

Hempel recommends that the "ambiguity of statistical generalization" be handled by requiring 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" (Carnap's formulation of "the requirement of total evidence"). Of this requirement, Carnap says it has "no analogue in deductive logic," but Hempel comments, ". . . it seems more accurate to say that the requirement is automatically met here."⁴⁸

In its application to explanations, it seems clear that this requirement is neither absent nor automatically present. We must in fact add it to the condition, and make it more specific by requiring that an explanation be acceptable for a phenomenon only so long as no facts are known about the circumstances surrounding the occurrence of the phenomenon which the explanation cannot accommodate. The explanandum is rarely a complete description of everything that is known to occur, and one could never support the claim that it should be since this would frequently be unfulfillable. But an explanation has to pass other tests besides (and sometimes instead of) supporting an inference to the explanandum, and this is one of them.

I conclude with a comment on the over-all argument in Hempel's discussion of the differences between statistical and nomological explanation. The paradox of ambiguity has its roots in a misuse of statistical inference that is, as he points out, quite well known in the literature. Hence "the ambiguity of statistical explanations" is only apparent and not real, since one ought to have borne in mind the requirement of total evidence. Hence it cannot be an identifying property of such explanations. And, according to my arguments, prophylactic treatment is necessary in both of Hempel's realms of explanation. I think a combination of these two kinds of argument applies—though I have not shown this—to the "non-additivity of statistical explanations." And it seems quite clear that taking account of the probability of the categorical statements in nomological explanations will show that they share with statistical ones the property of being more or less likely. It seems to me that there remains only the definitional difference that the internal inference is in one case deductive and in the other inductive.

⁴⁸ See pp. 138, 139, this volume.

Explanation, Prediction, and "Imperfect" Knowledge

The Attack on the Deductive Model

Can we explain events after they happen which we could not have predicted beforehand? Do history and the other studies of man differ from the physical sciences in this respect? Do a man's character and motives satisfactorily explain his actions or do these in turn need explaining? Questions of this sort are raised, not for the first time, by certain recent criticisms of the so-called deductive model of explanation. This model holds, briefly, that to explain an individual fact we deduce it from one or more other such statements in conjunction with one or more generalizations or laws. A law, in turn, may be explained by deducing it from other laws. Prediction, upon the model, has the same logical form as explanation. In predicting something as yet unknown, one deductively infers it from particular facts and laws already known. We infer from given premises stating what is known, what so far has not been known, whether it be in the past, present, or future, or whether, as in the case of laws, time is irrelevant. It follows that if anything can be explained deductively by a set of premises after it has occurred, it could in principle have been predicted from these premises before the event. Nor does it make any difference whether the premises are statistical or deterministic. If they are deterministic, we may predict an individual event; if they are statistical, only statements about classes of events may be either explained or predicted. Virtually all those who accept the deductive model hold that it applies not only to physical but also to human phenomena, whether individual or social, whether in the past or in the present. Recently popularized among philosophers of history by a lucid exposition of Hempel's,¹ the deductive

NOTE: This paper was read, in part, at the 1960 International Congress for Logic, Methodology, and Philosophy of Science held at Stanford University.

¹ C. G. Hempel, "The Function of General Laws in History," *Journal of Philosophy*,

model traces its lineage through John Stuart Mill and David Hume back to Galileo.

In physical science, ever since physicists abandoned the anthropomorphic-teleological “why” in favor of quantified relations among relevant variables, explanation has meant deduction. John Stuart Mill, who had the clearest anticipatory vision of what a scientific study of man and society could be, extended the notion to these areas. The extension, always bitterly resented, is now once again under attack. A certain piquancy is added to the current attack by those who now attack the deductive model for the physical sciences as well. This global nature of the attack gives the current controversy its special interest. The assailants either radically reject the model *in toto* because it is *deductive*, or, more moderately, reject it not because it is deductive but because of the nature of the *premises* from which the deductions are allegedly made. In either case, the critics have given what appears to be a “new look” to all the old and tired arguments against a science of history and of man—the arguments from uniqueness, from freedom, from mind, from complexity, from values, and the rest. Nor is that all. The extremists challenge not only explanation by deduction but the very notion of valid deduction itself. I shall first discuss the more prevalent radical position of those who reject the standard model simply because it is deductive. Since there is a common core to the radical and the moderate views, it will then be possible to treat briefly of the more moderate criticism which attacks only the nature of the premises. It is indeed the broader philosophical implications of this common core, cutting much deeper than the issue at hand, that I am particularly concerned to exhibit as clearly as I can. The arguments of moderate and extremist alike arise from an overemphasis on language as it is used to communicate with others, to the neglect of language as it is used to describe the world. This preoccupation in turn results in a notion of “conceptual analysis” with philosophically untenable consequences. I shall try to show why this is so and, in particular, why the criticisms of the deductive model, based on this preoccupation and that notion, are without force.

The rejection, by the extremists, of deductive explanation is but part

39: 35–48 (1942), reprinted in *Theories of History*, P. Gardiner, ed. (Glencoe, Ill.: Free Press, 1959). Frequently cited also is the more detailed article by C. G. Hempel and P. Oppenheim, “The Logic of Explanation,” in *Readings in the Philosophy of Science*, H. Feigl and M. Brodbeck, eds. (New York: Appleton-Century-Crofts, 1953).

and parcel of a more sweeping rejection of formal logic. Three related arguments are used to support this double rejection. First, appealing to ordinary usage they find, to no one’s surprise, that ‘deductive’ is applied to many different kinds of inference. Whenever locutions such as “because so and so, therefore,” “this, so that,” or “this involves that” occur, we may accordingly “properly speak of ‘deductions.’”² ‘Explain,’ too, is revealed to have many different uses. The deductive model, assuming as it does the logician’s notion of ‘deduction’ and a particular use of ‘explain,’ doubly affronts ordinary usage and is, accordingly, doubly “irrelevant.” Second, the critics deny that anything corresponding to the logician’s narrow, tautological sense of ‘deduction’ actually occurs in common sense or in science. Since there is no deduction, naturally there is no such thing as deductive explanation. Finally, they maintain that an appeal to so-called conceptual analysis is as conclusive as an appeal to logical truth. This third reason, as we shall see, is the most fundamental of all.

Common-Sense Facts vs. Philosophical Claims

Do we ever predict what we cannot explain or explain what, before the event, we cannot predict? We do it all the time. We predict a future event, like the outcome of next week’s election, even though we cannot *deduce* it. Moreover, whatever the outcome, we then proceed to explain it. Of course we do. We explain a man’s sudden death by his being struck by a car. The historian too explains past events that he is in no sense trying to predict. To proclaim these uncontroversial common-sense facts is one thing. To propound a philosophical thesis is quite another. At issue now is the philosophical claim that accompanies the announcement of these everyday “explainings” and “predictings.” Specifically, the claim is that no laws either are or need be deductively invoked, *either explicitly or implicitly*, to permit the inference from some individual facts to another which they explain or predict. Moreover, an appeal to laws is held to be “wholly unnecessary” for identifying the causes of an event. The causes of the Civil War, of my car breaking down, of why a test tube blew up, of a carpet’s being stained, all can be “understood” and understood with “primeval certainty” by the appropriate people, some of whom are simply endowed with a “well-developed capacity for identifying causes,”

² See G. Ryle, *The Concept of Mind* (London: Hutchinson, 1949), p. 300; S. Toulmin, *The Uses of Argument* (Cambridge: Cambridge University Press, 1958), p. 121.

without appealing, either implicitly or explicitly, to laws.³ Marshaling the evidence against the deductive model, with its implied symmetry between explanation and prediction, the critic comes triumphantly upon history. We are even treated to a dithyramb on history as the "mother subject for explanations," just because it is no different from common sense.⁴ Certainly, the historian now often explains what he cannot predict. However, this obvious fact does not in itself justify making history the paradigm case of "sound explanation." Nor, of course, does the mere occurrence of deductive explanation and prediction in physical science, should one grant that it does occur, *in itself* justify making physics the paradigm case, as is customary with proponents of the deductive model. A philosophical thesis is not merely a description of what does or does not happen to occur. My primary purpose is to examine the grounds for the philosophical claim that is associated with, but I believe not supported by, appeal to certain indisputable facts of common sense. In the course of this examination, I shall also try to show how the deductive model can consistently account for these facts, as well as for the present and, for all we know, perhaps permanent situation in history and related areas.

The Appeal to Ordinary Usage and to "Understanding"

The assailants, probing the idiom, discover as many senses of 'explain' as there are possible answers in common speech to the question Why? In none of these senses do they discern any appeal to deduction in the logician's sense. We explain to a child how to multiply fractions; we teach him certain rules. We explain the meaning of one term, using others. We explain the structure of the ruling class of a given society—we describe a pattern. We explain the symbols of formal logic to a class—we describe what is made to stand for what. We explain the rules of chess. We explain a man's actions by pointing out his motives or his purposes. We explain the connection between the moon and the tides. Perhaps, in this case, we merely state a constant conjunction. Or we may derive it from the Newtonian law of gravitation in conjunction with certain other statements. And so on, and so on. The ordinary uses of 'explain' are manifold indeed. Only the last case involves deduction. So there is at least one case which does involve it. In spite of this case and in spite of the admitted

multiplicity of uses, there is a drumming insistence that all of these uses of 'explain' are "essentially" the same—that there is a true meaning of 'explain' common to all of them, except, ironically, deduction. This true meaning is "the ordinary sense," the only one which "really explains."⁵ Ordinary use is thus not merely described. It is appealed to as the standard that everything else must meet if it is to "count" as an explanation.

Just as individual facts, like the Civil War or a car breaking down, can be "understood" without appeal to laws, so with other things. A teacher explaining to a student how to multiply fractions, an art critic explaining why one painting is better than another, a nuclear physicist explaining the principle of complementarity, a father explaining the rules of chess to his son—each, like the mechanic explaining why a car broke down, has his own way of telling whether or not the other person understands what is being explained to him. What is the sense of 'explain' common to all these various uses—explaining facts, rules, a symbolism, how to do something, and so on? The answer, though disappointing in its triteness, reveals the difference between the study of language as a medium of communication and its study as a description of the world.

Every teacher has a way of finding out whether or not a student has understood what he has been talking about. This is "the ordinary sense," the "essential sameness" of meaning that preoccupies the critics of the deductive model. Has someone understood what we have said? Is it "intelligible" to him? To find out, we ask him certain questions, depending upon the kind of thing we are testing his understanding about.⁶ If he answers appropriately, then we have successfully communicated with him. This psychological sense of understanding what is conveyed by a communication does indeed underlie most of our ordinary uses of 'explain.' The critics, understandably concerned to deny that they are engaged in factual inquiry, insist that this meaning of 'intelligibility,' 'understanding,' or 'explain' is not psychological. To be sure, one of the ordinary uses of 'psychological' is as a synonym for 'mental'—something going on in someone's mind. Another is of something "subjective"—varying from individual to individual. The critics protest that they mean nothing either mental or subjective, in this sense, and hence nothing "psychological." They mean rather a behavioristic test. We can tell that someone under-

³ W. Dray, *Laws and Explanation in History* (London: Oxford University Press, 1957), p. 79; Scriven, in Gardiner, p. 449.

