THE PLACE OF THE EXPLANATION OF PARTICULAR FACTS IN SCIENCE*

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On the critical side it is argued that, contrary to a widespread view, the explanation of particular facts does not play a central role in *pure* science and hence that philosophers of science are misguided in supposing that the understanding of such explanations is one of the central tasks of the philosophy of science. It is suggested that the view being attacked may stem in part from an impression that the establishing of a general law is tantamount to the explanation of particular facts that "fall under" the law. This suggestion effects a bridge to the more positive part of the paper, which consists of an exploration of the complexities exhibited by the relation between the two activities. More specifically, I point out a number of disabilities, any one of which could prevent us from being able to explain particular facts that fall under a given law even after having established the law. Some of these have to do with the form of the law, and some have to do with our powers of detection vis-à-vis the particular facts in question. The former sort have to do with the ways in which laws may deviate from the strict-necessary-and-sufficient-condition ideal. The latter include the following points. (1) The individual explanatory factors may lie beyond our present powers of detection. (2) The complexity of such factors may be too great for us to be able to interrelate them. (3) We may be unable to apply concepts used in the law to facts in this area (although they are in fact applicable).

The topic of "scientific explanation" receives a great deal of attention from contemporary philosophers of science. Much of the discussion is focused on the problem of what it is to explain a particular fact (hereafter "EPF"), such as the failure of an engine, a drop in temperature at a particular time and place, an outburst of temper, the development of a neurotic symptom, the souring of a certain bottle of milk, the defeat of Napoleon at Waterloo, a change of attitude on the part of a certain person at a certain time, and the revival of cities in western Europe in the eleventh century.[1] Thus the celebrated account of scientific explanation by C. G. Hempel and P. Oppenheim ([4]) opens with a consideration of two examples of explanations of particular events, and of the two chapters concerned with explanation in R. B. Braithwaite's book, *Scientific Explanation* ([1]), one is taken up with EPF. This preoccupation obviously reflects a conviction that the understanding of this kind of explanation is a central concern of the philosophy of science, and this conviction in turn reflects a conviction that the provision of such explanations is a central activity of science.

In this paper I shall first argue that this widespread conviction is flatly mistaken, that in what may be called "pure" or "theoretical" science, at any rate, such explanations have no place, or at most a minor or peripheral one. Then I shall

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[1] I shall not attempt to give an account of the difference between general and particular facts. For purposes of this paper the rough familiar distinction indicated by our examples will be sufficient.

explore some of the complexities involved in the relation between the central concerns of pure science and the job of EPF, in the hope of thereby arriving at a more judicious assessment of the bearing of science on EPF. The positive contribution of the paper is to be found in the exhibition of these complexities; the polemics with which we begin have the primary function of leading us to the latter topic.

I

Let's begin with a fable. Dr. Wissenschaft, a well-known research physicist, is relaxing over a cocktail before dinner, and Frau W. says to him, "You look awfully tired, dear. Have a hard day at the lab?" "Yes," the eminent doctor replies, "it was terribly exhausting, but I accomplished a great deal. I succeeded in explaining 250 cases of moisture forming on the outside of glasses when they were filled with ice water."[2]

Now what's wrong with this story? Obviously it is a ridiculous suggestion that a research physicist, eminent or not, should spend his working hours in this fashion. It may be thought that the absurdity stems from the simplicity of the task, or from the fact that it is merely a repetition of what has already been accomplished in the development of the discipline. But that cannot be the true explanation. If one were to set out to explain the exact pattern of moisture formation in an individual case, it would be by no means a simple matter, for reasons to be brought out in sections **III** and **IV**; in most cases a detailed explanation may well lie beyond our powers. And for the same reasons such explanations are by no means mechanical repetitions of an initial paradigm; each case may well present fresh problems of detail. The absurdity stems from the fact that it was with the explanation of a number of *particular* facts that our hero was occupied. The story would have served the same illustrative function, though less dramatically, had the particular facts been of a more recondite character, e.g. the deflection of laser beams. Research physicists no more concern themselves with the explanation, one by one, of a number of particular cases of a certain kind of laser beam deflection, than with the explanation, one by one, of a number of particular cases of condensation on the outside of glasses of ice water.

However, the thesis that EPF does not loom large in *science* is clearly too unqualified. The term 'science' is a sprawling one, of a family resemblance type, applied at one time or another to such diverse activities as theology, physics, soil control, medicine, geography, "political science," engineering, history, and the pronouncements of Mary Baker Eddy. Moreover the term has strong evaluative connotations, so that the question as to whether something or other is to be called a science is often by no means a "merely verbal" one. There is no doubt that within some of the activities called "scientific" EPF does loom large, e.g. the practice of medicine, history (including such disciplines as historical geology and historical astronomy as well as the history of human affairs), and the "applied" branches of psychology and sociology. A physician may be concerned with the explanation of a particular rise in temperature or the onset of a particular case of

[2] An EPF of this sort is one of the examples of "scientific explanation" given by Ernest Nagel ([6], Chapter 2).

hysterical weeping. The historical disciplines are centrally concerned with the explanation of changes in particular political institutions, of the decisions of rulers, and of the migrations of peoples. Historical geologists are concerned to explain the origin of particular mountain ranges. Applied sociologists try to explain the incidence of delinquency in a particular city or the outbreak of riots at particular times and places. Nevertheless there is a clear distinction between these pursuits, which we will range under the rubric, "applied science," and what may be called the "pure" or "theoretical" sciences. We may take as our chief distinguishing mark the fact that the pure sciences but not the applied sciences are centrally concerned with the establishing of general nomological propositions, together with whatever is a necessary prerequisite of this, e.g. the development of concepts and measures, and whatever naturally flows from success in this enterprise, e.g. the development of higher level theories. It is our contention that EPF is not a central concern of pure science.

Let us note in passing that the above distinction between pure and applied science by no means represents a *decision* on my part to segregate the activities of scientists into compartments on the basis of some arbitrary criterion. On the contrary the distinction reflects important cleavages in the actual organization of human activity. There is a well recognized, though by no means water-tight, division of labor between the medical researcher, who searches for general regularities in bodily processes, and the practicing physician who, *inter alia*, attempts to diagnose particular ailments of particular persons. Similar divisions of labor exist between the experimental and clinical psychologist, and between the research sociologist and the social worker. The skills, temperament, and resources required for the formulation, testing, and integration of general laws and theories are markedly different from those needed for explaining particular cases in real life, and although the two sorts of activities are sometimes carried on by the same person (Freud being a signal case), in the main they are allotted to identifiably distinct groups, the members of which receive different training and carry different titles.

The claim that EPF plays no major role in pure science calls for more support than is provided by the above fable. How are we to settle this question? One could keep a log of the activities of representative pure scientists during their working hours and note the extent to which they attempt to EPF. I have no doubt that such a survey would unequivocally support my contention. More fundamental support would be gained, however, by considering whether EPF has any essential place in the central activity of pure science, the establishing of general laws. This is a complex topic, but perhaps even a superficial discussion would be helpful at this point.

