist must, in his explanatory practice, make value assessments or decisions, not in place of but over and above, and especially about, his covering laws or explanatory hypotheses. Moreover, Dray's attempt to enter a wedge between scientific and historical explanations has not been successful largely because, following other defenders of the "empathy" position, he uncritically accepts the CL theorists' claim of value-neutrality in scientific inquiries. This error, I think, prevents him from sustaining a successful attack against the CL theory of explanation, and also hinders him from developing the important residue of Weber's position.

Further, in considering why Dray so persistently resists the CL proposals in the face of their cogent opposition to his rational model, the answer again seems to turn on his uncritical acceptance of the CL theorists' value-free claim. He seems to believe, as a result, that if appraisals or value decisions do not constitute the explanatory force of historical explanations, then Weber's positivist insistence on the value-neutrality thesis will go unchallenged, except in some presystematic sense. Believing, additionally, that the value-neutrality thesis is unacceptable, he is led to view history as a branch of the humanities, and thereby to a twofold criticism of the CL theory: as a kind of conceptual barrier to a humanistically oriented historiography, and as a purely visionary attempt to reform history along scientific lines.

But since, as we have argued in this chapter, even scientific explanation requires the making of value judgments, and hence at least to this extent a humanistic orientation, Dray's resistance to the CL theory is both unnecessary and poor strategy. While a humanistic orientation of history and of science is indeed important, the price of abandoning history as a branch of the science of society seems much too high, especially when we might well have both. And just this constitutes the point of our thesis that Hempel's CL theory survives the varied logical criticisms of 'empathy' theorists, but only on condition that pragmatic elements be included essentially in a logical reconstruction of probabilistic explanations, that the emphasis on the structural aspects of explanation be extended to include a purposive ingredient, and hence that the value-neutrality thesis be denied. But denied, of course, so as not to be committed prematurely, if at all, to either a behavioralist reduction of belief to action or a crudely pragmatic reduction of truth or confirmation to utility.

This being in sum our case, its completion requires a defense against the charge of Rescher and Joynt. It needs to be shown, in particular, that history does not differ from scientific or legal inquiry by having, unlike them, but one goal or purpose, the pursuit of truth and nothing but the truth. But, since the other differences between history and the law seem cogent, we must then account for these differences in standards of evidential acceptability without appealing to the value-neutrality thesis as the explanatory rationale. For if they are correct in thinking that this is the rationale, then our opposition to value-neutrality in scientific explanation might not support our general case regarding the extension of the CL theory to historical explanations. Let me offer then, as a finale, a brief alternative account of the weaker standards of evidential acceptability in history.

Rescher and Joynt correctly acknowledge that good or sufficient evidence for reasonable belief is not a matter of any absolute standards but a function of both pragmatic and expedient considerations. It concerns the purposes for which we require the evidence and also the kinds of relevant data available in the particular discipline.[44] But they stress only the different purposes or functions of history and law, a difference between the investigative quest for truth and the adjudicative quest for

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[44] Ibid., p. 564.

adjustment of conflicting claims regarding things of value. This to the exclusion of the matter of what they call expediency. In fact, their major conclusion, that history seeks the truth and only the truth, rests completely on this exclusion.

One further related puzzlement seems noteworthy. In the context of the experimentalist argument presented in the last section, one would expect this conclusion about the truth-seeking purpose of history to be defended by assimilating history to the pure theoretical sciences rather than to the applied sciences. If there are in historical inquiry no other goals than truth-seeking, it would seem unreasonable to accept historical explanations that provide a mere comparative likelihood, *i.e.*, that are merely more likely to be true than their competitors. We should instead be inclined to accept only explanations providing a high likelihood *per se*. In other words, if we are to accept hypotheses in a reasonable manner on the basis of a comparative likelihood, which might mean that the degree of confirmation or evidence for the hypotheses *per se* is extremely low, we should expect this acceptance to be based on action undertaken to realize some objective or goal other than just truth-seeking.