⁴ Scriven, in Gardiner, p. 452.

³ M. Scriven, "Tautisms as the Grounds for Historical Explanations," in *Theories of History*, P. Gardiner, ed. (Glencoe, Ill.: The Free Press, 1959), p. 456 and *passim*.

⁴ *Ibid.*, p. 458.

stands something by what he says or does. It is thus allegedly not psychological because it is behavioristic, not mentalistic, and it is not subjective because we can devise a test by which anyone can tell whether any other fellow understands or not.

Only if one is frozen in the science of the nineteenth century can one believe that psychology is concerned with the “working of men’s minds” rather than their behavior, verbal or otherwise. If one also happens to feel that a science of psychology is unnecessary because we already “know well enough” by “ordinary good sense”⁷ why people behave as they do, then one may persuade oneself that the suggested test for “understanding” is not psychological. But this is verbal magic. How we learn, communicate by means of, and react to, language as part of our social environment are all facts about human behavior. The appeal to ordinary usage and the interest in communication leads to a notion of ‘explanation’ that depends not upon the way the world is, the connections among things, but upon *something in us*, upon the way we respond in the game of question-and-answer that we play, not with the world, but with each other. Later on I shall have more to say about the relevance for the attack on the deductive model of this curious combination of behaviorism with a rejection of the science of psychology. For the present, it seems clear that, like the word or not, this remains a psychological sense of ‘explain.’ And this sense of the term does not tell us what *justifies* saying that an event happened one way rather than another, nor—apart from a quaint appeal to “judgment” and special talents—does it tell us how it happens that we are able frequently to make correct inferences from what we know to something past, present, or future that we do not yet know. In other words, it doesn’t answer the philosophical questions about explanation and prediction.

Language as Communication vs. Language as Description

Among all possible means of communication, language is the richest and most subtle. By language we convey to others our desires, plans, attitudes, and, among other things, information about how to do certain things or about the way things are or seem to be. How we learn a language and, having done so, contrive to communicate successfully with each other are, to repeat, questions about human thought or behavior in

⁷ Ryle, *The Concept of Mind*, pp. 52, 324, 326. An incisive critique of some of Ryle’s views on psychology may be found in M. Mandelbaum’s “Professor Ryle and Psychology,” *Philosophical Review*, 67: 522–530 (1958).

social situations. They are thus to be answered by sociopsychological investigation, whether this be our everyday experience or the controlled methods of the social sciences. Speculation from, or analysis of, how we speak can at best offer cues to the factually correct answers to questions about the so-called higher symbolic processes which are involved in linguistic behavior. However, since much of what we try to convey to each other by means of language is information about the world, language also has a descriptive function. No matter how complicated the processes by which we understand one another may be, they can only occur because language is also used to describe the world, including ourselves as part of it. The study of language as description is the study of terms and statements as they are used to make such assertions, no matter how we came to acquire these uses. Given this language that, indubitably, has been socially learned, philosophical questions may be asked about it. By analysis of the language itself we try to see what there is about it that, *once it has been understood*, enables it to tell us something about the world and also what it actually does tell us about the world’s content or its form.

A physicist’s published report of the theoretical significance of an experiment and his laboratory conversation are two quite different things. The latter is elliptical and context bound in a way his report cannot be. He may “explain” an observed phenomenon to a co-worker by simply mentioning some other fact, very likely something that he has done. The co-worker will “understand” him. The information necessary to this understanding is supplied by the total context within which the conversation occurs, both the social situation itself and the knowledge the two men share. The published report, however, will have to be considerably more circumstantial about the connection between the experiment, the observations, and what it was performed to test. Moreover, the report will use terms with constant meanings throughout. The sentences will be complete in the sense that they will be true or false, independently of the time or place at which they are read, unlike, say, “The light just went out.”

Since the physicist writes for other physicists, even his report may not explicitly mention all the information that justifies certain statements, for again there is a context of shared knowledge that may be taken for granted. But the obvious differences between the explicitness of the report and laconic laboratory conversation makes clear what a complete report would be like and, under some circumstances, must be like. Only a complete report, including that which is spoken as well as that which

is left unspoken, presents the observational and logical grounds for any claim. Ordinary communication relies heavily on the linguistic and non-linguistic context. To examine language as description, this context must be supplied. A context-free language is artificial only in the sense that the physicist's report is "artificial" compared with his conversation. It makes explicit what, in ordinary communication, is supplied by the total context. By filling in the context, we know what it is that concepts are being used to refer to and what statements about the world ordinary sentences are being used to make.

The logician uses a similarly improved language to explicate the notions of logical truth, deductive proof, logical impossibility, and the rest. The logical notions articulate the criteria by which, in common sense and in science, we are *justified* in asserting that someone is being inconsistent or that an observation refutes a generalization. The logician's improved language is useful to the philosopher precisely because and only insofar as it is a reconstruction of a large part of the language that we speak. The logician's notions of "logical truth," "deduction," and the rest, correspond to, though they do not duplicate, certain indispensable concepts in ordinary use. The logician's term 'analytic,' for instance, is not identical with the ordinary uses of 'necessary,' but it corresponds to the use in such statements as 'It is necessary that white horses are white.' The philosophical logician thus clarifies *one* use in common speech of 'necessary' and, at the same time, shows how it differs from other uses of the term, as in 'It is necessary that all men die.'

Why Explanation Must Be Deductive

Grant for the moment that deduction is one core meaning of explanation, at least within science. This is an anthropological fact like any other about the linguistic behavior of a certain group of people. Why give this use pride of place in preference to all others? Not, as some think, because of some scientific prejudice in favor of physics as the model inquiry. Rather, it goes the other way. Physics is an advanced and "model" science precisely because it does provide deductive explanation and prediction of its phenomena. To show why this is so requires explication of *this* use of 'explain.' The philosopher surely is not merely an anthropologist describing the linguistic habits of the natives—whether they are plain men, members of the class of historians or of physical scientists—and what different things among them "count" as explanations. Such description

is at best preliminary to the task of exhibiting the structure of a reasoned explanation, showing which structure logically justifies that which is to be explained. In philosophy and elsewhere an appeal either to a definition or to a logical truth is conclusive. One does not quarrel with a statement which is true merely by virtue of a convention about the use of words. Similarly, if a statement is backed up by reference to other statements from which it logically follows, one does not argue, as long as one accepts the truth of the premises.

The purely verbal case is too trivial and obvious to need elaborating, but what makes the appeal to premises conclusive? Briefly, because the conditional corresponding to the premises and conclusion of the argument is a tautology or logical truth. In a context-free language we can formally characterize the notion of "tautology." Consider "It's raining or it isn't." One must eliminate vagueness—the drizzle beloved of students—and the event must be located in time and space before we can characterize the statement as a tautology in the strict sense. Only when these are implicit in context is the statement even an everyday tautology. For that matter, as has been insisted, only then is the expression even a statement. Only for *statements* can we state the criteria, which are characteristics of *the statements themselves* and not of anyone's responses to them, that make them tautologies or logical truths. If the generalizations and individual statements of fact serving as premises are accepted as true, then, because of the tautological connection, the conclusion *must* be true. This and this alone is the virtue of deductive explanation. Once such terms as "must," "guarantees," and "logically implies" are clarified, then it is clear why deduction and deduction alone "justifies" the conclusion. At the same time, it is also clear why any other kind of explanation of individual facts cannot possess this conclusiveness. Either the explanation is deductive or else it does not justify what it is said to explain.

All this is not to "set limits" to what may constitute a sound explanation, but to explicate *one* sense of sound "explanation" which is the only one that is conclusive for the reasons just mentioned. For the philosopher concerned with language as description, the task is to show what, so to speak, in the facts themselves, or, to be accurate, *in the statements asserting them*, rather than in the mind or behavior of a particular person or group of persons, makes one or more statements a "reason," in a precise logical sense of reason, for one or more others. What in the statements themselves makes one statement a prediction from others, one confirm a

general hypothesis, another refute it? Deductive explanation is the only answer to this question, insofar as language is used to speak, not just about how we speak, but about the world.

One particular appeal to ordinary usage to bolster the claim that scientific explanation is not deductive verges on the bizarre. Reading 'deductive' to mean 'syllogistic' in the Aristotelian sense, it is concluded that scientific explanation, since not syllogistic in form, is not deductive.⁸ Explanation in science is indeed not syllogistic, at least in the most interesting cases, namely, those involving quantification, relations, or both. But I think it may be said safely that however the half-educated plain man may use the term, no scientist since Galileo has so narrowly conceived "deduction" and certainly no logician for almost a century. Yet, insofar as these critics concede a correlate to strict deduction in ordinary speech, including science, they perceive it to mean something like Barbara. Hence, if not Barbara, then not deductive. Words *do* mean only what we say they mean. But one use of a term hardly justifies the assertion that nothing corresponds to another and different use of that same term.

Like 'deductive,' the word 'explain' has, as we saw, many uses. To assail the deductive model because it does not apply to all these uses is irrelevant at best, puerile punning at worst.⁹ Deduction is not put forward as the model for those uses of 'explain' when we speak, say, of explaining (i.e., describing) the structure of the Egyptian ruling class, a symbolism, or how to play chess. The model is proposed for only one but one very important use of 'explain.' In particular, the occurrence of certain events, like an eclipse or an earthquake, can, upon the model, sometimes in practice and always in principle be explained and predicted deductively. Practically, we can both explain and predict the occurrence of an eclipse. We can only explain the earthquake after it happens. The critics' most plausible case rests, of course, on such all-too-common "earthquake" situations. But they are not content to rest on such cases. Though conceding at one place that Newton "did achieve a large number of mathematically precise and scientifically illuminating deductions [sic!] from his theory," this is, to put it moderately, not the prevailing tenor of the argument.¹⁰ What one hand gives, the other snatches away. Or perhaps it was only Newton him-

⁸ Scriven, in Gardiner, p. 462; S. Toulmin, *Philosophy of Science* (London: Hutchinson, 1953), p. 25.

⁹ Scriven, in Gardiner, pp. 463, 468.

¹⁰ M. Scriven, "Explanation and Prediction in Evolutionary Theory," *Science*, 130: 477 (1959).

self who could accomplish the feat of deduction. In any case, the claim is that not even in physics do we really have deductive explanation. Apart from the untenable, because false, appeal to usage, the argument has, so to speak, two facets or takes two different forms. The first grants that laws *do* play a role in some explanations, but such explanations are allegedly still not really deductive. The other holds that explanation of an individual fact is always by other such facts without the intermediary in any way of laws. After a few preliminary comments about the structure of classical physical theory, I shall discuss these two different arguments in the order mentioned.