Within the complex of activities that make up pure science the point at which the scientist is most specifically concerned with particular facts is the inductive testing of low-level hypotheses, where by 'low-level hypotheses' we understand those that are so formulated as to make it possible to empirically discern instances of the constituent variables, thereby opening the way to confirmation by some pattern of inductive inference from their instances. Thus the claim that water heated to 212°F at normal sea level atmospheric pressure will boil, can be tested by finding, or instituting, instances of water so treated and then determining by observation, in each case, whether the water boils. Or, to take a somewhat more complicated example, more complicated both in involving more elaborate observational procedures, and in involving statistical considerations rather than mere enumerative induction, let the hypothesis be "Severe punishment of children for dependency tends to increase dependency." Here again we shall seek to discern cases in which such punishment has occurred, and in each such case we shall attempt to determine whether the child's dependency subsequently increased. Of course, since this latter hypothesis embodies only a tendency claim, we shall not require such an association in every instance, but only some kind of statistically significant distribution.

Now although in such test procedures we are necessarily dealing with particular facts, it is no essential part of our procedure to seek to *explain* any of those facts. In order to put the second hypothesis to an inductive test we are not called upon to explain why any particular parent severely punishes his child for dependency, nor why any particular child becomes more (or less) dependent.[3] We are required only to note the presence or absence of these variables in each case (and their magnitude, in case the variables are quantified). No explanation of any sort is involved.

This point has been obscured by the widespread (though often criticized) tendency to assimilate explanation to prediction. There is no doubt that the inductive testing of hypotheses frequently involves prediction and in a sense always does so. Whenever we test a general hypothesis by induction from its instances what we do could be construed as making one or more predictions on the basis of the hypothesis and then determining whether the facts turn out as predicted. Thus we test the first of the above hypotheses by predicting for each of a number of samples of water satisfying the conditions specified that it will boil, and determining in each case whether the prediction is verified. Tendency hypotheses do not yield a prediction for each individual case, but they do yield rather vague predictions for populations. The one cited above, e.g., does yield the prediction (or postdiction) that for any sufficiently large, properly chosen set of cases of severe punishment for dependency, a statistically significant proportion of them will exhibit subsequent increased dependency. But though our hypothesis testing can be construed as a process of testing predictions of particular facts, it does not follow that we are seeking to *explain* any particular facts. To seek to explain, e.g., a case of increased dependency would involve first noting that such a case has occurred and then searching the surrounding context for something that is responsible for it. But only the first is involved in inductive testing. Having noted the occurrence of the continued dependency we need have no further concern with that particular case as such; we have already milked it of any significance it has for purposes of inductive confirmation. It must not be forgotten, though it often is, that explana-

[3] Of course someone may use the hypothesis thus confirmed in explaining cases of increased dependency. In the sequel we shall have a great deal to say about the use of general principles in EPF. But this point has no tendency to show that EPF plays any role in the process of establishing general principles.

tion is an activity with distinctive goals, for which a distinctive setting and distinctive preconditions are required. I do not even attempt to explain something unless I ask certain kinds of questions about it after having come to accept its existence. In inductive testing I ask no further questions about the particular instances, once its existence is noted.

There is a grosser assimilation that may not be without influence in promoting the idea that EPF is central in pure science, the assimilation of laws to explanations. Consider the following passages.

> ... the sciences seek to discover and to formulate in general terms the conditions under which events of various sorts occur, the statements of such determining conditions being the explanations of the corresponding happenings ([6], p. 4).
>
> The search for reasons goes behind the facts to relationships between facts and the general laws which can be derived from those relationships, and the discovery of these things constitutes scientific explanation ... ([2], p. 91).
>
> An account of psychological explanation on which *no* psychological theory turned out to be an explanation would be *ipso facto* unacceptable ([3], p. 163).
>
> But a theory of a phenomenon is an explanation of the phenomenon and nothing that is not an explanation is worthy of the name of theory ([5], p. 22).

If laws and theories were identical with the explanations of the facts that fell under them, then of course pure science, in being centrally concerned with laws, would be equally concerned with the explanations of facts that fall under those laws. But once we extract the identity claims from the above passages and expose them to the clear light of day, it is obvious that they are mistaken. It is surely clear that on any plausible understanding of these terms a law (or theory) is, contra Nagel and Fodor, one sort of entity and an explanation another sort, and that to identify a law (or theory) with an explanation is to commit a "category mistake" of the most flagrant sort. And it is equally clear, contra Caws, that discovering a general law is not *itself* an activity of explaining anything, however useful the results of the former activity may be for the carrying out of the latter. In formulating a general law, e.g., that ontogeny recapitulates phylogeny, one is not thereby reporting, describing, or formulating any explanation of anything; and in reporting the discovery or confirmation of that law one is not thereby reporting having explained anything.

There is no profit in continued flogging of these formulations, literally interpreted. Let us take it that our authors spoke ill-advisedly, carelessly, or in a fit of hyperbole. Nevertheless, the pervasiveness of the formulation argues against our taking it as an insignificant isolated slip. It would seem to indicate a rather widespread failure to keep clearly in mind the fundamental conceptual distinctions that are involved in considering the place of explanation in science.

I have just been arguing that EPF plays no *essential* role in induction. But that conclusion is compatible with the claim that it plays a subsidiary role, that it can be involved under certain special conditions. And I believe that to be the case. The chief occasion for EPF in hypothesis testing is the occurrence of counter-instances, the failure of a particular case to come out as predicted. Suppose, e.g., that a particular sample of water fails to boil when (apparently) heated to 212°F at (what is apparently) normal sea-level atmospheric pressure. Of course, we can just take this event to disconfirm the hypothesis, in which case there will be no further concern with it, at least vis-à-vis that hypothesis. But alternatively we may, especially if there are a variety of reasons for supposing the hypothesis to be correct, seek to explain the (apparent) counter-instance in such a way as to render it compatible with the hypothesis. Thus we may explore the possibility that the thermometer reading was incorrect, that there were unusual and unsuspected influences on the atmospheric pressure, or that our sample was not pure water. Such attempts may result in our showing that what appeared to be a counter-instance really was not. Or they can have a heuristic value by suggesting ways of enriching the original hypothesis so as to take account of previously unsuspected influences. Thus the provision of such explanations can further the goals of pure science. Nevertheless the fact remains that they do not constitute an essential and inevitable feature of inductive procedure. If our tests invariably had positive results, the need for these kinds of explanations would never arise. Our conclusion still stands that EPF plays no central role in pure science.

