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Presuppositions of Propensity Theories of Probability

Introduction

A fully satisfactory account of a propensity theory has yet to be given even by proponents. Much less satisfactory have been the accounts given of propensity theory by its opponents. This paper is aimed to contribute to the debate over propensity theory by drawing attention to its metaphysical presuppositions. Although I do endorse a propensity theory of probability, this paper aims only secondarily to defend propensity theory against attack. Nonetheless a good starting point in my exposition might be an initial appraisal of a number of recent writings, in the hope that creative criticism may aid a more satisfactory characterization of probability construed as propensity.

Propensity theories of probability are relatively new in the field and are not yet widely endorsed. The still fairly widespread antipathy among scientists and philosophers of science to holding metaphysical beliefs militates against acceptance of a theory which is firmly rooted in ontology, and may even hinder understanding it.

The more popular views (frequency theories, logical theories, subjective theories of probability) are consistent with a positivist philosophy which emphasizes what may be experienced or observed more or less directly. Not so propensity theories. Frequency theories of probability focus upon the empirical outcomes of trials on chance setups. Logical theories focus upon the support some statements afford others. Subjective theories appear to presuppose that the root of probabilities lies in human ignorance — as Joseph Butler put it: "Nothing which is the possible object of knowledge . . . can be probable to an infinite intelligence; since it cannot but be discerned absolutely as it is in itself —

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certainly true, or certainly false. But for us, probability is the very guide of life" (1736, introduction).

By contrast with these theories, propensity theories dig deeply below the phenomenal surface, resembling in this respect what Bunge calls representative theories (1964). Indeed, propensities may be thought of as rooted in just those structures, characteristics and relations to which deep explanatory theories draw attention. This hints at the main ontological presuppositions of propensity theories and noticing it helps us to avoid certain confusions. For instance, failing to recognize propensities as deep, even occult, hence only indirectly testable — resembling in these respects field theories in physics — may mislead thinkers into confusing propensities with their chance outcomes (cf. Hacking, 1965), or into confusing the sense of propensity with how propensities might be measured or tested (cf. Braithwaite, 1953, Sklar, 1970), or into supposing that a syntactical or semantical rather than an ontological analysis will suffice to expose the sense of 'propensity' (cf. Levi, 1965).

Two distinct propensity theories of probability were introduced independently by C. S. Peirce (1910) and Sir Karl Popper (1957). Unfortunately neither philosopher has expressed his view sufficiently fully for the sense of propensity to be unambiguously clear. It is not my chief concern here to do exegesis on the original writings — although a little of that is needed to disentangle propensity theories from various misrepresentations. My aim is rather to give a fuller account of propensity in general and of a version of Popper's propensity theory of probability in particular.

I have discussed the preferability of Popper's view to Peirce's elsewhere (1974) and need not repeat that discussion at this point. Some of the arguments will in any case need to be repeated in my criticisms of recent writings by Mellor (1971) and Giere (1973) who both seem to prefer Peirce's theory where this differs from Popper's.

I

1. Adversus *Sklar: Are Some Propensities Probabilistic?*

The importance of metaphysical presuppositions is well brought out by a consideration of Sklar's attack on Popper's propensity theory of probability. The attack quite misses the mark because Sklar's metaphysical assumptions appear not to allow the formulation of Popper's theory.

For example, Sklar assumes determinism when discussing classical statistical situations (e.g., coin-tossing) (1970, pp. 361, 363, 366) and thus leaves room for only subjectivism or an objectivist theory related to the frequencies of actual outcomes. Thus, if propensity theory of probability were inconsistent with determinism, as I think it is, it would be ruled out at the start. Further, Sklar assumes a questionable version of positivism in seeking to analyze propensity in terms of the outcomes of trials on chance setups, thus taking concrete, individual phenomena as analytically primitive. If propensity were an unanalyzable primitive (as field of force is), no propensity theory could survive such (operationalist) analysis. Consistent with this positivist assumption is Sklar's confusion of the meaning of a term with the means of testing the truth of assertions employing the term (1970, p. 360), a confusion shared by Braithwaite.