"Perfect" Knowledge and Deductive Explanation

Celestial mechanics is a paradigm of what has usefully been called "perfect knowledge."¹¹ Nonatomic thermodynamics is another. In the first theory, mass, velocity, and distance are variables that interact only with each other; in the second, volume, temperature, pressure, and a few other variables of this sort do the same. In each case, no variables that in fact make a difference have been omitted from the expressions of the theory. These theories are *complete*. Nothing that happens at any other time or place than that being considered affects the behavior of the properties with which the theory is concerned, or, at worst, we know how to take account of these outside influences in our predictions or computations. The system, in other words, is *closed*. The laws of these theories are *process laws*, that is, the values of any one variable at any time can be computed by means of the laws from the values of *all* the others at any other time. We can predict the future course of all our variables or compute their entire past history; we know how changes in any one variable produce changes in any other; we know to what extent by tinkering with the system we can bring about desired changes, to what extent we are powerless.

Such knowledge may, not unreasonably, be called "perfect," for there is clearly nothing else we could possibly want to know, as far as these variables are concerned. Nor is there any unreasonable sense of "perfection" implied by this notion. Every measurement has its limits of accuracy. Inductive generalization may be overgeneralization, as indeed we

¹¹ The distinction between "perfect" and "imperfect" knowledge is Bergmann's. For a thorough discussion, see G. Bergmann, *Philosophy of Science* (Madison: University of Wisconsin Press, 1957).

now know about the Newtonian law of gravitation. These frailties are common to all empirical science and do not affect the nature of the deductions we can make from such theories, when we assume that the laws are true and that our measurements are accurate.

From the Newtonian law of gravitation in conjunction with the positions of the sun and the planets at any one time, we can predict or postdict their position at any other time. Thus we can deductively predict future eclipses or explain present and past ones. The laws of Newton may be applied, of course, to a vast number of vastly different situations, terrestrial as well as celestial. If we know enough about the initial conditions, then we can predict what will happen. Using these laws, we can predict how long it will take an elephant to slide down a green, grassy hill, if we know only his mass, the angle of inclination of the hill, and the coefficient of friction. Or we can predict where and when a cannon ball will land, if we know but a few relevant variables. In some cases, where we fail to predict, as when a bridge collapses under a certain load, we could have done so, if we had taken the trouble to find out the state of the system, that is, the initial conditions, before the catastrophe. If we did not take the trouble or if it was too complicated to do so, then, after the event, we deductively explain it by reference to the laws and to what the conditions must have been to make the bridge give way.

“Certainty” of Premises vs. Deductive Validity

How, then, in the light of all this can the critics nevertheless maintain that explanations in physics are not deductive? The physicist, it is pointed out, might be wrong either in deciding that a particular universal law applies to a given set of conditions—whether or not they are within the scope of the law—or in describing the initial conditions. The physicist must, therefore, like the historian, use “judgment” and even “empathy” rather than formal deduction in making his inferences.¹² Or so the argument goes.

However, the choice of premises, or even the use of false ones, is not in the strict sense a logical matter and does not affect the validity of a deduction. The validity of the deduction itself is here confused with the error of using, say, a first approximation like Boyle’s law where only a more general gas law would do, or in believing that the effects of friction

¹² Scriven, in Gardiner, pp. 459, 462.

are negligible when in fact they are not. Once the scientist has used his “judgment,” which is indeed a psychological matter dependent on his skill and training, then the deduction follows. The explanation is deductive, whether or not we are “certain” about the premises, either the law or the initial conditions. A prediction in the same circumstances is also deductive. The scientist, in fact, tests his judgment by deductively inferring what must be the case if what he believes to be the applicable laws and the true initial conditions really are so. Premises deductively imply a conclusion no matter how certain or unsure we may be about their truth. Logical entailment (or inconsistency) is a property of the statements themselves, not of our knowledge about them. If the prediction were not deductive, then there would be no reason why its failure should cast doubt upon the premises. In fact, it shows that at least one of them is false. In theoretical science, when a prediction fails, this generally, though not necessarily, casts doubt upon the laws. In engineering or applied science, on the other hand, the initial conditions become suspect. Far from requiring “exact truth” for its premises, all that the deductive model requires is exact statement of a hypothesis about their truth. The hypothesis is then tested by the “exact deduction” which, I am sorry to have to repeat, does not mean and never has meant only the syllogistic deduction with which the critics sometimes equate it.

Hempel’s careful statement of the deductive model unfortunately uses an illustration that, from the point of view of the principle involved, is inadequate. Understandably, for expository purposes, trying to avoid the mathematical complications of those areas where perfect or near-perfect knowledge exists, he uses a homely, everyday illustration, the breaking of a car’s radiator in freezing weather. Hempel’s so-called failure here is not one of deduction, but a failure of complete knowledge. He cannot state all the initial conditions that will permit him to deduce that the radiator broke. If one appreciates that philosophical explication is explication of the principles involved, this is no failure at all. Once again, only because there is a deductive connection between the laws of physics and, among other things, the state of the weather and insufficient antifreeze in the radiator, have we any good reason to assert that the cold rather than the sparrows who daily perch upon the hood explains the breakage. The physicist does not judge “inductively . . . what the explanation is.”¹³ Cor-

¹³ *Ibid.*, p. 457.

rectly stated, he makes an inductive judgment about the premises. And to make this correction is not, as is claimed, to “convert” either the premises or the judgment into a deduction. We want to know whether a given statement is deductively entailed by certain others. This is a logical question. To import considerations about what the scientist happens to know, certainly or otherwise, or even, if knowing, what he happens to assert explicitly or to “quote,” is to doubly muddy the waters by first introducing irrelevant psychological factors and then confusing context-bound sentences and complete statements.

In his zeal to confute the deductive model, Scriven makes the remarkable statement that even in those parts of physics where what I called “perfect knowledge” exists, namely, classical thermodynamics, “*deduction of the exact values to be explained from such laws is a matter of chance.*”¹⁴ This conjures up an image of physicists writing down conclusions at random. But this is not quite what happens. A quantitative science uses measurement to obtain the values of its variables. Measurement, as I mentioned before, has its limitations. Every measurement has a so-called error component associated with it. This “error” is the deviation from a mean value that can be expected under repeated measurements of the same variable. In other words, in repeated measurements of, say, the length of a bar, the quantity obtained as a result of the measurements will tend to cluster around a certain value. The result of a measurement is thus not a single value but a frequency distribution determined by the nature of the measurement. This means that the premise expressing the measured initial conditions always has a statistical component. It means further that the conclusion deduced from such premises must also be about a frequency distribution, not a single value. There is no deduction “by chance” or any other way of a single, exact value. There is instead a strict deduction, by the probability calculus, of a so-called “chance” variable, that is, a frequency distribution, which is quite another thing.¹⁵

But we have only begun to probe the depths of confusion that these critics display about the use of statistical premises. To probe deeper, we must turn to the notion of “imperfect” knowledge. Moreover, the attack on the deductive model derives its prima-facie plausibility from those

¹⁴ *Ibid.*, p. 460.

¹⁵ An excellent discussion of this issue and others related to the comments below on the use of statistical premises may be found in P. E. Meehl, *Clinical versus Statistical Prediction* (Minneapolis: University of Minnesota Press, 1954); cf. especially pp. 56ff.

areas about which we have only imperfect knowledge. It is well to be clear, therefore, about what we can and cannot do when “imperfection” prevails.

“Imperfect” Knowledge and Deductive Explanation

Perfect knowledge is the ideal, actualized only in certain branches of physical science. Elsewhere, as in biology, economics, sociology, psychology, and the social sciences generally, knowledge is conspicuously “imperfect.” We do not know all the variables that affect, say, a person’s resistance to disease, or his behavior under certain circumstances, or the price fluctuations of certain commodities. Our theories in these areas are not complete, nor do we know fully the conditions for closure of such systems. Since we do not know all the factors that make a difference to the variables with which we are concerned, we also have no process laws. From the values of the variables at any one time, we cannot predict their value at *all* other times. Yet, the social and biological sciences have developed techniques to compensate for lapses in closure and completeness. The most important of these techniques is the use of statistical concepts. Which face will turn up when a die is cast is determined by numerous causes—the center of gravity of the die, the force with which it is thrown, and so on. An attempt to calculate the results of each throw by means of the laws of mechanics is practically hopeless because of the difficulty in precisely measuring all the initial conditions. Instead, we represent, as it were this multiplicity of causes by a probability distribution for the attribute in question. The use of the statistical concept marks our ignorance of all the influencing factors, a failure in either completeness or closure or, usually, both. Similarly, the social scientist, deliberately selecting for study fewer factors than actually influence the behavior in which he is interested, shifts his goal from predicting individual events or behaviors to predicting the frequency with which this kind of behavior occurs in a large group of individuals possessing the circumscribed number of factors. This use of statistical concepts, due to lapses in closure and completeness, differs from their use for errors of measurement. Even assuming perfect observation, they would still be necessary.

Statistical knowledge is not the only kind of imperfect knowledge. We also have nonquantified imperfection. Any law, whether it be about physical objects, persons, or societies, is “imperfect” if it does not permit us to compute (predict or postdict) the state of the system, either an

individual or a group, at any moment from its state at one moment. Consider the assertion that a boom in trade is always followed by slump and depression. This is imprecise with respect to time, for it does not tell us exactly when the later events will follow the earlier one, nor how long each will last. Moreover, its concepts have a fringe of vagueness that make it difficult to tell precisely when we have instances of the kinds of events mentioned. In psychology, the laws of learning that make essential use of the past history of a person in order to predict his future behavior are also imperfect. The equilibrium laws of physics, whose concepts are not vague, are nevertheless imperfect because though they tell us that under certain conditions no change in certain respects will occur, they do not tell us what will happen if these conditions are not fulfilled. To be sure, the equilibrium laws of physics are derivable from the process laws of the theory, but for those of economics no such perfect laws are available. In general, imperfect laws are indefinite with respect to time, or hedged in by qualification, or they are statistical.

The inadequacies of such "imperfect" knowledge do not affect the possibility of deduction. Not only do we sometimes know enough to deduce some of these laws, like the law of the lever or certain statistical laws of physics, from process laws, but all kinds of deductions can be made *from the imperfect laws themselves*, whether or not they are in turn deducible from something else. An explanation utilizing imperfect laws as premises is not the same as Hempel's "explanation sketch." The latter he describes as "a more or less vague indication of the laws and initial conditions considered as relevant, and it needs 'filling out' in order to turn into a full-fledged explanation."¹⁶ Imperfect laws, as here defined, need not be vague or, as explicit premises from which deductions can be made, incomplete. For instance, 'All men are mortal,' though universal, is imperfect by our criterion, since from it we can neither explain nor predict a man's death at a particular time. But such nonquantitative universal generalizations, as well as many laws of the biological and social sciences or statistical laws generally, are not necessarily "vague." Nor do they necessarily need "filling out" before they can be used for significant deductions or "full-fledged" explanations.