Let me hasten to add that I am not at all asserting that explanation is not central to pure science. On the contrary, explanation may without exaggeration be said to be its crowning achievement. But it is explanation of general nomological facts, not EPF that occupies this position. Physics tries to explain phenomena of locomotion, expansion, and electrical transmission; chemistry the rusting of metals and the souring of milk; psychology, differential rates of learning and depth perception. But the research physicist does not occupy himself with the explanation of the boiling of a particular kettle of water as such (except under the special conditions mentioned above); he is concerned to explain the nomological fact that water (under certain conditions) boils at 212°F. The theoretical psychologist does not attempt to explain the fact that Johnny has not learned to read whereas Jimmy has, but rather the general fact that by and large one learns to read more rapidly when the lessons are temporally spaced in certain ways. The search for explanation that is essential to pure science does not begin until some law-like generalizations have already been established (or at least accepted), and then it is directed to the question of why those generalizations hold.

Scientists often speak of explaining a certain "phenomenon" or a certain "effect," and this terminology may have encouraged the supposition that they are bending their energies to EPF. But in fact these terms are standardly used in pure science to denote general regularities, not particular happenings. Thus psychologists are concerned to explain the "Zeigarnik effect" and the "phenomenon of spontaneous recovery." But the Zeigarnik effect is the *general tendency* of people to remember uncompleted tasks better than completed tasks, and the phenomenon of spontaneous recovery is the *general tendency* of an "extinguished" response to recover strength after a period in which there are no trials, either reinforced or unreinforced.

In this connection we may note that many examples of "scientific" explanations of particular facts presented by philosophers gain their plausibility as instances of "scientific explanation" through being more readily interpretable as explanations of general regularities. Thus the classic paper of Hempel and Oppenheim on scientific explanation ([4]) opens with two examples of explanations that are categorized by the authors as "the explanation of particular events occurring at a certain time and place." The character of the examples may be briefly imparted by specifying the explananda.

(1) "A mercury thermometer is rapidly immersed in hot water; there occurs a temporary drop of the mercury column, which is then followed by a swift rise."

(2) "To an observer in a rowboat, that part of an oar which is under water appears to be bent upwards."

Now when one thinks of an explanation of (1), on the basis of physical principles, one is almost inevitably led into thinking of the attempt to explain the fact that, *in general*, the specified result ensues when a mercury thermometer is rapidly immersed in hot water. A physicist who concerned himself with such a question would be trying to throw light on the question as to why this generally happens, rather than trying to ferret out the particular factors involved in some particular case. And the accounts given in the article are of that sort. They mention types of factors and processes that one would expect to be present generally in the sorts of cases specified. No reference is made to the particular features of any particular case. Of course the material that is presented *can* be presented in the form of an explanation of a (real or imaginary) particular case, individuated as to time and place of occurrence. But that would be purely *pro forma*; it would have no bearing on the content of what is said. We are concerned with the explanation of a particular fact in any distinctive sense only where we are faced with problems that arise from the particular concrete nature of that particular fact as it actually exists independent of our categorizations. Where the particular fact (real or imaginary) is *merely* playing the role of an illustration of some regularity, then what we are really considering and trying to explain is the regularity, and it is obfuscating to present the enterprise as an attempt to explain a PF.

The philosophers against whom I have been arguing might try to neutralize my criticisms by averring that they had not meant to be confining their subject matter to what I have called "pure" science, that they were using the term 'science' in a more inclusive sense to range over the applied and historical disciplines as well and hence that by my own admission EPF does figure centrally in science as thus broadly construed. To this reply I reply in turn as follows. (1) Hempel, Nagel, *et al.* certainly give the impression through their examples and through the general course of their discussions that they mean to concentrate on pure science. (2) In any event the distinction between pure, applied, and historical science is a crucially important one, especially with regard to the present issue. Whatever the intentions of Hempel *et al.*, it is well worthwhile to make the points I am making about pure science, points which these philosophers do not make, and indeed are not in a position to make until they explicitly draw these distinctions. (3) There is an important sense in which pure science is more basic than the applied and historical science, for ideally the latter will consist of applications of the results of the former. In fact, of course, many of the "applied" disciplines, like social work, clinical psychology, and history, have to get along as best they can without basing their procedure on well established general laws, because of the backward state of the disciplines that are charged with the provision of such laws. But in an ideally constituted intellectual economy the situation would be as specified. Hence it is profoundly misleading to speak of EPF as in some special or honorific sense paradigms of "scientific explanation." It is not that kind of explanation that furnishes the central concern of the most fundamental departments of scientific activity.

II

To say that pure science and diagnostics[4] are essentially distinct enterprises is not to deny that they are importantly connected. And indeed the two activities do have important bearings on each other and in both directions. With the polemics of section I behind me, I shall now turn to the positive task of tracing out some features of these lines of influence. Since the influence of pure science on diagnostics is more complex, more controversial, and less well understood, I shall concentrate my remarks on that topic.

But first a word about the other direction. Apart from the point mentioned above concerning the role of EPF in the reaction to an apparent disconfirmation of a general hypothesis, I would suppose that the greatest value of EPF for pure science is a heuristic one. In attempting to explain PF's in ways not covered by existing laws, the diagnostician may come to suspect certain nomological connections, suspicions that may flower into testable general hypotheses. Thus Freud arrived at many general hypotheses in the attempt to explain particular neurotic symptoms in ways not covered by any existing body of generalizations, and many of the early general hypotheses of behavioristic learning theory arose out of attempts to understand the behavior of particular cats in puzzle boxes. That is, an apparent success in explaining a PF, F_1, in terms of certain factors, $f_1, f_2, \ldots$, may lead one to fruitful general hypotheses as to the dependence of facts like F_1 on factors like $f_1, f_2, \ldots$. I take it that this kind of influence is unproblematic.

On a very general level the bearing of pure science on diagnostics is quite obvious also. The basic point is that in establishing general laws pure science provides valuable resources for the activity of EPF; the more knowledge we have of general regularities the better position we are in to find the explanation of PF's that fall under those regularities. In seeking an EPF of the kind under consideration in this paper we are looking for what is "responsible" for the fact, that from which it results, that on which it depends for its existence. I will label such explanations "causal explanations," though I do so with some trepidation, since I consider the term "causal" to carry many misleading associations.[5] If we are trying to thus explain a fact of type F_1 it will certainly help us to know that F_2-type facts are generally associated with F_1-type facts in a certain way. If our aim is to explain Johnny's retarded learning, and we know that it is generally true that people learn slowly, relative to their intellectual potential, when emotionally disturbed, that will

[4] I use this term for the assemblage of attempts to EPF.

[5] I am leaving aside the question of what other kinds of EPF there might be. I am convinced that there are other kinds, but for the purposes of this paper it is not necessary to go into that.

at least give us something to look for. And if we ascertain that Johnny is in fact emotionally disturbed to a significant extent, then we will have some reason to take his emotional disturbance as at least part of the explanation.[6] That is, the results of pure science (a) give us leads as to what sort of thing to look for in trying to explain a PF, and (b) give some support to particular suggested explanations. They provide us with resources both for discovery and for justification.