Furthermore, it seems to me Sklar is asking the wrong question. My question, "Are some propensities probabilistic?" deliberately inverts the usual kind of question of which Sklar's title "Is Probability a Dispositional Property?" is an example. This type of question invites thinkers to contrast theories on the mistaken tacit assumption that there is only one proper theory of probability — this in spite of all of Carnap's influence, though it was probably Popper who first pointed out that there may be many theories, some corresponding to various interpretations of the calculus of probability, some not. Perhaps probability is not so homogeneous a concept that it must admit of only one sense. Perhaps some propensities could be probabilistic (or be the causes of chance outcomes) without all propensities having to be probabilistic or all statistical probabilities having to be rooted in probabilistic propensities. By contrast, the question I ask invites thinkers to scrutinize the concept of propensity and to ask whether some propensities might not be probabilistic. Thus my question calls for a general theory of propensity: it asks not simply whether certain predicates are dispositional, but rather what is it for an object or an object-environment complex to have a disposition, tendency, bent, proclivity, or propensity. This question will be taken up more forthrightly in later sections.

The divergence of Popper's views from those adversely criticized by Sklar is great. For example, Sklar writes "that probability is a dispositional property of 'the world,' a property that can be fully analyzed by reference to objective properties of things or of states of affairs, but only if the analysis makes use of the subjunctive mood . . . the subjunctive analysis needed is straight-forward, probability being analyzed in terms of what would be the limits of relative frequencies of outcomes in possible sequences of experiments. This is, I believe, Popper's view" (1970, p. 355). Sklar offers telling criticisms against this frequentist (mis)representation of Popper's views, leaving Popper's actual views quite unscathed. It is almost pointless for Popper to respond to Sklar's arguments; a simple denial that his view is captured by Sklar's characterization of it might suffice.

Nonetheless, one or two interesting points are raised by Sklar's analysis, aside from the point that there are important metaphysical presuppositions to a full-fledged propensity theory, for instance, Sklar's claim related to the distribution of values of parameters specifying initial conditions during a sequence of trials. Sklar claims that "what this distribution would be is completely unconstrained by any law-like features of the actual world whatever!" (1970, p. 363.) According to Popper, as he expressed himself to me in a private communication, there is a law of nature, that unless they are constrained, initial conditions have a ("natural") propensity to scatter over the interval left open to them by the (constraining) experimental conditions. Of course, that there is such a law of nature is a matter of conjecture. It is precisely this conjecture which, according to Popper, he and Lande have tried to formulate.

Secondly, Sklar raises the problem of the relativity of estimates of probability to specifications of the trial setup. He raises it in a curious context, namely, embedded in the problem of the utility of probability assertions, but it is an important problem in its own right. Indeed the problem was crucial to Popper's giving up a frequency interpretation of probability. In his 1959 article Popper presents this problem in a sharp fashion and adduces arguments which go to show in advance that Sklar's own theory of probability (compare Sklar, 1970, pp. 364–65) is at fault. Incidentally, Sklar does not cite Popper's 1959 article and thus may be supposed to have missed that presentation of a refutation of his own position. I shall discuss the problem of relativity to specification in section 3, where I discuss Giere's views. Giere considers it interestingly within the context of the problem of the single case, a problem Sklar does not seem to notice.

2. Adversus *White: Propensity Is Not Liability*

Alan White's article (1972) contains a number of errors of exegesis of a note by C. S. Peirce (1910), which should not be left uncorrected, as well as a number of other errors which render his criticisms of attempting to analyze probability in terms of liability beside the point. First the exegetical errors.