On the other hand, it is indeed true, as I illustrate later, that we often have to make guesses as to the appropriate imperfect laws, about either

¹⁶ Hempel in Gardiner, p. 351.

individuals or groups of individuals, that will permit us to explain or predict a given event. However, the deductive model by no means requires that premises be the deterministic process laws of perfect knowledge. Once this is grasped, the admitted difficulty in formulating so-called universal laws, of which the critics make so much, no longer appears insuperable. They set a demand that is not logically required by the model. After all, deductive inference was with us centuries before Newton formulated the first process laws.

"The criterion of deduction must be abandoned if the criterion of universal (non-statistical) hypotheses is abandoned." Moreover, if we only have statistical knowledge, then though we cannot predict, we can use such knowledge to explain with "certainty" the occurrence of individual events.¹⁷ Neither of these claims, as I have just suggested, is justified. A statistical law asserts that if each member of a certain class has a certain attribute, then a certain fraction or percentage of them will have another attribute. For instance, "60 per cent of all cigarette smokers develop lung cancer." This is a generalization or universal statement, for it says of all cigarette smokers, past, present, and future, those observed and those as yet unobserved, that 60 per cent of this group will suffer from cancer of the lungs.¹⁸ Like all statistical generalizations, the evidence for it is a finite number of cases. The statement asserts, however, that in the class of all individuals of a certain kind, a particular attribute will turn up with a specified frequency. In this respect, a statistical generalization is as "universal" as a so-called deterministic or nonstatistical law stating that each and every individual having a certain character will also have another. In both cases, the law goes far beyond the evidence. If it did not, but was just a summary of observations, it would have neither explanatory nor predictive power. The difference between them is not that one permits prediction while the other does not, but in the nature of what can be predicted or, what amounts to the same, in how they are tested.

From a deterministic law, given the initial conditions, we can predict an individual event. From a statistical law and its initial conditions (the

¹⁷ Scriven, in Gardiner, pp. 457, 464; and in *Science*, 130:479, 480.

¹⁸ For the sake of an illustration, I have stated the hypothesis in this very alarming way, just as one might say that 60 per cent of all Norwegians are blond. Realistically, the actual lung-cancer hypothesis is a bit less terrifying, asserting a comparison between smokers and nonsmokers and that a higher percentage of the former than the latter will develop lung cancer. The relevant logic of the situation is identical in either case. See the examples that follow.

occurrence of a large group of cigarette smokers), we can predict only a so-called mass event, that is, the frequency with which an attribute will be distributed in the given class. If an unbiased coin is tossed a large number of times, then the frequency with which heads will turn up is 50 per cent. This says something about the class of *all tosses* of a coin, though it says nothing about what will happen in any particular toss. Similarly, the lung-cancer generalization says nothing about any particular cigarette smoker, though it says a good deal about the class of all cigarette smokers. From a statistical law, then, nothing can be predicted about an individual event. On the other hand, neither can we explain an individual event by reference to such a law.

It is embarrassing to rehearse these elementary matters. But the critics force such rehearsal upon one, for they argue to the contrary. Consider the nonquantified, implicitly statistical statement that Scriven calls a “hypothetical probability prediction,” namely, that if a flood occurs, then animals who can swim will be more likely to survive than those who cannot.¹⁹ Scriven notes that unless we can predict the flood, we cannot predict which animals will survive. However, he believes that, in retrospect, “we can *explain why* certain animals and plants have survived even when we could not have *predicted that* they would.” In fact, given such a law, even if we could have predicted the flood, we could not have predicted anything about individual animals. We could have predicted only that more animals who can swim would survive than those who could not, that is, an implicit frequency distribution. By the same token, we cannot explain why a particular animal happened to survive. For, as Scriven says, there may be many other unknown factors besides swimming ability that contribute to survival. Our knowledge is incomplete. That, indeed, is why we can only state a “hypothetical probability prediction” or statistical law. In other words, though we can explain and, moreover, explain deductively why more fishes than chipmunks survive, we *cannot* explain why a particular fish survived. Since many do not, the “explanation” in terms of swimming alone is clearly inconclusive, far from “certain,” no matter how plausible it may appear.

From statistical generalizations, we do not deduce “with probability” that a certain event will occur, rather we deduce exactly the relative frequency or “probability” with which an event will occur in a certain group.

¹⁹ In *Science*, 130: 478.

Similarly, contrary to what Scriven maintains, both statistical and deterministic laws are falsified if the prediction fails.²⁰ The difference is only in the falsifying event. In the statistical case, the failure, not of an individual event, but of an attribute to occur with a certain frequency in a “mass event” falsifies the law. If our generalization is not quantified and says merely that one event is “more likely” than another, then if in other large samples that event does not turn up more times, the law is falsified. If of a specified large group of cigarette smokers, satisfying certain conditions, only 50 per cent develop lung cancer before their death, then the “law” has been falsified. The use of statistical hypotheses does not, therefore, require abandoning deduction. Quite the contrary. Just as in the deterministic case, without deduction we could neither test statistical hypotheses nor, for that matter, have any rational grounds for, say, recommending a decrease in cigarette smoking or, to change the example, to inoculate our children against poliomyelitis. No doubt we would much prefer to know that each and every child receiving a certain vaccine is immune to the disease, or the exact conditions under which cancer develops in a particular person. Yet, statistical knowledge is not to be scorned though it is imperfect rather than perfect. It is far from valueless to know the factors statistically correlated to the frequency of occurrence of an event. And it is the exact deductions from such knowledge that make it valuable.

Scriven’s claim that statistical premises nondeductively permit us to explain individual events rests on no firmer foundation than converting a specious plausibility into a “certainty.” But he also maintains that we can explain individual events without recourse to laws at all. In particular, an appeal to laws is held to be “wholly unnecessary” for identifying the causes of an event. Let us see.

Explanation by ‘Causes’ vs. Explanation by Laws

“We can explain but not predict whenever we have a proposition of the form ‘The only cause of X is A,’ for example, ‘The only cause of

²⁰ Just as deterministic laws cannot be conclusively *verified*, statistical laws, by their nature, cannot be conclusively *falsified*. Accepting an observed frequency in a sample as the true probability requires an induction that this frequency will persist for indefinitely large samples; this may not be correct. Lack of conclusiveness, however, does not mean that we may not have good evidence either for accepting a deterministic law or for rejecting a statistical one. Conclusiveness, again, is not required of the premises but only of the connection between premises and conclusion.

parisis syphilis.’”²¹ Given A, we cannot predict X, for only a few cases of A develop X. Only A in conjunction with certain unknown conditions is followed by X. Therefore, Scriven maintains, given a case of A, “on the evidence” we must predict that X will not occur. He is mistaken. “On the evidence” we are in no position to predict any such thing. No such prediction is *logically* justified, no matter how soothing or useful it may be, any more than we can predict of any particular cigarette smoker that he will or will not get cancer. To make his prediction, Scriven explicitly uses the premise that only a few A’s develop X. Now, if we make the decision, as is customary when action is necessary, always to assume that the statistically more likely case will occur, then of course we can predict that X will not occur. But, then, having made that assumption, the prediction of the nonoccurrence of X follows *deductively* from these premises. True, we cannot predict, under the hypothesis given, that X will occur, but when it does we can explain it. However, as we shall see, the deductive model can account for this practical asymmetry.

The sentence “The only cause of X is A” needs considerable unpacking. Scriven, believing that “cause” is an unanalyzable concept that everyone just naturally understands, denies any need for unpacking. He can therefore maintain that such sentences present instances where we can “explain what we could not have predicted, even if we had had the information about the antecedent conditions.” Or, as he goes on to say, “sometimes the kind of correlation we need for prediction is absent, but a causal relationship can be identified.”²² The only way Scriven persuades himself that he can explain an event that could not even in principle be predicted is by leaving “causal” statements wholly unanalyzed. Despite the confident use of the causal idiom in everyday speech, we may still significantly ask under what conditions statements like “C is a cause of E” are true or false. I shall not take the time here to exhibit the problematic nature of the notion of “cause.” Nor do I believe that to most this needs exhibiting. How then must the statement be unpacked?

To say “The only cause of X is A” is at least to affirm the law that X never occurs without A. In other words, A is a necessary condition for X, or “Whenever we have X, then we also have A.” It is also, however, to say *more*, namely, that there is a complex of conditions, of which A is always one, under which X occurs; that is, certain other factors, b, c, d,

²¹ Scriven, in *Science*, 130: 480.

²² *Ibid.*

and A are sufficient for X. In other words, A is a necessary condition and also one of several jointly sufficient conditions. This indeed is a situation in which we speak of one event as the “cause” of another. As is obvious, this causal imputation is far from being independent of any laws, known or surmised. By hypothesis, we do not know the other sufficient conditions. If we knew them, then deductive explanation *and* prediction would follow directly from a statement of the necessary and sufficient conditions. However, since we do not know the sufficient conditions, how do we account for the fact that actually we would normally explain X by A? Our only justification, and in fact the only way anyone, including Scriven, does justify doing this, is by implicitly adding to our knowledge of the necessary-condition law, our “guess” about the sufficient-condition law. Knowing that both X and A have occurred, we assume the presence of the unspecifiable b, c, and d. The explanation of X then follows deductively. That is *why* we accept A as an explanation of X. In order to predict X from A, the unknown factors must also be specified and this we cannot do. The asymmetry exists in practice, but not in principle. Nor is this an “unhelpful” sense of ‘in principle.’ For only by exhibiting the form of the argument that would, if we knew b, c, and d, permit the prediction, can we clarify why the purported explanation really does state “why” X occurred.

This explanation implicitly assumed that certain unspecifiable events had occurred. Even more frequently, perhaps, the implicit premise is that certain easily specifiable events did *not* occur. We explain a man’s death as due to his being struck by an automobile. This is not because we grasp the meaning of the term “cause,”²³ but because we know a law to the effect that if anyone is struck by a car, then he will be either killed or badly hurt. This is imperfect because, among other things, it does not tell us *which* alternative will occur. We therefore cannot predict the death from the law and the fact that he was hit by a car. But knowing it and, therefore, knowing also that the second alternative (or whatever others there may be) did not occur, the death follows deductively from the law, the initial conditions, and the denial of the alternative. The explanation of the event is conclusive because, given the explicit and *implicit* premises, it must have occurred. Clearly, if we knew the implicit premise before the event, it could be predicted. In very many cases where we know

²³ *Ibid.*

such “disjunctive” laws, we can in fact also eliminate all but one of the alternatives.