Though this much seems clear, as soon as we try to become more specific we become embroiled in controversy. Most of the attempts to become more specific have concentrated on the question of just what kind of support a general law gives to an EPF? What connection, logical or otherwise, must hold between the law and the explanation in order that the former lend credence to the latter? Thus the recent literature on the subject has been dominated by debates between the Hempel–Oppenheim "deductive" model of explanation and its critics. Although these problems are interesting and important ones, they are not the ones I shall explore. Rather I shall address myself to the relatively neglected question of the extent to which the possession of general laws puts us in a position to explain PF's that fall under those laws? Or to put the question negatively, what sorts of deficiencies could render us unable to explain a PF that falls under a given law, even though we have established the law?

It may seem that we cannot discuss this question except in the framework of some solution to the problem of the logical relation of law to explanation. But I do not believe this to be the case. I take it that all the points I will be making can be seen to be correct without our having already settled on some particular solution to that problem; in fact, I would make it a condition of adequacy for any solution to that problem that it be able to accommodate these points. The reader must determine from the subsequent course of the discussion whether this claim is correct.

I should make it explicit that I take this part of the paper to be significantly related to the first part, and not just by virtue of the fact that both have to do with the relation of EPF to pure science. I believe that there is a widespread sense among philosophers of science that the possession of general laws comes rather close to being enough to put us in a position to explain PF's that fall under them, that general laws are much more nearly sufficient for this purpose than our subsequent discussion will show to be the case. I cannot really document this belief, partly because the matter is insufficiently discussed in the literature, and partly because the conviction I am taking to be widespread is a rather hazy one, not susceptible of a sharp formulation. If there is such a widespread conviction it could well contribute to the failure to recognize that EPF is not central in pure science. For if one feels that establishing general laws, the central activity of pure science, were almost enough in itself to put us in a position to EPF, then it is understandable that he would have a sense that the latter as well as the former activity "belongs" in pure science, and even understandable that he should make the otherwise incredible assimilations of law and explanation exemplified by the quotations on page 17. And this would be especially understandable given that the topic is rarely considered

[6] The ensuing discussion will provide reasons for denying that under those conditions we can conclude, without more ado, that the emotional disturbance *is the* explanation.

explicitly. However all this is highly speculative, and nothing in the paper hangs on it. If my suspicion is well grounded, then this part of the paper removes one source of (not argument for) the view attacked in (i). But if it is not well grounded, this part stands on its own feet as an exploration of some of the complexities of the interrelations between establishing general laws and EPF.

I shall proceed by pointing out and illustrating[7] a variety of deficiencies from which we may suffer even after having established one or more general laws, any one of which deficiencies could prevent us from being able to use the law in the explanation of PF's that fall under it. I suspect that individually each of the points I shall be making will be readily admitted, and that one by one they will appear too obvious to deserve mention. Nevertheless they have not received the attention they deserve, and I believe that by systematically rehearsing them we will be helped toward a juster view of the relations of pure and applied science.

These deficiencies may be usefully divided into those stemming from features of the law, and those stemming from our fact-gathering capacities vis-à-vis the PF to be explained. This division can be set out sharply in terms of the following familiar way of viewing the use of general laws in the EPF. One has a general law with C as the consequent variable, and $A_1, A_2, \ldots$ as antecedent variables.[8] One observes an instance of C and seeks to explain its occurrence. One then looks around for instances of $A_1 \ldots$ occurring in the appropriate spatio-temporal relation to the observed instance of C. If one succeeds in finding them, one can then use the law to ground an explanation of that instance of C in terms of those instances of $A_1 \ldots$. For example, let the law be: "Whenever a spark passes through a mixture of hydrogen and oxygen gas, the gases disappear and water is formed." If one has confirmed this law, and one is confronted with a case of water formation, one then seeks to determine whether this formation was preceded by the passage of a spark through a mixture of hydrogen and oxygen gas (at the right place) and the disappearance of the gases. If he determines that it was, he can use the law to give an explanation of the water formation in those terms. Now if these law-based EPF's typically involve both an appeal to the law and the ascertaining of particular instances of the antecedent variables, we could expect any breakdowns in the process to occur in one or the other of these phases.

In order to underline the difficulties of each sort, we shall exemplify them with cases in which everything is in order in the other phase of the process. That is, when discussing difficulties that stem from the character of the laws involved, we shall

[7] My illustrations will be restricted in two ways. First, they will be drawn mostly from the social sciences, especially psychology, and second they will not involve any elaborate quantification. I am confident that all my points hold also for highly quantified laws and explanations in the physical sciences, but I will not have time to argue the point in this paper.

[8] In all the laws I shall be considering in this paper there will be an intuitively clear distinction between antecedent and consequent variables; i.e. the law will be naturally construed as asserting that one sort of thing is dependent on, is a function of, or results from, another sort of thing(s), but not vice versa. Hence there will be a clear direction in which the law will be applied in EPF's. It will be applied to explain instances of the consequent variable in terms of instances of the antecedent variables but not vice versa. The points I shall be making will hold also for genuinely reciprocal laws, where instances of either variable can be invoked to explain instances of the other, e.g. Boyle's laws. I am restricting my examples to the one-way laws just because the illustrations can be carried out somewhat more simply.

suppose ourselves to be fully able to ascertain any instances of the antecedent variables that may be present; and when discussing deficiencies in our fact-gathering capacities, we shall work with cases in which the laws themselves are of the most favorable character.

III

Let us first consider difficulties that stem from the character of the laws. The basic point here is that laws may be of different logical forms and of differing degrees of strictness; and depending on where a law is located on these dimensions, it may yield, together with appropriate factual material, a more or less determinate conclusion as to what is responsible for a given instance of its consequent variable.

First consider an unqualifiedly general "if and only if" (necessary and sufficient condition (NSC)) law.

> (3) "Water at normal sea-level atmospheric pressure will boil *iff* it has been heated to 212°F."

Such a law will, even without supplementary information about the particular case at hand, furnish an ideally adequate ground for an explanation of a particular instance of C (consequent variable). Given that the law has been established we do not even need to ascertain in a particular case of C that an instance of A (the antecedent variable) is present in order to be assured that the boiling is to be explained in terms of being heated to 212°F. The law by itself is sufficient to assure us of that. If it is true that water under such conditions boils *if and only if* it has been heated to 212°F, then we do not need to separately identify the "being-so-heated" in an individual case in order to be justly confident that this was why the water boiled.[9]

Unqualified NSC laws give the maximum yield for EPF. Laws of no other sort are sufficient all by themselves to ground an EPF even in the most general terms. But laws that deviate from the NSC paradigm differ among themselves in ways that are highly significant for their use in EPF. First we shall consider unqualified necessary condition (NC) and unqualified sufficient condition (SC) laws.

1. Unqualified NC laws may be exemplified by:
   (4) "Oxygen is necessary for combustion."
   (5) "Early dependence on another human being is necessary for normal personality development."