In calling Popper's propensity theory of probability (see Popper, 1957, 1959, and 1967) a "resuscitation" of Peirce's (1972, p. 35), White failed to take proper account of two significant differences between the theories. Peirce ascribes his "would-be" 's as a property of objects regardless of environment — in this he is followed by Mellor (see his 1969, 1971) — while Popper explicitly makes them relative to environment. This is not simply a matter of the conventions of language use, as Mellor suggests, but rather, and more importantly, a matter of what we want to assert, as scientists, about the world. Do we wish to say that the disposition of falling to the ground is a disposition of an object, as such, regardless of its environment? Would we not rather say it was a disposition relative to a gravitational field? Secondly, Popper's theory, but not Peirce's, is aimed to make sense of single-case probabilities.

White's second exegetical mistake is to ascribe to Peirce the intention to *analyze* the notion of probability (1972, p. 35). As far as I can tell, Peirce (1910) attempts no such analysis. Certainly Peirce does not describe what he is doing as analysis. Rather he says he is aiming to *define* probability (1910, pp. 404, 405, 409), a word "that we, all of us, use . . . with a degree of laxity which corrupts and rots our reasoning" (p. 405). Peirce's method is first to distinguish probability from likelihood and from plausibility by characterizing the latter terms (pp. 406–8), and then to define the meanings of probability statements by giving synonyms, one of which he coined for the purpose. This is not analysis. Indeed it is questionable whether we are called upon to analyze probability, construed as a propensity, any more than we are called upon to analyze gravitation, construed as a field of force. Some positivists attempt analyses of probability in terms of the relative frequencies of different kinds of outcomes of trials on a chance setup, but their analyses, one suspects, are aimed to eliminate probability as an unanalyzed primitive, in favor of discrete events or inert properties of particular objects (being head uppermost, say) as primitive. What Peirce's theory aimed to do, by contrast, was to suggest probability had roots in the qualities of the relevant physical objects.

Thirdly, White misrepresents what Peirce calls a "habit." White claims that a habit differs from a liability in the same way "what does happen when" differs from "what would happen if" (p. 35), which is an infelicitous contrast in view of Peirce's identification of habit with "would-be." Indeed Peirce is very careful not to confuse habit in his sense with habit in White's sense, that is, repetitious behavior (Peirce, 1910, pp. 409–10, 412).

Perhaps a more serious shortcoming is White's failure to appreciate which property propensity theories ascribe to what objects. White appears to be unable to shake off frequentist predilections sufficiently to think probability could be anything but the chance of certain outcomes of trial runs on chance setups. He seems not to appreciate that, as Popper puts it, "the word 'probability' can be used perfectly properly and legitimately in dozens of senses, many of which, incidentally, are incompatible with the formal calculus of probability" (1967, p. 31). (See also Popper, 1935; Carnap, 1945; and Bunge, 1967a, 1967b.) White writes, "Unfortunately for the propensity theory . . . probability is not *in fact* attributed to such things as coins, dice, . . . etc. It is not the probability of the coin, but of the coin's falling heads, that is in question" (p. 38, my italics). Thus with one sweep of his bold "in fact," White slays all propensity theories of probability! One must protest! If White is right on current usage (which I doubt), propensity theory challenges current usage — as do all novel theories. Propensity proponents, whatever else they disagree about, tend to agree that the probability of a coin's falling heads, *construed as a propensity*, is not a property of the coin's falling heads nor even a property of the set of possible outcomes of the coin's being tossed, but rather is a property of the physical object in the game — according to Peirce and Mellor, a property of the coin; according to Popper, Hacking, and myself, a relational property of the complex of tossing device and coin. Of course, probability construed as a frequency may be a property of the coin's falling heads relative to other possible outcomes. (For example see, Von Mises, 1919a, 1919b.) But that is a different theory.

White's most serious mistake, serious because it ruins his argument, is to identify propensity with liability. To be sure, Popper is to blame for introducing the phrase 'is liable to' as a partial explication of propensity (1957, p. 67). But White is to blame for taking the synonymy of liability and propensity seriously, because liability is referred to nowhere else in Popper's writings on propensity theory. Usually Popper uses 'tendency' or 'disposition' as synonyms. (See, for example, Popper, 1957, pp. 67, 68, 70; 1959, pp. 30, 31, 35, 36, 37; 1967, p. 32).