Explanation and Imperfect Knowledge of History and Society

Consider now the perhaps more radical situation, where an event in the past, for which there is no question of predicting, is to be explained. Every day we give such *post hoc* explanations of unpredicted events. The issue is not whether or not we do so, but whether or not the deductive model can account for these explanations. We have already seen some cases where it can. But let us examine events more properly historical and social. Take an easy case first. The unpredicted Lisbon earthquake occurred. We explain the consequent misery and wreckage by reference to it.²⁴ Of course we do. Furthermore, we do so deductively. From the law ‘Earthquakes lead to misery and wreckage’ and the *now* known initial condition, namely, the earthquake, there is no problem about explaining the misery and wreckage. The explanation of the event after the fact is clearly deductive, by means of a law, not without it. It is no miracle or accidental conjunction of events that misery accompanied the Lisbon earthquake; we expect it to do so in all earthquakes. Only because we know that earthquakes lead to destruction, floods, and so on, which in turn result in human misery, does the earthquake explain the misery. Otherwise, we might as well say that the Lisbon earthquake was just an incidental accompaniment of the misery. Without the law, the explanation is inconclusive; maybe we can sometimes have earthquakes in heavily populated areas without misery and wreckage. Only because of the deductive connection can we say it *must* have been so. Thus only the deductive connection explains the event. But we did not and cannot predict either the earthquake or, *ipso facto*, the misery. Yet, surely an effort of will is needed to doubt that, even though our recent record is not markedly more notable than at the time of Lisbon, we might someday know enough geology to predict earthquakes. Our knowledge in such matters is imperfect: we know that areas of a certain geological structure can expect earthquakes, but we cannot predict exactly when they will happen. The *ex post facto* explanation is nevertheless deductive. The principle of the deductive model has not been confuted. But what of historical

²⁴ Scriven, in Gardiner, p. 468.

events that do not have physical causes? The situation is admittedly more difficult to reconstruct, but not on that account wholly resistant to deductive explanation.

Suppose that we wish to predict the Supreme Court’s decision on a school integration case that is before it. How do we evaluate two different predictions about the outcome? We consider the reasons that, if pressed, each predictor will give. If no reasons can be given or if the reasons, when made fully explicit, do not appear to be true or, if true, do not entail the predicted decision, then, with equal justification, the opposite may as well have been predicted. What kind of statements could serve to *justify* a prediction? We might well cite approximate statistical laws about certain social variables, for instance, about the equalizing effect of war and industrialization. References to our so-called national character implicitly involve other such laws about how Americans have behaved in the past and can be expected to behave in the future. Statistical laws about individual behavior, for instance, of the judges, would also be employed. Knowledge of this kind, in conjunction with what we know about the present situation, serves as the basis for “educated guesses.” The best educated guess is one that *must* be true, if certain premises are accepted. We make this guess rather than another because it is logically entailed by our various bits of “imperfect” knowledge. It is conjectural because the premises are conjectural, but the connection between them and the event predicted is deductive.

After the decision has been handed down, we explain it in the same way. Of course, if by ‘prediction’ we mean any prophecy, or simply “a claim that at a certain time an event will occur,”²⁵ then we may certainly predict without being able to explain. On the other hand, if by a prediction we mean one for which reasons can be given, then after the event we should also be able to explain it. If we cannot, then we know that our premises must be false. Since after the event we know what conclusion we want to draw, it is all too easy to invent the appropriate reasons as premises from which the court’s decision, say, follows. Only successful prediction gives us evidence for accepting the premises. “Prediction” need not be of an event in the future, only of something not as yet known.

In history, too, we use all kinds of imperfect knowledge about societies, institutions, and individuals to make inferences not about the future but

²⁵ Scriven, in *Science*, 130: 480.

about what we do not yet know in the past. We then look for the evidence in the usual historiographical or even archeological sources. It is true that having predicted a current political event, we can wait and see what happens. However, in history, after all, we are also given plenty of well-confirmed facts. We are frequently much more concerned to explain than to establish them. So the principle is not really different. There is no such thing as “historical” explanation, only the explanation of historical events. To be sure, such explanations, as well as those proposed for contemporary events and for complex human behavior, are all in the same boat. History, whether contemporary or past, far from being the paradigm of sound explanation that some now take it to be, must make do with conspicuously imperfect knowledge. I shall make a few more remarks about explanation in history later on. But, as we saw, deductive explanation and prediction do not require that our premises be “perfect.” The critics make the philosophical claim that inference from one fact to another does not require the use of laws. Accurately as this may describe our enthymematic speech habits, the claim is not logically justified by the fact that our knowledge is, in many areas, imperfect and, for all we know, likely to remain so.

Inference Tickets and Enthymemes

Once we have distinguished between the elliptical, context-bound use of language for the purposes of communication and the use of language for description, it is clear, I believe, that insofar as an individual fact justifies the inference to one or more other such facts, laws are always implicitly or explicitly invoked. Otherwise, the justification of the inference remains a mystery. Since those who now assail the deductive model deny that the premises logically entail their conclusions, we may well ask what they believe does justify the inference? The answer lies in the use, in one form or another, of Ryle’s notion of an “inference ticket.” Attending to those idiomatic expressions that signal the use of argument, like “because so and so, therefore,” “this, so that,” “this involves that,” as well as “if, then,” Ryle concludes that individual facts are deduced from premises stating other individual facts. Ryle accepts the standard interpretation of natural laws as hypothetical or conditional statements. He denies, however, that laws state facts. “Butter melts when heated” or “Tempered steel is flexible” are, on his account, not factual assertions. This, if I may say so, is an eminently “philosophical” rather than commonsensical use

of ‘fact.’ Philosophical uses are puzzling and must be explicated. Ryle does indeed provide an explication, one which, as I shall try to show, carries with it an untenable implicit metaphysics. The difference between singular statements (not containing dispositional predicates) and hypotheticals, according to Ryle, is in the jobs they perform. The job of singular statements is to report “facts,” that is, actual happenings or occurrences. Hypotheticals, on the other hand, “narrate no incidents.”²⁶ Their job is to serve as inference tickets, licensing inferences from one or more individual facts to others. The general hypothetical “warrants” the argument “from factual statement to factual statement,” but is not itself part of that argument. Judged by the logicians’ account, Ryle’s is clearly enthymematic. The “inference ticket” licensing the deduction is, of course, not the general hypothetical which is instead an implicit premise, but the rule of *modus ponens*.

The inference-ticket notion plays a large role in the arguments of those who now attack deductive explanation. The universal statement is said to serve as a “warrant” or “justifying ground” making “legitimate” or “guaranteeing” the step from fact to fact. Though Scriven’s boundless confidence in “judgment” and “primeval certainty” leads him to deny that “we should lose faith in an explanation” if we cannot formulate justifying grounds, he concedes that sometimes universal premises are used to “justify” an explanation. Since these warrants, justifying grounds, or inference tickets are allegedly not part of the explanation itself, explanation from individual fact to individual fact is not deductive. It is indeed held that no distinction can be drawn between deductive and inductive arguments, that the logician’s formally deductive arguments are really inductive or “substantial.” For instance, the astronomer can predict a future eclipse from the “standard equations of stellar dynamics” and the present position and motion of the heavenly bodies involved. Yet, this prediction is said to be “substantial,” not tautologically deductive, because the prediction allegedly is made from the present and past positions of the heavenly bodies. These latter—the data and “backing” for the standard equations—“do not positively entail the conclusion.”²⁷ Of course they don’t, but no one ever said they did. Patently, the evidence we have for a uni-

²⁶ Ryle, *The Concept of Mind*, pp. 120, 125.

²⁷ Toulmin, *Philosophy of Science*, pp. 84–85, 93ff, and *The Uses of Argument*, pp. 101, 114, 121–122, 220; Scriven, in Gardiner, pp. 446ff, 456ff; Ryle, *The Concept of Mind*, pp. 120–125.

versal statement—the standard equations—is here confused with what that statement itself actually asserts. The conclusion follows from the equations themselves and the appropriate initial conditions. Either the equations—the so-called warrant or justifying grounds—and the initial conditions jointly entail the conclusion deductively or they can neither predict nor explain it.

The suggestion to “construe” natural laws as material rules of inference is not a new one. It has been made from time to time by various philosophers as a way out of certain philosophical problems regarding universal propositions. Various difficulties with this view, apart from the fact that this is not their actual role in scientific practice, have also been pointed out.²⁸ The philosophical nerve of the matter, however, is plucked by Braithwaite’s comment that to treat a logically contingent statement as a principle of inference is to “mix experience and the logical methods by which we think about experience in a very confusing way.”²⁹ Why this is confusing should be, but apparently is not, obvious. One reason it is not obvious is the promiscuous use of ‘logical’ for quite different things, while at the same time retaining the connotations of the term in its strict sense. Statements about the past, for instance, are said to be “logically” different from statements about the future, and *therefore* one cannot be deduced from the other.³⁰ This is but one instance of an appeal to “logic” as if that were what it is not, namely, a logical reason in the strict sense.

Another instance is the use of “logic” to mean function or job. Both contingent hypotheticals and logical truths are used as “inference tickets”; both therefore have the same jobs or “logic.” Calling two things that may have the same function in communication “logically” the same at best blurs the issue about the structural or logical, in the strict sense, difference between analytic and synthetic statements. This difference is signaled by at least some uses of ‘necessary’ in common speech. These, in turn, may reflect differences in the descriptive content of each kind of statement, no matter what their “job.” The issue appears to be settled in the negative

²⁸ See E. Nagel, “A Perspective on Theoretical Physics,” *Mind*, 63: 403–412 (1954), reprinted in his *Logic without Metaphysics* (Glencoe, Ill.: The Free Press, 1956); and H. G. Alexander, “General Statements as Rules of Inference,” in *Minnesota Studies in the Philosophy of Science*, Vol. II.

²⁹ R. B. Braithwaite, *Scientific Explanation* (Cambridge: Cambridge University Press, 1953), p. 86.

³⁰ Toulmin, *The Uses of Argument*, pp. 13, 220.

when in fact it has not even been raised. But there is an even more fundamental reason for this confusion of experience with reasoning about it, to which the others are mere corollaries.

In ordinary communication, if a person asserting “this, so that” can produce a corresponding general hypothetical, we say that he has “the right” to conclude that from this.³¹ But suppose we go on to ask what there is about the world or the way we reason about it which justifies our saying this. That is, why does the general hypothetical give us the right to infer that from this? I mentioned before that an appeal to logical truth or to definition is conclusive in a way that appeal to contingent truth is not. If we accept the logician’s notion of logical truth, then we can answer this question. *Modus ponens* licenses the inference and that, in turn, is justified by appeal to the notion of logical truth or tautology. Ryle rejects this answer in part at least because he rejects the question. That is, he holds that an appeal to a statement of the form ‘If P then Q’ is as conclusive as an appeal to a statement of the form ‘If ‘If P then Q’ and ‘P,’ then ‘Q.’ At one place, Ryle rejects the logician’s reconstruction of the “p, so q” argument as enthymematic on the Tortoise and Achilles grounds that the use of a rule as a premise leads to an infinite regress.³²

But Ryle’s use of this argument is mistaken. Since they are metalinguistic, rules are indeed of a different logical type from premises. But the justification of, say, *modus ponens* does not involve the use of its corresponding hypothetical as a premise, or, therefore, the use of another rule, and so on ad infinitum. Rather, the use of the rule is justified by pointing out that the hypothetical sentence corresponding to it—which is *not* a premise of the argument for which it is used—is a tautology. All that the Tortoise proved to Achilles is that some (at least one) rules are always necessary. He did not prove that these indispensable rules could not themselves be justified. This justification is indeed a fundamental task of the philosophy of logic, as contrasted with logic itself. Ryle rejects this task and holds instead that one cannot even ask what justifies the rule because the job of the hypothetical is to be a rule. It “means” a rule, and the appeal to “meaning” is, everyone agrees, conclusive. With the appeal to “meaning,” we come to the heart of the matter, the confusions about “conceptual analysis.”