Yet Rescher and Joynt clearly acknowledge the evidential criteria operative in history to be "akin to those of practice, *i.e.*, of *applied* science, rather than to those of theory or *pure* science." Thus can the historian rest satisfied when his explanations are shown to be merely "significantly more likely than any of the comparable alternatives."[45] The unsettling and unreasonable consequence of this position can be avoided, however, only by grounding the acceptance on other goals than truth-seeking. And this, in effect, they do by making the notion of sufficient evidence a function of the expedient matter of the kinds of evidence available, i.e., of "the paucity of the data and the difficulties in their interpretation, which are typical and insuperable features throughout his /the historian's/ domain."[46] Hence, their case for the conclusion that history seeks the truth and nothing but the truth, and thereby for their account of the other differences between history and the law, rests on excluding just this expedient or cost factor. For when this factor is considered in the analysis of these differences in the strength of evidential standards, the differences between the strong criteria applied in law or science and the weaker criteria utilized in history turn out to be not clear-cut but differences of degree.

Further, the explanatory rationale of these differences is not that history, unlike law, seeks the truth and only the truth, but rather that the cost factor or pragmatic utilities referred to in the last section differ. Surely there can be no doubt, for instance, that the seriousness or importance of making a mistake in legal adjudication is much greater by our moral standards than comparable mistakes in historical inquiry. Hence, Rescher and Joynt

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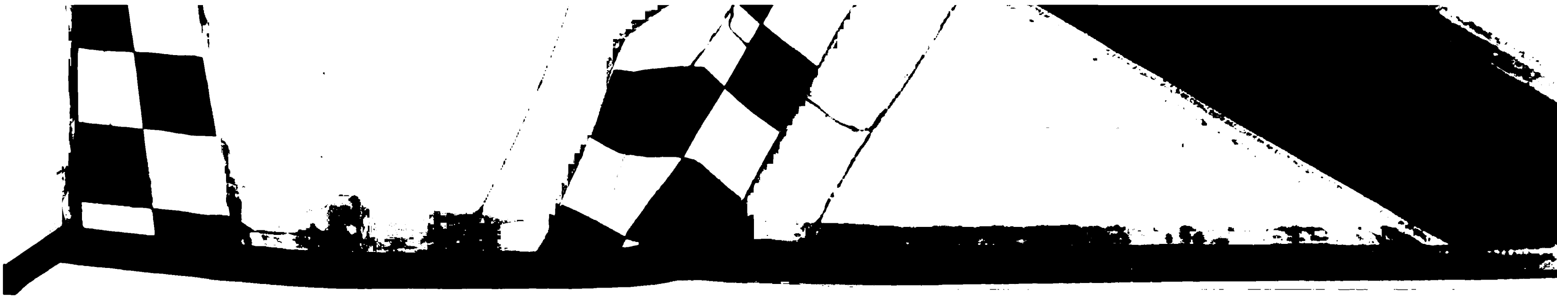
[45] Ibid., p. 563-4.

[46] Ibid., p. 564.

correctly point to the protection of individual rights as the reason for requiring stricter evidential standards in legal proceedings. In this sense, legal hypotheses are more closely linked to "alternative actions having immediate practical consequences."

Nevertheless, the historian also has pragmatic utilities to consider in accepting or rejecting explanatory hypotheses. Surely the reason he often accepts hypotheses with little likelihood *per se* but high comparative likelihood is not because he seeks truth alone, but because his quest for truth is tempered by the paucity of data and difficulties of interpretation. It is not because there is no "stake" involved but because the "stake" is less critical, *i.e.*, because the losses involved are only indirectly related to immediate practical consequences. Of more direct concern for the historian are the costs involved in the difficulties of devising alternative explanatory theories and the complications of accepting a wrong theory from the point of view of such allied fields as psychology and sociology. So, since historical explanations have a higher degree of "theoretical purity" than legal hypotheses, they are more tenuously linked to immediate practical consequences in action, and hence the degree of seriousness in making a mistake in history will be much less than in legal proceedings.

But the crucial point of our argument, to repeat, is that the only reasonable grounds for accepting hypotheses on a comparative likelihood rather than on a high likelihood *per se* is that the acceptance be based on action undertaken to achieve some goal



other than just truth-seeking. This very point was recognized by Bishop Butler even in the following passage quoted by Rescher and Joynt: "In matters of practice it will lay us under an absolute and formal obligation, in point of prudence and of interest, to act under that presumption or low probability, though it be so low as to leave the mind in very great doubt which is the truth."[47]

It seems unsettling and unreasonable indeed to accept an hypothesis on evidence which leaves the mind in very great doubt as to its truth, if our only goal is truth seeking. If this point be granted, it follows that the acceptance must be tied to action, however tenuous the link might be. Hence, according to the experimentalist argument, the historian as well as the scientist must make value judgments, in which case his goal cannot be just truth-seeking alone. And, finally, as a result of these considerations we rest our case for the twofold thesis that Hempel's CL theory of explanation survives the varied attacks of 'empathy' theorists, but only on condition that the value-neutrality thesis be surrendered.