White is correct to notice the categorical difference between liability and likelihood and quite correct to affirm "one cannot analyse the notion of being likely in terms of the notion of *being liable*" (p. 43). But the declared purpose of his paper was to show the propensity theory of probability to be mistaken (p. 35). Remarks about liability are not at all to that purpose. White's criticisms concerning liability do not hold for tendency or disposition, save that likelihood is not to be analyzed in terms of tendency or disposition either. But then propensity proponents do not use these concepts to analyze 'probability' but to interpret it. Propensity theory of probability remains unscathed from White's attack, since precious little remains of his argument if propensity theory does not imply either that probability in the sense of chance is to be analyzed in terms of propensity or that propensity is to be understood as liability.

One final remark. It should not be surprising if proponents of propensity theory of probability differ. There is not simply one theory, as White's title suggests: there are at least eight significantly different theories, varying in their acceptability, ascribable severally to Peirce (1910), Braithwaite (1953), Popper (1957), Hacking (1965), Bunge (1968a, 1969), Mellor (1969, 1971), Giere (1973), and myself (1973a, 1974), aside from a "qualified propensity theory" such as Levi's (1967) and a caricature of a propensity theory such as Sklar (1970) criticized, but which does not belong recognizably to any of the authors mentioned. Incidentally, in none of these theories (save Sklar's caricature) is chance *analyzed* in terms of propensity and in none (save for Popper's slip of the pen) is propensity identified with liability.

3. *Giere versus Popper: The Problem of the Single Case*

Let us suppose, with Popper, that the frequency theory of probability (or the frequency interpretation of the calculus of probabilities) offers a consistent account of probabilities with respect to statistical sequences. There remains the problem of the single case. From a frequentist position, "the probability of an *event of a certain kind* . . . can be *nothing* *but* the relative frequency of this kind of event in an extremely long (perhaps infinite) sequence of events" (Popper, 1959, p. 29). Now, if the *kind* of event is defined in terms of the sequence, decisive objections can be raised. If, on the other hand, the kind is defined in terms of the generating conditions, there has been a shift from frequency theory to propensity theory. This, at least, is how Popper argues; and I endorse the argument. The crucial point about propensity theory, as I understand it in Peirce and Popper, is this: the probability of this or that outcome's happening given certain initial conditions of a chance setup is determined by the physical characteristics of the setup and not by any sequence to which the outcome might belong.

Giere thinks Popper's account of propensity runs into the same difficulties as Popper thought frequency interpretations did. Specifically, Giere argues that "relativity to specification" (1967, p. 39) plus defining propensity as a disposition "to produce sequences whose frequencies are equal to the probabilities" (1959, p. 35) leads to a relativity to sequences, since, as Popper asserts, "any particular experimental arrangement may be regarded as an instance of more than one specification for 'its' repetition" (1967, p. 38). Let me quote at length from Giere (1973):

> But what counts as a repetition of the same experiment? For example should the maximum height of the coin be specified? If it is not, then the toss in question may be a member of many virtual sequences, e.g. one in which the height is ten feet and one in which it is nine feet. But if the limiting frequency of heads is different in these two virtual sequences, as it might be, then, following the line of argument Popper himself employs against the limiting frequency interpretation (1959), it would be wrong to assign to this single case the virtual frequency of the sequence of ten foot tosses if it was in fact a nine foot toss. Thus Popper fails to solve the problem of the single case for the old-fashioned reason that he provides no solution to the problem of the reference class.

Let me say immediately that Giere is quite right to question what appears to be Popper's view that propensities are essentially tendencies to produce sequences. I say "appears to be" since it is not unambiguous in Popper's writings that this is his view. Giere is right to challenge such a view's capability to solve the problem of the single case. I shall return to this point shortly. First, however, I wish to clear up a confusion in Giere's remarks.