³¹ Ryle, *The Concept of Mind*, p. 300.

³² Cf. G. Ryle, “‘If,’ ‘So’ and ‘Because,’” in *Philosophical Analysis*, Max Black, ed. (Ithaca, N.Y.: Cornell University Press, 1950).

“Conceptual Analysis” and Rejection of Hypothetico-Deductive Theories

The critics of deductive explanation quite consistently also attack the hypothetico-deductive model of scientific theories, according to which a theory is a deductively connected set of general statements, some of which, the premises or axioms, logically imply others, the theorems. These statements of a theory are hypothetical in a double sense. First, as empirical, contingent, or logically synthetic general statements, they may be falsified; second, some of them are used as premises in a deductive argument—the theory—whose purpose is to show what else must be true if the premises are true. It is worth mentioning that neither of these senses of “hypothetical” has anything to do with the process by which universal hypotheses are formulated, whether it be by induction, a hunch, or a dream. They are “generalizations” only in the sense that they are of generalized, that is, universal form. Some believe that if laws are not arrived at by a process of generalization from observed instances, then they cannot be contingent universal or general statements.³³ This is certainly consonant with the typical process-product confusion, but otherwise has less than nothing to recommend it. But the matter goes considerably deeper than this apparently merely verbal matter. For the critics deny that scientific laws are “hypothetical” in either sense mentioned. They are, of course, held to be rules and not premises in a deduction but, more revealingly, neither are they contingent or subject to refutation by observation. They are rather true by virtue of “meaning,” by virtue of the way the scientist uses the terms connected by laws. A few comments about meaning are first of all in order.

Terms themselves do not mean. We mean by their use. Yet terms, including ‘meaning’ itself, have many different uses. We ask for the meaning of life, wanting to know why we are here, and whither, if anywhere, we are going. We ask for the meaning of an event, like a falling star, wanting to know, perhaps, what it portends. Or we ask for the meaning of another event, like an election, wanting to know what it indicates about the temper of people. We search out the meaning of a drama or a novel, wanting to know what moral it points for man and his world. Or, more mundanely, we ask for the meaning of words, like ‘rabbit’ or ‘acceleration,’ wanting to know no more than to what they refer. In the former, possibly

³³ See Toulmin, *Philosophy of Science*, p. 49; N. R. Hanson, *Patterns of Discovery* (Cambridge: Cambridge University Press, 1958), pp. 107ff.

more intriguing, questions about “meaning,” we are interested in significance in a common-sense use of that term that is generally clear in context. In the last example, however, we are interested in the observable referent of the term. ‘Rabbit’ means (is used to refer to) rabbits; ‘acceleration’ means (is used to refer to) the rate of change of velocity with time. In asking for “meaning” in the sense of significance, we want to know with what *other* things the events or things asked about are connected. Some terms, like ‘cephalic index,’ have meaning in the sense of reference but no significance. We know well enough what the term is used to refer to, but we know nothing about it, that is, there are no laws connecting a person’s cephalic index with any of his behavior.

Other terms have both meaning and significance. We know that rabbits eat lettuce, and we also know, among other things, that the acceleration of free fall is a constant. If we did not know the meaning of a term in the referential sense, then we could not discover its significance. We would not, in fact, know what we were talking about, either what it was that ate lettuce or what it was that is constant. A worthwhile concept has both reference and significance. In the first sense of meaning, terms themselves do not mean. We mean by their use. To put it differently, *referential* meaning is a matter of convention; it is something we give the concept. *Significance* or lawfulness, however, is not a matter of convention, but a factual matter, that is, a matter of the way things are. We can define any concept we wish to define. But we cannot endow a concept with significance. It either has it or it has not.

To blur this distinction is to blur a contribution of the mind—the concepts we use—with what is not such a contribution, but, independently of the way we speak about it, is a matter of the way the world goes. Realism and idealism are the relevant metaphysical tags for those who, respectively, insist upon and those who blur this distinction. In particular, the formula “Meaning is Use” blurs the distinction. More particularly, the doctrine that natural laws are true by virtue of the “meaning” of their constituent terms obliterates it. For the two questions, about what a thing is and about what happens to it, are held to be not two questions but one. The terms, we are told, cannot be identified apart from the laws in which they occur.³⁴ In other words, given a law, “If P then Q,” we cannot know that we have P unless we simultaneously know that “it” has Q. This

³⁴ Toulmin, *Philosophy of Science*, p. 52; Hanson, *Patterns of Discovery*, pp. 61ff.

seems to make Q a definitional property of P. But definitions are tautologies, and it is denied that laws are tautologies. Yet the “meaning” of ‘P’ is ‘Q,’ and denial of the law is “conceptually untenable.”³⁵ Again, all terms are said to be “theory loaded.” Their meaning is given by the entire theory, that is, the context, in which they occur. Since the relevant context varies with each use and each user, no two people ever use a term in the same way. Lightning and thunder are said to mean something different to a youngster than to a meteorologist. A clock means something different to Galileo’s apprentice than to Galileo.³⁶ Accurately stated, the meteorologist knows more about flashes and rumbles than does the boy, and Galileo knew more about clocks than did his apprentice. Otherwise, how would we ever know that they were all talking about the same thing? If a term means something different in every context in which it occurs, then, as has been well pointed out, an exception to or falsification of a law becomes “conceptually impossible” because the term means something different in the law and in the statement of the exception.³⁷ If an exception is “conceptually impossible,” then the statement itself is conceptually “necessary.” Instead of our speaking being determined by the way the world is, what is possible in the world is determined by the way we speak about it.

On the hypothetico-deductive account of theories, they have the form ‘If P then Q’ and ‘If Q then R,’ then ‘If P then R’; the major “if-then” expresses a logical entailment between the premises or hypotheses of the theory and the conclusion. However, if laws are held to be rules for “deducing” one individual fact from others, then what rule justifies deducing laws themselves from other laws? The critics need not answer the question for, on their view, no such deduction occurs. A theory is not a deductively connected set of statements, as indeed, if the terms differ in meaning each time they occur, it cannot be. Not the sentences, but the terms of a theory are said to be “logically linked,” in some yet further Pickwickian sense of ‘logical.’³⁸

The idealistic monists, it will be recalled, rejected classical syllogistic logic, the only logic they knew, because they maintained that terms differ

³⁵ Hanson, *Patterns of Discovery*, p. 115.

³⁶ *Ibid.*, pp. 56–57.

³⁷ In the excellent critical review of Hanson’s book by P. K. Feyerabend, “Patterns of Discovery,” *Philosophical Review*, 69: 247–252 (1960).

³⁸ Toulmin, *Philosophy of Science*, p. 85.

in meaning every time they occur, so no conclusion could “really” be drawn by the methods of that logic. Rejecting the abstract universal, in favor of the so-called concrete universal, they argued that not sentences but concepts were “deductively,” as they said, connected by the expansion of “meanings” into ever broader and more inclusive contexts. So it is here. What I have called the component laws are instead statements about the meanings of the terms. The “Q” in the first premise has a different “meaning” from that in the second because it occurs in a different context, yet they are “logically linked” since one incorporates or expands upon the other. The meaning of a term is thus given by all the statements in which it occurs. These statements are alternatively either rules for the use of the term or “linguistic,” “conceptual,” or even “logical” truths about it. True by “meaning,” they are yet synthetic or, at least, not analytic. Though not tautologies, they are nevertheless the last court of appeal. Used as rules, they need no further justification. That is why hypothetical or conditional statements are “nonfactual” and only singular statements are empirical statements of fact. All generalizations, once they have been accepted and passed into usage, become true by meaning. It then becomes “conceptually impossible” to deny an empirical law. The bridge from ‘conceptual’ to ‘conceivable’ is as short as that from ‘conceptual’ to ‘logical.’ The psychologically inconceivable thus becomes the “logically” contradictory, their negations, “logically” true.

Borrowing next on the conclusiveness of the appeal to logical truth in the strict sense, the appeal to these nonanalytic “conceptual truths” deviously appears equally conclusive. That is why one can say that the rule “licenses” the inference, but one cannot ask what licenses the “license.” It is also the basic reason for the rejection of formal logic, whose terms in an argument must remain constant in meaning and which distinguishes generalized statements that are logically true from those that are factually true. Denying that deduction in the formal sense ever occurs, naturally there can be no deductive explanation or prediction. If the “meaning” of a concept is always another concept, then the job of statements to describe the world, their connection with something *nonconceptual*, becomes inexplicable. Confusion of sociopsychological description of language as communication with structural analyses of what this communication asserts about the world leads, not for the first time, to a philosophy that loses the world in a system of “meanings.” This untenable conse-

quence is, we shall see, further supported by analysis of the more moderate criticism of the deductive model.³⁹

Criticism of the Premises of the Deductive Model

The more moderate criticism of the deductive model agrees that unless an explanation logically entails, in the strict sense, what it explains, then "it will fail to explain why what it purports to explain should have happened rather than something else."⁴⁰ It denies, however, that any of the premises need be a universal law. The appearance of paradox, if strict deduction is not abandoned, dissipates when we learn that a general premise is indeed required, though this premise is not granted the status of a natural law. Nor, as I shall try to show by examining the position as presented in a carefully argued paper by Donagan,⁴¹ is the disagreement merely a matter of words. Since Donagan's major criticism as well as certain associated doctrines derive from some of Ryle's ideas that have already been discussed, it will be possible to be rather brief. Donagan's argument is especially worth examining because, free both of the tedium of "arguments" that amount to little more than puns from usage and of obscurantist appeals to "judgment" and "intelligibility," it points up even more sharply the structural connection between the various doctrines and the virtually universal hostility of their proponents toward a science of behavior.

Donagan's argument against the deductive model allegedly depends upon Ryle's philosophical behaviorism. Our common speech is studied with expressions which, if taken in their "ordinary sense," as Moore would say, are used to refer to mental states, to such things as conscious feelings, thoughts, desires, beliefs, and sensations. But ordinary language is fickle and here deceives us for, according to Ryle, mental states are but "mythical" entities, products of the metaphysicians' fancy. Accordingly, he "construes" statements about mental states as general hypotheticals or dispositional statements about how people behave under certain circumstances. These dispositional statements license us, in the now familiar manner, when confronted with the circumstances stated in the

³⁹ For an analysis of the metaphysical uses or misuses of this notion of "conceptual truth," see G. Bergmann, "Strawson's Ontology," *Journal of Philosophy*, 57:601-622 (1960).