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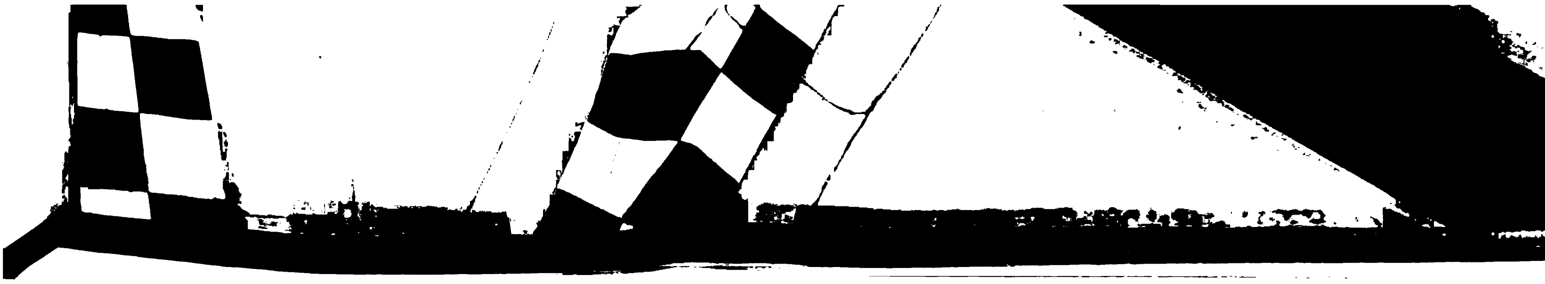
[47] Ibid., p. 563.



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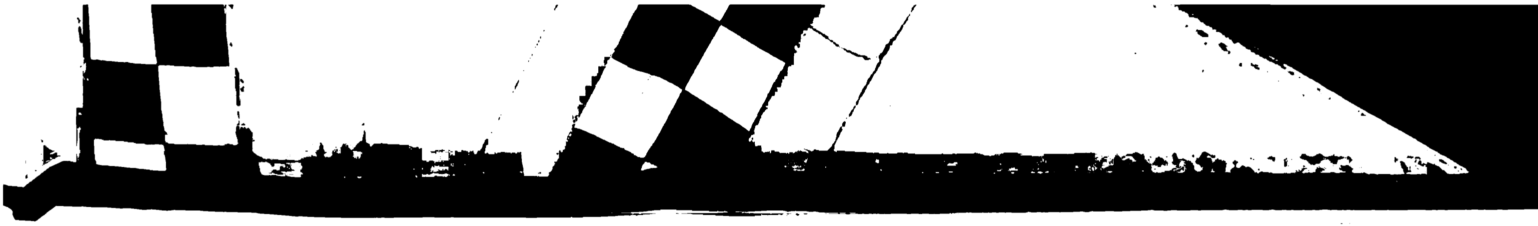

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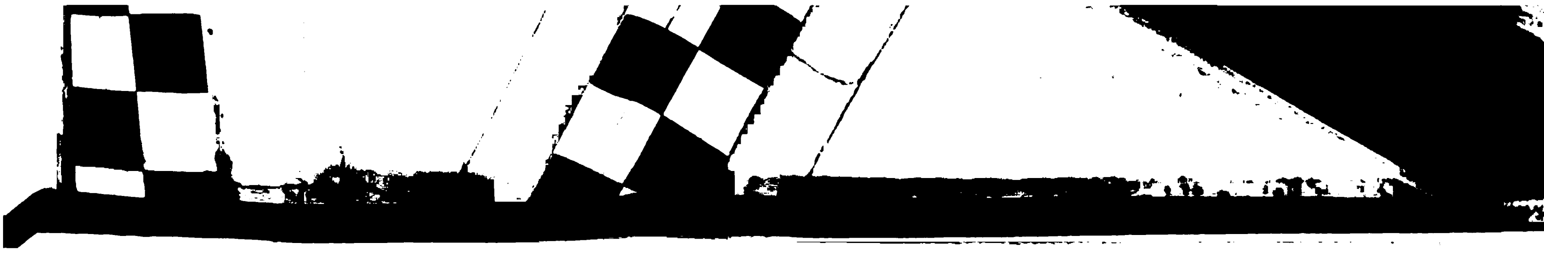

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