Giere appears to confuse specifying generating conditions with specifying sequences. I have difficulty understanding the relevance of the remarks about maximum height of toss of a coin unless either Giere has confused those two types of specification or Giere wishes us to believe that maximum height of toss is part of the generating conditions. This last I do not accept, though I am quite prepared to accept that the generating conditions may include a factor which *results* in different ranges of height tossed. What, we may ask, is the import of Giere's remarks about different *sequences* to which a nine-foot toss may belong? Certainly we should not be distressed if certain (virtual and virtually infinite) sub-sequences of the (virtual and virtually infinite) sequence of possible trials of any "well-characterized" chance setup had different limiting frequencies. This is perhaps to be expected; it is a component of the problem into which frequentists run when attempting specification of sequences. It fails to be a problem for the propensity theorist, however, since the more or less well-done specification of *generating conditions* gives no grounds for differentiating (virtual) sub-sequences of the (virtual) sequence of possible trials on the setup. Related, though quite different problems which arise for the propensity theorist, but not for the frequentist, are as follows: the relations between the virtual sequence picked out by the specified generating conditions and any actual sequence obtained by trials on a setup governed by those conditions; and the problem of interpreting the results of a statistical test. Giere's remarks about different heights of toss bear on this latter problem: if the factor resulting in different ranges of height tossed is omitted from the specification of generating conditions, actual sequences of tosses so generated may vary, some including, some not including, tosses above nine feet, thus increasing the range of possible members and thus the difficulty of interpreting the observations. It is, of course, open to the experimenter to improve the characterization of the setup, if this should occur to him, to include the missing factor and to try again. Nonetheless Giere's remarks do not hit Popper's propensity view, their intended target. Especially, Giere's claim that Popper has failed to solve the problem of the single case because he has failed to solve the problem of the reference class is beside the mark. The problem of the reference class does not arise in the same manner for propensity theorists as it does for frequentists nor is it the same kind of problem. For frequentists, as Popper has shown, the problem bites deeply into their interpretation of probability since it touches the difficulty of specifying sequences without specifying generating conditions. For propensity theorists, it raises merely the practical problem of interpreting tests, since the virtual sequences picked out by generating conditions will be more or less narrow in range of possible members accordingly as the generating conditions are more or less restrictively specified, the experimental setup more or less well characterized.

Let us now turn to the question whether a propensity theory which interprets propensities as tendencies to produce sequences is to be preferred to one which eschews such interpretation.

Giere clearly prefers to characterize propensity with no reference to a sequence. Popper has done this in referring to weighted possibilities. But Popper has taken a second step in interpreting the "weights" as "measures of a tendency of a possibility to realize itself upon repetition" (1967, p. 32). What end is served by taking this second step? At a first glance, there are two ends that might be served: first, unless propensities are linked to frequencies of chance outcomes, the estimates of probability interpreted as propensity cannot be tested by the most obvious method of comparing those estimates with the empirical measures of probability interpreted as a frequency; secondly, single-case propensity theory could illuminate the sense of probability as used in statistical laws in science only if single-case propensities are linked to the virtual frequencies of various possible outcomes of repeated trials. Nonetheless, those two advantages should not lead us to insist on the propensity *to produce sequences* being essential to the sense of the propensity to produce a single event, else the claim to give an account of single-case propensities is not adequately fulfilled. Let me put it this way: we may take the propensity to produce single events as prior and hence explain propensity to produce sequences. We could also, perhaps, explain the randomization within certain limits, of the values of measures of initial or generating conditions. But if we do so, there is a certain circularity in demanding that the sense of single-case propensities depends upon the sense of propensity to produce sequences. The circularity is not vicious, just disquieting. Perhaps it can be dispensed with. Perhaps we can accept that the sense of single-case propensities is illuminated, where repetitions are appropriate, by considering the virtual distribution of various outcomes on a virtual long run of trials, without its depending on such consideration.