⁴⁰ A. Donagan, "Explanation in History," *Mind*, 66:145-164 (1957); reprinted in Gardiner, pp. 428-443.

⁴¹ *Ibid.*

protasis, to predict that the behavior mentioned in the apodosis will occur. Or, conversely, we are licensed to explain the behavior by reference to the protasis conditions. Donagan does not adopt the inference-ticket view. The license for him, as for the logician, lies in the entailment relation, not in the general hypothetical itself. The latter therefore must occur as a premise of the argument. The use of such hypotheticals or dispositional statements as premises in deductive explanation does not depend, however, as Donagan believes, upon the truth of Ryle's philosophy of mind. Methodological behaviorism as a program in psychology and, more generally, the scientific study of man is one thing. There it is proper and essential.

Philosophical behaviorism or materialism, the denial that there are such things as mental states, is something else again and is not implied by methodological behaviorism. Drawing his philosophical conclusions about what exists from the study of language as a vehicle of communication rather than description, Ryle finds ordinary language a poor guide to ontology. Identifying how we can tell with what there is, he revamps the old verifiability theory of meaning. The meaning of mental terms in common speech becomes the behaviors that, as we ordinarily say, testify to them. I suggest that to argue from how we communicate with each other about our own and other people's states of mind to what exists is exactly analogous to the error of those verificationists who "construed" statements about the past to be "really" about the present and future, because it was by means of such observable consequences that they were verified. The ontological price for ignoring the difference between the communicative and descriptive uses of language is high indeed. Be that as it may, we are here concerned with the possibility of scientific, deductive explanation and not with the philosophy of mind. We need therefore only inquire whether the differences Ryle discerns between explanations by so-called lawlike dispositions and explanation by laws are indeed real differences. For two things are made to depend upon them. First, only explanations by means of laws are held to be "causal" explanations. Second, explanation by means of, allegedly noncausal, "lawlike" motive or dispositional statements is held to be final or conclusive, requiring no further explanation. If true, these claims support the contention that explanation in history and the other studies of man and society is fundamentally different from that in natural science. Since the proponents of the deductive

model generally deny that this is a difference of principle, the issue is more than merely verbal.

“Lawlike” Hypotheticals vs. Causal Laws

‘Jones is vain’ is construed by Ryle as ‘Whenever Jones finds a chance of securing the admiration and envy of others, he does whatever he thinks will produce this admiration and envy.’⁴² The “mentalistic” terms in this hypothetical—‘admire,’ ‘envy,’ and ‘thinks’—doubtless could be replaced by behavioristic, dispositional correlates. Such statements are held to be lawlike rather than laws, because, though general with respect to time, they mention individual persons or things. It is worth noting, however, that some uses of dispositional terms are not general at all. Behavioristically defined, both ‘hungry’ and ‘irascible’ are dispositional. ‘Hungry,’ referring to a present state of a person, would be defined by a molecular, that is, nongeneral, hypothetical. ‘Jones is hungry’ means, say, ‘If Jones is now presented with food, then he will eat it’; or, if I may be permitted the greater clarity of a symbolism, ‘If $F_1(a, t_1)$ then $F_2(a, t_1)$.’ This is a non-generalized molecular statement. “Hunger” is thus a complex property being attributed to a specific individual at a specific time, just as “being red and hot” would be a complex property attributed to a poker. ‘Irascible,’ on the other hand, referring not to a present state of a person but to his disposition to behave in certain ways under certain circumstances, has the form ‘(t) If $F_1(a, t)$ then $F_2(a, t)$.’ It is a quantified or generalized statement. All terms referring to personality or character traits, rather than to present states, would thus be generalized over one or more variables, whether they be time, objects, or circumstances.

Just as these hypotheticals about persons always have some degree of generality, so natural laws are never completely general. Galileo’s laws hold only for bodies on Earth; we have laws about the expansion of the chemical mercury and Kepler’s laws for the planets; the law of radioactive decay contains a constant whose value depends on the substance involved; and so on. All known laws have some kind of scope restriction which is part of the law itself. Usage certainly does not insist upon complete generality for what is to “count” as a natural law among scientists. The logically significant distinction is generalized versus molecular rather than “law” versus “lawlike.” General hypotheticals about a person are of the

⁴² Ryle, *The Concept of Mind*, p. 89.

same logical form, in the strict sense, as natural laws, while both are to be distinguished from statements of individual fact or any molecular compound of them. The issue might appear to be merely verbal or at best a classificatory problem in a case where the boundaries are blurred, if so much were not made to hang on it.

Donagan acquiesces in the standard Humean account of causality, as expressed by Hempel’s assertion that “Every ‘causal explanation’ is an ‘explanation by scientific laws’; for in no way other than by reference to empirical laws can the assertion of a causal connection between certain events be scientifically substantiated.”⁴³ Donagan distinguishes such causal explanation from explanation by dispositions, maintaining that this distinction renders historical explanation noncausal. Ryle, we recall, holds that “This because that” or “The window broke because struck by a stone” is itself a complete, nonenthymematic explanation. “This window broke because brittle” he also holds to be a complete explanation. The former is “causal” because it mentions a “significant prior or simultaneous happening,” namely, being stoned. The latter, however, is not causal, because “being brittle” is not a happening or episode. Being brittle is, we also recall, not even a fact, as Ryle uses that term, about the window, since it is expressed by the general hypothetical, “If this window is struck sharply, then it will break.” Donagan maintains that though causal explanation is “Hempelian,” that is, fits the deductive model, the dispositional one “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.’”

But the claim that one fits the deductive model while the other does not will not bear closer examination. Nor will the further claim about the only way to derive the statement that certain windows broke. Ryle’s philosophical behaviorism and his rejection of formal deduction here reinforce each other. Ryle, one might venture to surmise, insists that mention of the “nonfactual” disposition is a complete explanation because of his even greater concern to deny that there are any events or happenings corresponding to mental terms which could serve as causes of motivated events. If, for instance, “vanity” is behavioristically construed as a personality disposition term, then to say “He boasted from vanity” is not to explain his

⁴³ Quoted by Donagan, in Gardiner, p. 430.

boasting by reference to any "cause" or "happening."⁴⁴ The "happening" Ryle is concerned to deny is of course any *mental* happening. The denial of philosophical behaviorism, while accepting methodological behaviorism and the general scientific thesis of psychophysical parallelism, does of course not at all commit one to the belief in mind-body interactionism. What matters, however, is that Ryle, given his quaint views about the subject matter of psychology, *thinks* one is thus committed, so being vain, or in love, or brittle cannot be "facts" or "happenings."

Actually, spelling out these enthymemes, it becomes clear that *both* of them, "Broke because stoned" and "Broke because brittle," mention "significant prior or simultaneous happenings." In the first case, the implicit premise or "inference ticket" is the general law "All windows break when stoned," while the happening, being stoned, is explicitly asserted. In the second case, the lawlike hypothetical defining the dispositional predicate 'brittle' is explicitly asserted, while the implicit premise is the individual statement of fact asserting an instance, namely, that the window was struck sharply, of the protasis of this hypothetical. In all logic, Ryle should permit singular statements of fact to be "inference tickets," for that is their job in such "explanations." Ryle feels no discomfort about this "episode proposition" that, he agrees, is implicitly subsumed under the general hypothetical presumably because it is only the antecedent part of the defined motive or character term and the motive itself, the entire hypothetical, is not made a "happening." Another reason why Ryle holds hypotheticals to be "nonfactual" is that when we explain by reference to a disposition rather than by mentioning a specific event, we say we are giving a reason rather than a cause.⁴⁵ I think he is right about this use of 'reason,' but only because when we explain by reference to a disposition, we are, by definition of the disposition, implicitly giving the premises of a deductive argument. And giving premises that logically entail a specific event is certainly one good use of 'giving reasons' for that event.

Spelling out Ryle's enthymematic arguments reveals not only that individual happenings are involved in each case but also the inaccuracy of Donagan's contention that the only way for the Hempelian model to derive the breaking of the window from its being stoned is by the buried law 'All windows break when stoned.' It can of course also be derived from the "lawlike" disposition 'If this window is hit by a stone, then it

will break.' Therefore, anyone who rejects the inference-ticket notion and opts for *deductive* explanation cannot also accept "being brittle" as a *complete* explanation. He has to grant that there is an implicit singular premise. To reject the deductive model, he must therefore fall back on insisting that the difference between so-called causal and noncausal general hypotheticals is more than one of degree of generality. And, indeed, Donagan does give reasons for claiming that there is an *essential* difference between dispositional statements about material objects and those about persons which serve as premises in nonphysical explanation. This alleged difference is believed to be relevant to the deterministic thesis and the possibility of a science of man. For this reason, as well as for what it reveals about "conceptual analysis," the argument is of special interest.

Motives, Meaning, and a Science of Man

The lawlike statement 'If this window is struck sharply, then it will break' is derivable by specialization from the general law about the brittleness of windows, 'If any window is struck sharply, then it will break,' which is generalized not only with respect to time but also with respect to windows. Lawlike statements about a person's motives or character are, however, allegedly not similarly "generalizable." This limitation, in turn, is believed to justify the claim that explanations in terms of motives or character are not only "complete," but also "final" or conclusive. That is, though like Kepler's laws they completely entail what they are supposed to explain, unlike Kepler's laws they do not themselves require further explaining.⁴⁶ But, first of all, this notion of "generalizability" is ambiguous. To be sure, we neither know nor expect to know any general statement "All men are vain" from which Jones's vanity could be deduced. What is true of Jones is, in this sense, not generalizable to all men. Does it follow that there is no sense in which lawlike statements about individuals are generalizable, thus turning "non-Hempelien" into "Hempelien" explanations? Donagan believes that it does follow, for the buried assumption behind the lawlike 'The Danes who sailed south to the Irish Sea were plunderers first and settlers by afterthought' would be, he asserts, 'All men were plunderers first and settlers by afterthought.' But surely this is not the correct "buried assumption." If the Danes being plunderers first and settlers by afterthought implies, as he says, the lawlike statement

⁴⁴ Ryle, *The Concept of Mind*, p. 86.

⁴⁵ *Ibid.*, pp. 113–114, 89.

⁴⁶ Donagan, in Gardiner, p. 434; Ryle, *The Concept of Mind*, pp. 89, 325.

that 'If those Danes had opportunities of sufficient plunder in a territory, they would not settle in it,' does it not also imply the same thing for Italian plunderers? In other words, the correct buried assumption is 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 dispositional term 'plunderer.' From this definition, together with the information that the Danes were plunderers, we could derive some of their behavior. To be sure, the universal premise of this explanation is a verbally necessary truth, since it reflects our definition of the term 'plunderer.'