From such statements we can derive no complete account (even using realistic, workaday standards for completeness) of what produced a given instance of combustion or a given case of normal personality development. The law only specifies one of the factors that must be present if the phenomenon is to be forthcoming. It does not tell us, even in very general terms, what else is involved. And in searching for an explanation of a given PF, it may be some other part of the picture in which

[9] Of course in looking for an explanation we may want more specific information about the cause than this. We may want to know just how the water was heated to 212°F, where the thermal agent was located, etc. If so we will need to do some research in the particular case.

we are specially interested.[10] If an insurance investigator is trying to determine what started a certain fire, he will not be enlightened by being told that oxygen was present. Thus provision of the law is by no means sufficient for the provision of a particular explanation.

2. Unqualified SC laws may be exemplified by:
   (6) "Passage of a spark through a mixture of hydrogen and oxygen is sufficient for the formation of water."
   (7) "The reduction of a drive while an organism, *O*, is emitting a response, *R*, in the presence of a stimulus, *S*, is sufficient to increase *O*'s tendency to emit *R*'s in the presence of *S*'s."

From such a law alone we cannot infer what kind of thing is responsible for the formation of a particular body of water or a particular *S–R* bond, for the law does not rule out the possibility that there may be other sufficient conditions as well; in which case we could not tell from the law itself which of the sufficient conditions is responsible in a particular case of *C*.

Although unqualified NC and SC laws do not by themselves determine any explanation of any PF, they are related to such explanations in a relatively simple and straightforward manner. A NC law will by itself give us at least one piece of what would be an ideally complete EPF. From (4) alone we can infer that the presence of oxygen will be one part of a complete explanation of a given case of combustion. And if to an established SC law we add the information that an instance of *A* was present at the right time and place, then, apart from sticky problems about overdetermination, we need nothing else in order to conclude that the correlated instance of *C* can be so explained. Suppose I have confirmed (7), and I am confronted with a case of the strengthening of an *S–R* bond. I then ascertain that drive reduction did occur repeatedly while the organism in question was making responses of the kind in question to stimuli of the kind in question. All this will put me in an ideal position to support an explanation of that *S–R* bond strengthening in terms of those drive reductions.

The consideration of unqualifiedly general laws is of limited importance for building up a realistic picture of science as it is actually carried on. Especially in the social sciences we seldom are in a position to assert an unrestrictedly general connection of any kind between the variables we are studying. Deviations from unqualified laws of the sort thus far considered are of two main types: those which attach "boundary conditions" or "disturbing factors" to the statement of the law

[10] In this paper I am not going into the question of what sorts of things would satisfy the demands for EPF that arise under different conditions. It is obvious that people are looking for different parts of the "total picture" in different contexts of inquiry. For example, a medical researcher and a detective are looking for different sorts of things when they seek to explain someone's death. But these complexities are not crucial for the central issues of this paper. Let's say that there could be a context of inquiry in which mention of the presence of oxygen would serve to satisfy a request for an explanation of a case of combustion. Nevertheless it is obvious that there are many other contexts in which such a specification would not be to the point. And more generally, for any given necessary condition, there will be contexts in which mention of that necessary condition will not be pertinent to the kind of "Why?" question raised.

("open-ended laws"), and those that only assert "tendencies" ("tendency laws"). We shall consider each in turn. For simplicity of exposition I shall restrict myself to SC laws modified in each of these ways.

(A) Open-ended laws specify *A* as a sufficient condition of *C*, unless certain unspecified (or incompletely specified) "disturbing factors" are present, where it can be presumed that these factors are often absent, or at least present only to a negligible degree. Thus, the "drive reduction" law mentioned above would be more accurately formulated in this way.

> (8) "The reduction of a drive while an organism, *O*, is emitting a response, *R*, in the presence of a stimulus, *S*, is sufficient to increase *O*'s tendency to emit *R*'s in the presence of *S*'s, *unless O* is killed at that moment, or *unless* there is, simultaneously, a stronger increase in some other drive, or unless it was extremely upset emotionally at the moment, . . ."

And apart from the possible disturbing factors mentioned, and apart from those we could now mention if pressed to do so, we can be pretty sure that there are presently unsuspected factors which are such that if they did eventuate the *A* would not produce the *C*. For example, before electricity was understood, no one knew that the presence of an electromagnetic field could prevent an iron ball from following the path dictated by a vector resultant of mechanical forces. In investigating some particular variable, e.g. speed of learning or strength of electric current, the scientific ideal is to find a manageably small set of antecedent variables of which the variable under study is a determinate function; but in most areas the best we can hope for is a greater or lesser approximation to this ideal.

This situation is, in the case of the most developed sciences, not disadvantageous for many practical purposes such as bridge building, for relative to those purposes the intrusion of extraneous factors can be counted on to be negligible. But for all that the open-endedness of a SC law in respect to possible disturbing factors prevents any logical derivation of an EPF from the law plus a report of the presence of an instance of the sufficient condition, a derivation of the sort we saw to be possible with a strict SC law. Supposing that we have established the drive reduction law in the open-ended form, and supposing that we have ascertained a particular case of *S–R* bond strengthening S_1, preceded by drive reductions D_1, it still does not logically follow that the former was due to the latter. For our premises are compatible with the hypothesis that some disturbing factor, e.g. extreme emotional disturbance, prevented the drive reductions from having the strengthening effect they would otherwise have, whereupon some other kind of sufficient condition led to the strengthening of the *S–R* bond. No doubt the premises in question support to a considerable degree the explanation of S_1 in terms of D_1, especially if the law contains the provision that any disturbing factors are only rarely present to any significant degree. Moreover we can, if need be, increase the degree of support by determining that none of the possible disturbing factors on the list was present. Nevertheless, so long as the list is open-ended, our support of the explanation will never become as conclusive as it could be with an unqualified SC law. For no matter how many facts we uncover, it will always remain a logical possibility that some hitherto unenvisaged factor has thrown off the normal relationship between D_1 and S_1. And this marks a crucial difference between unqualified SC laws and open-ended SC laws with respect to their potentialities for grounding EPF's.

A special case of the above, one which is highly significant for EPF, involves discrepancies between the laboratory and real life. It often happens that insofar as we succeed in confirming an unqualifiedly general law we do so only for special laboratory situations that are restricted in ways in which real-life situations usually are not restricted.

Thus suppose that a "decision theorist" has been testing the following hypothesis:

(9) "An agent faced with several mutually incompatible possibilities of action chooses one of those possibilities if and only if that action maximizes expected utility."

In order to concentrate on the variables in which he is interested he has restricted his experimental situations to trivial betting choices with respect to which his subjects have no emotional involvement, which are such that nothing other than monetary utilities are involved, and where the combination of utilities and probabilities are such that errors in computation would be extremely unlikely. But of course in real life decisions are influenced by "irrational" fears and hates, by mistakes in computation, and by sudden impulses. For all (9) tells us, a given decision in real life may be mostly determined by factors such as these, rather than by the antecedent factors specified in the law. Thus (9) does not have the utility for explaining decisions in real life (where most of our diagnostic problems arise) one would suppose from just looking at its unqualified form. If (9) is not to be flatly mistaken in its application to the real-life situation, it will have to be supplemented with a lengthy list of "disturbing factors," in this case *without* any assurance that they are only rarely present to any significant extent. Thus having ascertained that a given decision in real life was one which maximized expected utility, we cannot put that fact together with the law to derive without more ado an explanation of the decision. For this information is quite compatible with the decision's having been due to a sudden impulse rather than to a calculation of expected utility.