In contrast to the frequency-linked sense of propensity, a thorough-going single-case propensity estimate might be a weighted possibility estimate without regard to repetitions of any trial. Common use of 'probable' illustrates such repetition-free estimates. For example, in the troublesome case of the race horse. Several philosophers have found it difficult to say how propensity theory might apply to horse racing, and this is so, if applying propensity theory means furnishing a method for calculating rational odds. Nonetheless, in estimating odds, a sensible gambler pays attention to various factors related to the physical characteristics of the various horses in the race — the identity of the horse's sire and dam; who his trainer is; news from the stable about his health and how well he moves on different going; and so on. But it is inappropriate here to speak of virtual sequences: each race, composed, as it is, of so many possible factors (horses, jockeys, the going) has such a strong air of uniqueness. The probability of a particular horse's winning a particular race, construed as a propensity, may be envisaged as a weight of a possibility among several variously possible outcomes.

One further question concerning relativity to specificity. Popper has asserted that propensities are properties of repeatable experimental arrangements rather than any particular experimental arrangement. Is this assertion primarily practical (methodological), making an assertion about experiments, especially regarding limitations in how they may be specified, assuming they must be specified before being performed and assuming they are *repeatable* with reference to the terms of the specification for their first performance? Or is the assertion epistemological, making an assertion about our knowledge or possible knowledge of propensities, especially drawing attention to the fallible, corrigible, and simple character statements of propensity share with theories? Or is the assertion ontological, asserting that propensities as properties of a physical setup are not independent of human definition (specification) of a subset of the many other properties characteristic of the setup? I find it hard to accept that Popper's remark is ontological, since, if it were, it would imply that a ghost ("the observer") had crept back into the system: that real propensities are specification-dependent is tantamount to the abandonment of belief in *reality* as opposed to a scientific conjectural reconstruction of reality. I am prepared to say that in the case of conjectured reconstructions of reality *conjectured* propensities are specification-relative, but not that the *real* propensities are so relative. I think the propensity of such-and-such an outcome of a trial or a particular chance setup — say the propensity of the actual setup to yield a six uppermost on the throw of a biased die — has a value, let us say, r. Differing ideas about what constitutes that particular chance setup, before or after its being set up, may lead to varying conjectures regarding the propensity in question, more or less close to r. But the varying specifications and points of view do not alter r; they merely make it more or less difficult to estimate r correctly. Whereas the actual propensity to yield a six uppermost depends upon the gravitational field, the resilience of the surface onto which the die is cast, the location of the bias and whether it is fixed, the shape of the earth's magnetic field at the place of throw if the bias is magnetic, etc., an *estimate* of the value of the propensity will depend upon the values and the weights allowed to each factor *thought relevant*, thus displaying a subjective component.

4. *Mellor versus Popper: Of What Is Propensity a Property?*

Mellor sides with Peirce, and with common usage, in ascribing propensities to things. As Popper noticed, this has been common in science since Aristotle, who ascribed potentialities to things: "Newton's was the first *relational* theory of physical dispositions and his gravitational theory led, almost inevitably, to a theory of fields of forces" (Popper, 1957, p. 70). I shall assume that common usage shall not be an arbiter over propriety of scientific usage, since at least one aim of science is to reform knowledge and hence to reform common sense. Therefore one result of successful science may be a reform of common usage. This leaves the question of the ascription of propensities open. Let us consider a few examples, say, Mellor's. He cites solubility and fragility, and gives an extended discussion of dying and of spontaneous disintegration of atomic nuclei.

First, solubility. Popularly sugar dissolving in water is a paradigm. But in science textbooks, solubility is considered more generally. It is a relational property defined on at least two substances which mix. Van't Hoft, who pioneered much interesting work on the properties of solutions, defined a solution as a homogeneous mixture of two or more substances. His definition is generally accepted. It is a matter of convention which of two miscible substances is to be called the solvent and which the solute, if these terms are to be applied. If a solid is mixed with a liquid, it is conventional to call the solid the solute even when it furnishes more than half the weight of the solution. Moreover, theories of chemical composition and of dissociation aid understanding of the process of dissolving, thus pointing up more sharply the relational nature of solubility. It distorts our understanding of dissolving to say solubility is a property of the solute. To claim that solubility is a property of a dyad, albeit an ordered dyad, is not to claim that neither member of the dyad has peculiar properties relevant to solubility. On the contrary, chemical and physical theory points not only to the action of one substance relative to another, but also to the structure and properties of each substance relevant to such relative action. For example, work on atomic structure, which had light shed upon it by the theory of dissociation arising from a study of conductivity of solutions, continued alongside other work on solutions, osmosis, and so on.