The explanation of the Danes' behavior thus logically follows from the empirical fact that they were plunderers and the definition of that dispositional term. In the course of deductive explanations, we frequently do utilize definitions as premises. We must do so to derive theorems that contain defined terms from axioms that are expressed in the primitive terms of a scientific theory. In science, these definitions most frequently take the form of generalized or universal conditional statements. Such a defining sentence, like our definition of 'irascible,' may therefore serve as the universal premise required by the deductive model. The "explanation" is then vacuous and circular in a way in which deductive explanations utilizing *contingent* universal premises are not. Literally speaking, then, if the "Hempelian" model requires a contingent universal premise, then this explanation is not Hempelian. But this is at best a trivial objection. The process-product equivocation in the use of the term 'generalization,' which I mentioned earlier, has probably played a role in misleading Donagan. Since his premise, though general in form, is a definition and not a contingent generalization, he believes that it does not fit the "Hempelian" model. But the fundamental point of the deductive model is that no inference, either for prediction or for explanation, can be made from one or more individual statements of fact to other such statements, without the use of a *general* premise. In the most interesting and informative cases, the general premise is a natural law, but it may also be a definition. However, no one holds that deductions from definitions are the only kind we make in the course of an explanation. In addition to the definition we also must know some general laws containing the defined terms. In the case at hand, if we also know some general laws about plunderers, as defined, laws about, say, their religion or literature, then we could derive in

good Hempelian fashion further facts about the Danes' behavior and, of course, about plunderers of any nationality. Donagan, however, is asserting not only what is obvious, that there is no law about all men being plunderers corresponding to all windows being brittle, but rather that there can be *no general statements about plunderers*, as there are about brittle windows. That is why he believes that the premises in historical explanation must be lawlike statements about *the particular people involved*.⁴⁷ In general, explanations by reference to "the character of the agent" are held to be final and conclusive. What are the reasons for this view?

It is perfectly true that in everyday life when we explain a man's actions by reference to his motives, that is all we want to know. We then "understand" why he did what he did. Frequently, that's all we care about. The historian who succeeds in ferreting out an agent's motives has supplied an explanation adequate for his purposes. As we have seen, the reconstruction of such explanations shows them to be deductive, using the dispositional definition of the motive term and individual statements of fact as premises or reasons for the behavior. Other actions are less trivially explained by using *contingent* generalized premises about how people with such motives can be expected to behave in certain circumstances. Generalizations of this sort are, of course, imperfect. Nor is their imperfection due to their limited scope, namely, being about a single individual or group of individuals. They are imperfect because, generally, they are hemmed in by qualifying phrases which render them implicitly statistical. They are also imperfect because they are limited with respect to time. A man's motives may change with his circumstances or with, as we say, "age." While a man was prime minister, his motives were such and such; they changed when he joined the opposition. These circumstances can of course be built into the protasis as part of the initial conditions or as scope restrictions on either the generalizations or the definitions. "Age" is not a true variable, but a cover or suitcase word for all of the many things that happen to a man as he grows older. The *mot* "Character is fate" enshrines the belief that by understanding a man's character we understand what he does and what happens to him. We may sometimes explain a man's motives at a given time by reference to his character, meaning by the latter certain more permanent and less restricted dispo-

⁴⁷ Donagan, in Gardiner, p. 441.

sitions. To know a man's character means knowing many generalizations about him from which, in given circumstances, both the motives he will act from and his other behavior can be predicted or explained.

The historian uses these kinds of imperfect laws in order to explain the decisions made and actions taken. Nor does he necessarily "presuppose" any general laws about all men. He just uses the laws about his particular man. It would be odd, though, if he were to say that another man of similar character in similar circumstances would nevertheless behave very differently. "Character," after all, refers to a certain kind of man, though possibly, but just possibly, there might be only one of that kind. Even so, it is perfectly legitimate to ask why someone is the kind of man he is, why he has the character he has, even though in everyday life and in the study of history we might not be interested in that question. But certainly parents and pedagogues frequently are interested in it. We do try to "build" character. Since we try to do so, we might well want to know what forms it. To find that out, we have to go beyond generalizations about particular persons. Character is not only a cause of behavior, it might also be caused. Though explanations in terms of the character of the agent might satisfy us and historians, they might not quench a broader curiosity. For it, they would not be final and conclusive. Such persons might try to discover the laws of character formation. They might, in other words, try to develop a science of psychology. The fact that most of us are most of the time satisfied with explanation by reference to motives and character does not rule out a deeper search. The quest may well be futile, but there is no logical necessity that it be so. The critics of the deductive model, whether of its premises or its form, express considerable repugnance and opposition to this quest. This opposition is buttressed by arguments that go deeper than merely practical considerations. Once again, as we shall see, confusion is bred by the notion of "conceptual analysis." The fusing of reference and significance turns a matter of contingent fact, namely, that we cannot now explain or fully explain character, into a "necessary" and even "logical" truth.

I have been speaking of "defining" the disposition terms by means of general hypotheticals. This was, of course, a bit disingenuous. For it follows from the context theory of meaning I discussed before that the lawlike hypothetical corresponding to the statement that Jones is vain does not define the character trait but instead "expands its meaning." If we wish to unpack "all that is conveyed" in describing Jones as vain, we must

"produce an infinite series of different hypothetical propositions."⁴⁸ The operative phrase is "all that is conveyed." In everyday life the connotations of a term include both defining and nondefining properties, as long as the latter are fairly widely and firmly believed to be exemplified by the thing in question. This is what lends to everyday speech its so-called open texture. A concept's "meaning" or "criteria of application" expands as we learn more and more about "it." The difficulties with this view I discussed before. Our concepts may be open textured, but the world is not. If language is to be descriptive, it must indicate what there is in the world, no matter how variably we talk about it. On pain of Bergsonian ineffability, a descriptive language cannot duplicate the world's growth and change, including that of language. It need only account for it. By combining defining and nondefining properties, everything that we know about a thing becomes part of its "meaning." For instance, Donagan tells us that the Danes being plunderers implies not only that they would not settle in a territory but also a "host of further law-like statements" about their literature glorifying war rather than farming, about the kind of religion they would have, and so on.⁴⁹ If all these statements do indeed "unfold the meaning" of being a plunderer, then clearly we cannot ask whether one could possibly be a plunderer or, even, a Danish plunderer, and not produce this kind of literature or religion. By virtue of "meaning" nothing else could have happened, just as by virtue of meaning, Jones's vanity implies he would do the various things that, as we say, "express" his vanity.

But is everything a plunderer might do part of the meaning of the term? And if not, where do we draw the line? As we come to know more and more about Danish plunderers, more and more becomes a matter of meaning or "conceptually true." Let us show by example where this leads. Within behavior science, where definitions are necessary if the scientist is to know what he is talking about, the dispositional term 'hungry' is defined so that if a person, any person, is hungry, then when, say, he is presented with food under certain circumstances, he eats it. It is then discovered that hungry people are irritable. This is a fact or, if you prefer, a law about them. And it could not be discovered unless the meaning of 'hunger' was independent of that of 'irritable.' On the present view, however, this irritability becomes a lawlike hypothetical which is part of the expansion of the meaning of 'hungry.' The more such hypotheticals we

⁴⁸ Ryle, *The Concept of Mind*, pp. 44, 86, 113.

⁴⁹ Donagan, in Gardiner, pp. 436-437.

have, the more we add to the meaning of the term, until everything that we say about hunger is true by “meaning.” If there are an infinite number of lawlike statements expressing Jones’s “kind of vanity,” then where do we draw the line between what we mean by his vanity and what happens to be connected with it, like his ambition or his gregariousness? Again, arguing from language as communication rather than description, the world that language is supposed to be about is lost—for terms are tied down not to the world but to an infinite series of other terms or “meanings.” The illusory conclusiveness of the inference ticket is now transferred to the lawlike premise. Since the lawlike premises about an individual’s character and motives are true by meaning, no explanation of them is either necessary or possible. They are therefore “final.” “Conceptual truths” need no further explanation. Like the appeal to logical truth *in the strict sense*, appeal to these conceptual truths about motives and character are “conclusive.” By an a priori argument from “meaning,” an empirical science of human nature is shown to be unnecessary, impossible, or both.

Concluding Remarks

No matter what other disagreements they may have among themselves, those who now attack the deductive model share certain central and basic ideas which can and do lend themselves to a philosophically argued neo-obscurantism. In particular, the two ideas I have stressed, the view of language as communication rather than description and the notion of “conceptual analysis,” explain but do not justify their rejection of the deductive model of explanation and of formal logic. These ideas also structurally explain their rejection of a science of man. From the dismissal of the need for a science of psychology because “we all know well enough” why people behave as they do, to a metaphysically argued case for the impossibility of a science of man,⁵⁰ the pattern is always the same. Learning a language is a social phenomenon. Once one has learned to use the language of everyday living, by participating and sharing in the social process, then one “understands” individual and social concepts. Once one understands the “meaning” of these concepts, then one *already knows* all there is to know about man and social life. To understand the world, we need therefore not look at it, but merely analyze our concepts.

⁵⁰ P. Winch, *The Idea of a Social Science* (London: Routledge and Kegan Paul, 1958).

The Factual Content of Theoretical Concepts

Philosophers of science have more and more united in rejecting the older positivistic judgment that all descriptive words of an ideal language are, or are explicitly definable on the basis of, terms whose referents are phenomenally “given.” It is far from surprising that tendencies toward a phenomenal reductionism should be a serious philosophical pressure within many critical thinkers, for it is indeed difficult to see how the actual “content” of thought (whatever such an expression might mean) could transcend the limits of direct experience. Yet repeated failure to realize such a program increasingly dims the likelihood that scientific or everyday language can be reduced to phenomenal terms alone. To be sure, this might be interpreted as revealing merely the semantic imperfections of existent linguistic practices, but such a gambit is tantamount to abandoning the analytical scalpel for a dogmatic bludgeon, especially since a number of highly competent philosophers have seriously questioned the very possibility of a phenomenal language.

The problems of “meaning” and reductionism come into especially sharp focus in the analysis of scientific theories, for here they find expression in that conceptual framework which we use with maximal clarity. For it is in common-sense object talk, its usage refined and molded by years of pragmatic repercussions, that philosopher and layman alike carry on the business of living. And given this everyday “observation language,”

NOTE: This essay owes its existence to the vantage point erected by the philosophical tradition currently known as “logical empiricism.” This movement has with increasing penetration and acuity spotlighted the epistemic and ontological problems that underlie the use of theoretical concepts, and with the assistance of the modern renaissance in formal logic, has been developing an ever more powerful conceptual frame with which to attack these problems. The basic issues involved have been set forth with particular clarity by Feigl [5] and Hempel [7], while the reader will also profit from the articles by Carnap [4] and Hempel [8] in the earlier volumes of this series. I also wish to acknowledge my indebtedness to the National Science Foundation for the postdoctoral fellowship during whose tenure this essay was written.