(B) In the social sciences we are generally much further than this from strict NC or SC laws. We are generally unable to find or institute, even in the laboratory, situations in which our consequent variables, e.g. the efficiency of an organization, are shielded (even for practical purposes) from influences other than those being studied, e.g. the social structure of the organization. Hence we are unable to establish even the sort of approximation to strict laws described under (A). In the absence of any effective shutting out of alien influences, and pending the development of richer systems that will interrelate all the effective variables, we are forced to content ourselves with more modest hypotheses to the effect that C is *one* of the important influences on E. Such hypotheses are felicitously formulated as "tendency" statements.

(10) "The efficiency of an organization tends to be enhanced by an informal network of sub-groups."

(11) "Severe punishment for dependency tends to increase dependency."

A tendency law makes no claim about each individual case of *A* and *C*. For example, (10) is not disconfirmed by a particular case of a relatively inefficient organization that contains a large informal network of sub-groups; indeed it is not necessarily disconfirmed even by a large number of such cases. The hypothesis makes no commitment as to what always happens or even as to what happens in "standard" or "normal" cases. Confirmation is obtained by showing that the proportion of relatively efficient organizations in the class of those with large networks of sub-groups is greater than would be expected on the basis of chance alone.[11] And the hypothesis can only be disconfirmed by showing that this is not the case.

Such laws do not put us in a very strong position for explaining individual cases of their consequent variables. Having established (11), and then being confronted with a case of increased dependency *and* having ascertained that it was preceded by punishment for dependency, we still cannot be sure that the increase was due to the punishment. In that particular case there are two reasons for this judgment. First the point we made with respect to open-ended laws applies here with added force. (11) not only makes no commitment to an invariable progression from *A* to *C*; it does not even make a commitment to such a progression almost always, generally, or in all "normal" cases. Hence it leaves an even larger loophole for the intrusion of some factor that interferes with the punishment's "carrying out its tendency" in a particular case. For example, a particular punishing mother may have neutralized what would otherwise have been the dependency-increasing effects of her punishment by warm encouragement of the child's tendencies toward independent efforts, whereupon the increase in dependency resulted from other factors, e.g. a greater need of acceptance in the face of increased guilt feelings, or a regression to the oral stage. Second, even if severe punishment did play a role in bringing about a given increase in dependency, the tendency law carries no implication, or even suggestion, that this is the whole story or even most of the story. The mere fact that severe punishment for dependency has a *tendency* to increase dependency is quite compatible with its being sometimes or even always the case that when severe punishment does have this result it is only in conjunction with other factors. In fact if a tendency law is the best we can do then it would seem to follow that severe punishment leads to increased dependency, when it does so lead, only in conjunction with other conditions which are carrying a significant part of the load; for if it were sufficient by itself we could assert an unqualified SC law, and if it were virtually or usually sufficient by itself, we could assert an open-ended SC law. Thus, even where we are justified in following the lead of the tendency law in looking for explanatory factors, it will generally not be the case that a complete explanation (even given realistic standards for completeness) can be given in terms of such factors.

This rapid and sketchy survey of types of laws was designed to emphasize the point that laws put us in very different positions vis-à-vis the explanations of PF's

[11] I am not, of course, purporting to go into the subject of hypothesis testing. The above statement represents the weakest kind of demand on the weakest sort of tendency hypothesis. More sophisticated statistical tests may be used, depending on the character of the hypothesis and the nature of the data. For example, a more quantitative tendency statement, like "The efficiency of an organization tends to vary in proportion to the extent to which the organization contains an informal network of sub-groups" would require different treatment.

that fall under them, depending on the character of the law. At the very least such considerations should inhibit any tendency to think of the relation between law and EPF on any one model, and should inhibit any tendency to think of this relationship as typically being as straightforward as it is with, e.g., unqualified SC laws. More positively I have sought to show that quite often our *A–C* laws are such that, taken together with the report of a particular *A*, they fail in varying degrees to give conclusive support to any particular explanation of the corresponding *C*. In such cases, we may have established the law, and ascertained the presence or absence of all the variables mentioned in the law, and still not be in a position to advance with complete assurance an explanation of the particular *C*. Thus, the various deviations of laws from the unqualified SC or NSC paradigm constitute one class of deficiencies that can prevent our being able to utilize laws in the explanation of PF's that fall under them.

IV

The second main class of deficiencies has to do with our capacities for ascertaining the relevant instances of the variables mentioned in the law. In surveying the first class of deficiencies we allowed our diagnosticians to be free of any deficiencies of this second sort; and here we shall follow the reverse procedure, again in order to avoid contaminating our sources. That is, we shall restrict ourselves to cases in which the law is of the sort that puts us in the best possible position to explain particular *C*'s. More specifically we shall restrict ourselves to strict SC and NSC laws, and for purposes of illustration we shall pretend that the following NSC law has been established.

(12) "An *S–R* bond is acquired by an organism *O* *iff* emissions of *R* in the presence of *S* have been reinforced."

First let us get clear as to what particular fact-gathering is needed when we use laws of these kinds in EPF. We have already noted that where our law is of the SC type we need to determine that an instance of the appropriate *A* was indeed present before we can explain the *C* in question on the basis of the law. However, we have also noted that given a NSC law governing *C*'s, and given a particular *C*, the law will be sufficient by itself to assure us that that *C* was due to an *A*. Here no demands are placed on our fact-gathering capacities in the particular case, except for the presentation of the explanandum. However, this is true only so long as we are satisfied with an explanation in very general terms (of the level of generality at which the law is stated). And typically we are looking for more specific information about the determinants of a particular *E* when we seek its explanation. Suppose we are confronted with a particular case of an *S–R* bond acquisition, e.g. a small child's acquisition of a habit of thumbsucking while in bed. Given that this habit has been acquired (and that it is indeed an *S–R* bond) the law is sufficient to assure us that the habit was acquired because this child's thumbsuckings in bed were reinforced. But normally when we seek to explain the acquisition of a particular habit, or any other PF, we are looking for something more specific. We want to know just what the reinforcements were (organic satisfactions, anxiety reductions, encouragement from other persons, or what?), when and how frequently they occurred, and so on. That is, we want to know what there is in this particular child's case that is responsible for the habit; the assurance that some kind of reinforcement was involved is not enough. This is especially so when, as is often the case, we seek EPF's for diagnostic and/or remedial purposes. In seeking to understand Johnny's thumbsucking we may be interested in stopping, reducing, or nullifying the particular reinforcements that are keeping it going. We might also want to get into a position to prevent the further developments of undesirable habits on Johnny's part, and to know what to avoid in order to prevent thumbsucking in other children. For all these purposes we need to know just what reinforcements were operative in what situations and what their sources are. In the ensuing discussion we shall be thinking in terms of explanatory quests of this more typical sort, so that even where the law is of the NSC form, some identification of particular A's, as well as C's, is required in order to carry through the explanation.