Second, fragility. To uncover the sense in which being fragile is a property of interest in scientific inquiry (as opposed to a property of interest to normative ethics) we need to explore theories which deal with the behavior of substances or objects of particular shapes, under stress of varying kinds (torque, impact, sheering, etc.). Fracture may then be seen as a limit phenomenon related to elasticity. Clearly, elasticity is dyadic, since it relates things or substances to stresses of various kinds. Fragility is hardly a scientific concept at all, though the propensity to fracture under certain loads is. There are root relevant properties properly ascribable to an individual object, such as the strength of connectivity of its parts (which incidentally is a relational property, also, but it is a property the parts of the whole share). Nonetheless, the propensity to break is a propensity relative to conditions which overwhelm the forces connecting parts together; hence it is not a property an object has in (conceptual) isolation. Common usage confuses the issue, to an extent, since fragility usually is ascribed only to those objects which are disposed to break under *ordinary* impacts, such as may result from falling or from being struck by hand, but such breaking points occupy a mere short span of the range of possible breaking points.

Spontaneous disintegration of radioactive nuclei is one of the few phenomena under current scientific investigation (I forgo predicting what other similar properties may or may not be the subject of future inquiry) for which the relevant propensity is possessed by objects in isolation. (Even so, deeper theories may probe the characteristics of nuclear connectivity and expose how the parts come to disconnect — but that is another matter.) However, examples such as this will not bear the burden of interpretation of the sense of propensity, which more often than not in scientific inquiry is a relational property.

I do not wish to go deeply into Mellor's discussion of death risk since it raises no further questions germane to my purpose in this paper. But I cannot forbear to remark how Mellor comes so close to a dyadic theory of propensity without actually endorsing it. He writes, "With a variety of people in similar environments we take their different chances of death to display different propensities in them. With similar people in a variety of environments we take their different chances of death to display different propensities in the environment" (1970, p. 89). It seems curious of Mellor to fail to connect the various relevant properties of individuals (say, the weakness of their respiratory organs) with relevant properties of the environment (say, the air pollution index) in order to arrive at death risk as displaying (to use his term) a propensity *jointly* of the person and his environment to induce his death or as displaying a propensity of *an individual relative to an environment* to die. I find the notion of a person's propensity to die in isolation from any environment somewhat incoherent, although the propensity of a radioactive nucleus to disintegrate in isolation is not similarly incoherent.

My point is not that all dispositions are relative, but that dispositions of isolated systems should be considered as special cases, with relative dispositions (dispositions of systems relative to environment) as the general and basic case (cf. my discussion of this point, 1974).

II

5. *Variants of Propensity Theories*

If my aim is to expose presuppositions of propensity theory, it behooves me to say whether the exposure is intended to relate to all possible propensity theories or only to some. If the latter, I should say which. Let me first recall the two significant differences between Peirce's view and Popper's to which I referred in section 2: whether propensities are to be ascribed to systems (à la Peirce) or system-environment complexes (à la Popper); and whether the theory is intended to explicate the single case (à la Popper). These differences supply a table into which the eight theories I have referred to fit more or less neatly:

	Frequentist	*Single Case*
Systems	Peirce	Giere Mellor
Complexes . . .	Braithwaite Hacking	Bunge Popper Settle