It will be my contention that there is a variety of disabilities that may prevent us from carrying out these identifications in a particular case, even after having established the law. But first I should make explicit a certain limit to such disabilities. If our law is a low-level one (one that has been confirmed via some sort of pattern of inductive inference from its instances), then in the testing situations we must have identified a number of A's and C's to serve as a basis for the induction. We cannot have amassed direct empirical evidence for (12) without having noted a number of S–R bond acquisitions and a number of correlated reinforcements of S–R pairings. And since we have succeeded in making such identifications, we must have the capacity to do so.[12] However all that is required for the inductive testability of an A–C hypothesis, is that one be able to identify instances of A and C *in certain situations*. Satisfaction of this condition by no means guarantees our ability to identify instances of A and C wherever and whenever they occur. I now turn to the exhibition and illustration of several kinds of disabilities, each of which can prevent us from identifying particular A's in particular cases, and hence prevent us from carrying through an explanation of a particular C in terms of the law even if, *ex hypothesi*, the law does apply to the case in hand.

1. In a particular case the instances of the antecedent variables may be effectively hidden from us: in an irrecoverable past, in microscopic detail too fine for our instruments, or in a matrix of unobservable processes. Thus we may not be able to find out just how Johnny's thumbsucking was reinforced. This may be because (a) we have no way of recovering the history of the habit (although the crucial reinforcements could have been detected at the time). No records were kept, and the recollections of the parties involved are not sufficiently detailed and/or accurate to give us what we need. Or (b) it may be because the crucial reinforcing events involve

[12] Note that this point holds only of hypotheses that are tested inductively (by some pattern of inference from their instances). Where a hypothesis is tested solely by deriving lower level laws from it (in the context of some system) and then inductively testing the latter, there is no such necessary condition. However it is lower level laws that are chiefly involved in EPF, since diagnostics is generally carried on at a low enough conceptual level to permit the inductive testing of laws containing those concepts. Hence this is not a very serious restriction vis-à-vis our topic.

unconscious associations between the thumb and mother's nipple, or unconscious homosexual gratifications, and we may presently lack devices for reliably detecting such material.[13] These difficulties can bring it about that we have a particular case to which the NSC law perfectly applies, but to which we are unable to apply the law in sufficient detail.

2. The complexity of many actual field situations is too great to permit us to attain an adequate picture of the antecedent factors involved. (This could be viewed as a special case of 1.) We will best appreciate this point if we remember that many laws, and especially the most interesting ones, take the form not of a simple association of present-or-absent factors, but of a function in which the magnitude of a consequent factor is declared to depend on some organized complex of antecedent factors. Thus, frequently in order to apply a law we have to detect a number of instances of the antecedent factors and their interrelation, e.g. a number of reinforcements and the periodicity of their occurrence, and this complexity may be too much for us to handle. This is a familiar situation in physics. In mechanics we have well-confirmed laws representing the momentum of a body as a function of the vectorial sum of forces acting on it. We have confirmed such laws in simplified laboratory situations where we know what the distinguishable forces are because we have produced them, and where we keep the number of such forces manageably small. Consider then what happens when we try to employ these laws to explain the exact path described by a leaf blowing in the wind. The forces acting on the leaf at any moment result from complicated interactions of many wind currents, gusts, and eddies, constantly changing. An exact measurement of all these, at any one moment, is simply beyond our technical powers. And even if we could accurately measure each one, their interaction is too complex to permit us to map it.

Similarly if we try to trace the actual history of reinforcements that lies behind the development of a neurotic symptom (in contrast to the acquisition of a bar pressing response in a Skinner box) we will be faced with a forbidding complexity. In actual life reinforcements are seldom pure and unmixed. An expression of disapproval from another person may give rise both to dismay at having displeased the other person, *and* to gratification at having annoyed that other person, plus, perhaps, guilt over being gratified over his annoyance, etc., etc. Thus the same event can be positively and negatively reinforcing. The task of disentangling the strands looks formidable. Indeed once we step out of the Skinner box, with its satisfyingly discrete food pellets, the task of dividing the stream of experience up into reinforcement units is a bewildering one. A person lives through a continuous ever-changing stream of perceptions, thoughts, and feelings, the reinforcement value of which is shifting as continuously as any of its other features. The demarcation of appropriate units is an achievement that awaits further progress in psychology. Thus, it may be that the acquisition of a particular compulsion does exemplify a law more complex than (12), according to which the strength of an S–R bond is represented as a function of patterning of reinforcements; we may have inductively confirmed this law in

[13] Note that I am not suggesting any unobservability or undetectability in principle. I am considering cases in which there is no *conceptual* bar to detection, but where, due to practical obstacles or lack of the requisite scientific or technological development, we are in fact unable to carry out the detection.

simplified laboratory situations; and still we may be unable to apply it in detail to the development of the compulsion.

3. In cases of types 1. and 2. the factors that are actually responsible for the particular C could each be recognized as instances of A if we could only get at them. The difficulties had to do with actually detecting or interrelating those particular instances. However there are also difficulties having to do with the identification of particular factors *as* instances of A, even after their presence has been ascertained under some description. That is, even though the law does in fact apply to the situation, we may not yet have learned how to categorize this kind of situation in terms of the law; we may not yet have learned which specific features of situations like this exemplify concepts that figure in the law. For example, in the case of the reinforcement law we may, in a given case, not know which events, among those we know to have been present, are positively and negatively reinforcing. We may know that Johnny's thumbsucking was accompanied by fantasies of his father's penis but not know whether this was positively or negatively reinforcing, both, or neither.

This gap is particularly liable to develop where our laws involve high level theoretical concepts. Of course it *can* develop with laws at any level. Consider a law that is much lower level than the reinforcement law, e.g. that thumbsucking will tend to increase if parents express violent disapproval of it. Here the concepts are much "closer to observation." Nevertheless even here it is conceivable that in a particular case we are able to describe what happened in some terms or other but do not know which actions of the parents were expressions of violent disapproval. (Perhaps the setting is some alien culture in which the patterns of emotional expression are quite different from ours.) However we are much more likely to run into such difficulties when dealing with more abstract concepts like positive and negative reinforcement. At a given stage of inquiry we may be able to identify only certain kinds of positive and negative reinforcements. We may know, e.g., that reduction of hunger, thirst, and pain is, *ceteris paribus*, positively reinforcing, and the continuation or increase of such states negatively reinforcing. We may know that anxiety reduction is positively reinforcing and its continuation or increase negatively reinforcing. But there may still be large classes of events the reinforcing quality of which we are at present unable to determine. It is an attractively simple hypothesis that any event is positively reinforcing to the extent that it is drive reducing. Another unifying hypothesis would be that an event is positively reinforcing *iff* it produces pleasure (heightens hedonic tone) and negatively reinforcing *iff* it produces displeasure (lowers hedonic tone). But even if one of these hypotheses is correct (and insofar as they are clear enough to be assessed they are extremely dubious) we have only pushed the problem back to that of applying the very abstract concepts "drive reducing" or "pleasure producing." Here too we remain ignorant of many of the conditions of drive reduction and pleasure production. We are often in a position to give a detailed case history, e.g., of a neurotic patient, without being able to describe the happenings in *these* terms.

Take another example, again from psychology. Festinger's "theory of cognitive dissonance" states that to the extent that there is a preponderance of dissonant over consonant relations between the elements of some cognitive field, there will exist tendencies to change the situation. Such changes include altering cognitive elements, adding new elements so as to increase the relative proportion of consonant relations, or decreasing the importance of some of the terms of dissonant relations. Thus if a person finds himself in sharp disagreement with most of his associates on most matters, he will tend to either change his views, convince others of his positions, find other people who agree with him, or lower his evaluations of his present associates. Applications of the theory have been made to a number of domains, including choices between alternatives, induced compliance, and the holding of unpopular or obviously disconfirmed opinions. For situations of these kinds identifications have been made of cognitive elements and of the relations between them which can be termed consonant and dissonant. However there may be other phenomena the dynamics of which in fact conform to the principles of the theory, but where we are not presently able to make such identifications. The formation and dissolution of friendships *may* be one such case. If this were the case, we would be unable to use the principles of the theory to explain such transitions just because we are presently unable to discriminate, within a mass of information we have about such cases, the crucial consonant and dissonant relations between cognitive elements.

A hard-nosed operationalist would presumably deny the possibility of the sort of disability presently under discussion, on the grounds that insofar as we are unable in a given domain to identify cases of, e.g., reinforcement, we do not have a clear (valid, meaningful, scientific, etc.) concept of reinforcement in that domain. If we are able to identify reinforcements only for rats in Skinner boxes, then our concept of reinforcement is a concept of reinforcements for rats in Skinner boxes; it is illusory to suppose that we have a concept that in fact applies to the development of neurotic symptoms in human beings, without our being able to make the applications in particular cases. But it is an old story by now that such an operationalism is far too crude, far too single-tracked to do justice to the structure of scientific inquiry. A scientific concept is not solely constituted by the resources we presently have for detecting instances. Over and above all such techniques of detection and measurement, and, indeed, furnishing the foundations of such techniques, is a concept that draws its content from nomological entanglements in which it is taken to stand both in common-sense and in scientific theorizing. Thus the root concept of a positive reinforcement is that of some gratification or satisfaction, the root concept of a dissonant relation is that of some strain or tension in simultaneously holding two beliefs or attitudes. These vague and inchoate, but fundamental, notions undergird any explicit procedures we have for applying them to particular types of situations. These root notions may get enriched and/or altered in the course of devising techniques of application, but they have a genuine content, and an essential one, apart from any particular set of applications. Our techniques of detection are developed under the guidance of the root notion: it is because of the fundamental position of the latter that radically different procedures can be said to be measures of the same factor; and it is because of this that it makes sense to speak of seeking for ways to identify the *same* factor in hitherto uncharted areas. The development of scientific concepts is most accurately viewed not as the creation *ex nihilo* of new concepts as we invent new techniques of detection and measurement, but rather as the unfolding into effective use (*and* modification) of a seed that was already rich in potentialities for application. Hence we can meaningfully envisage the possibility that theoretical concepts we now possess do in fact apply to certain situations even though we do not now know how to carry out the application.

Let me summarize this survey of factors that can hamper our efforts to use general laws in the explanation of PF's that fall under them, even after the laws have been established. First the law itself may be of such a form that it does not conclusively or uniquely determine any particular explanation of a given *C*, even after we have ascertained a correlated *A*. We have seen that NC laws are of this sort, and also the laws that involve some qualification or deviation from a strict SC or NSC form, the types we categorized as "open-ended" and "tendency" laws. This leaves strict SC laws and strict NSC laws as the only types that do have an unambiguous bearing on the explanation of particular *C*'s, and we are all too seldom in possession of laws of this form. Second, even if we have established a strict SC or NSC law, and know that the *C* in question is due to *some A* or pattern of *A*'s, it does not follow that we are able to apply it to the detailed explanation of that *C*, for we may be unable to determine *what* instances, or even what kinds of instances, of *A* were playing an efficacious role. Such an inability may stem from several sources. First, the individual instances may in fact lie beyond our present powers of detection. Second, the complexity of this set of instances may be too great for us to be able to unravel and interrelate them. Third, we may be unable to determine which of the neighboring events are in fact instances of *A*.

The cumulative effect of this recital should be to emphasize the divide that separates pure science, focused around the formulation, confirmation, systematization, and explanation of general laws, from the diagnostic enterprises that are concerned with EPF. It should now be abundantly clear that not only is the process of establishing general *A–C* laws not the same thing as the process of explaining a particular *C*; in addition, there is no automatic, or even straightforward, transition from the one to the other. Thus it is a serious mistake to suppose that because pure science is centrally concerned with establishing general laws, it is therefore centrally concerned with the EPF. A recognition of the looseness of the connections between the two enterprises can help us to appreciate this point, and vice versa.

V

A failure to appreciate the complexities of the relationship between general laws and EPF can lead us astray in other ways as well, of which I shall just mention one. If we suppose that the possession of a general law guarantees our ability to explain the PF's that fall under it, we will be led to regard the extent to which we can explain the PF's in some area as a sensitive index of the degree of advancement of the science of that area. If one thinks of psychology, e.g., as essentially directed to the explanation of behavior, *and* understands this to mean that psychology is essentially trying to explain particular instances of behavior, then he lays himself open to the charge that since the psychologist (at least the theoretical psychologist) is generally no better at explaining particular instances of behavior than the man in the street, psychology has not made much progress. "They still can't explain Johnny's temper tantrums; they have not delivered on their promise." But in the light of the points made in this paper we can see that the promise has been misconstrued. Theoretical scientists should not be expected to excel at explaining particular facts in the field, or even to provide resources that can be mechanically applied to do so. That is not their job. The question of just how healthy or sick psychology is at present is a thorny question into which I cannot enter here in any general way. But what does emerge from this paper is that the extent to which we are presently able to give adequate explanations of particular actions, feelings, neurotic symptoms, personality developments, etc., may not accurately reflect the degree of development of psychology as a science. The points made in **III** and **IV** show that a science may be highly developed as a pure science, i.e. as an enterprise directed to the establishing, systematization, and explanation of general laws, while we are largely powerless to explain PF's falling under those laws. The pure scientist may have done his job well, while the diagnostician may still be strapped by one or more of the disabilities brought out in those sections. To be sure, in the heavenly city of science, not only will we have a comprehensive set of general theories, but all particular facts will be clearly revealed in the dazzling light emanating therefrom; but it would be too much to expect that we should move toward this consummation at an equal pace on all fronts.

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