Of course, we can readily distinguish Braithwaite's views from Hacking's by virtue of Braithwaite's insistence that the meaning of a probability assertion is to be found at least in part in the rules adopted for the acceptance or rejection of attributions of particular degrees of probability to particular outcomes. And we can distinguish Mellor's views from those of others by virtue of his curious dissociation of propensities from statistical probability. For Peirce and Popper, propensity theory was intended to interpret statistical probability; for Mellor, statistical probability is to be grounded on personalist theories while things having dispositional properties is what makes some chance statements true. The differences between the views of Bunge, Popper, and myself are much more slight and may even be illusory: clarification of the various expositions may reveal identity of view. I do not propose to expose these differences now, since I have done so in my (1974) to an extent, except to note that whereas Popper and I talk about the propensity of system-environment complexes to realize certain events, Bunge several times has talked about the propensity of events to realize themselves (e.g., 1967a, 1967b). This may simply be a difference in manners of speaking or it may more seriously be a difference in theories of causality of events. I think it the former and hence I do not wish to pursue it further.

Initially, I shall aim to expose some philosophical presuppositions of my own theory of propensity, which closely resembles Popper's and Bunge's. I shall argue that theories which do not share these presuppositions are less satisfactory on that account. Although I shall not give any detailed exegesis of the literature to pick out which thinkers appear to share my presuppositions and which not, I shall give some cursory indications.

6. *Skeptical Realism*

The first group of presuppositions to which I wish to draw attention cluster together to form a view I have dubbed 'skeptical realism'. Let me explain them one by one.

Realism. In the first place, and perhaps trivially, I should say I do not want to fight over word usage; hence I shall not object to a solipsist or anyone else using the term 'propensity' in any sense he cares. Nonetheless I think the sense of propensity in a metaphysics which failed to assign reality to things and to their properties would be vacuous.

In the second place, and more importantly, the realism I have in mind is opposed to various versions of instrumentalism or operationalism which rule out assigning reality to the possible referents of theoretical terms. I have a number of grounds for thinking false the philosophical theory which assigns existence only to those objects or characteristics of objects which are the matter of immediate perception, but I do not wish to go deeply into the problems of perception at this point. It perhaps suffices here to remark that a propensity theory embedded in such a view would be vacuous. On such a strict phenomenological view, it would be possible to give a construal to the term 'habit' in White's sense (see section 2 above) but not to distinguish between habit in that sense and habit in Peirce's sense of "would-be." Since that distinction is crucial to Peirce's (and Popper's) concept of propensity, a strict phenomenology is inimical to propensity theory.

In the third place, I should remark that the realism I have in mind does not imply either that sense data are reliably veridical (naive realism) or that there is a referent for every theoretical term in the sciences. Rather it implies that theories in the sciences are intended to model the real world more or less closely.

Realism is not peculiar to propensity theories of probability, being a presupposition also of frequency theories. It is not, however, presupposed by subjectivist or personalist theories of probability, since those theories are compatible with ontic determinism. Ontic determinism rules out objective chance and hence evacuates probability statements of any reference to reality except superficially, as mere reports of observed frequencies: probability statements refer instead to ideas, to private states of partial belief. This is not to say, however, that ontic indeterminism may not be wedded to a personalist theory of probability so that reality may be assigned to some referents of some probability statements (see Mellor, 1971).

Depth. Propensities generally do not lie on the surface of things, hence propensity theory presupposes depth. Depth has at least two senses worth distinguishing here. In the first place, depth refers to theoretical sophistication in the sense of maturity, as discussed by Bunge (1968b). In the second place, depth refers to distance from sight, to hiddenness. These are not the same concepts, although their extensions overlap. For example, the classic "dormitive virtue" is an *occult* power but displays no theoretical depth. Some propensities are deep in both senses, for example, the propensity of radioactive nuclei to disintegrate.

Although realism is not peculiar to propensity theory of probability, the presupposition of depth is. Both frequentist and subjectivist theories are compatible with a strict phenomenology, with what Bunge calls "black-boxism" (1964). Indeed, the development of frequency theories of probability in this century as the way to obtain an *objective* interpretation of probability went hand in hand with the development of a fairly strict positivism, which sought to reduce deep concepts (theoretical concepts) to observational concepts or even to operations.

A propensity theory which did not presuppose propensities to be occult properties (if not theoretical) and thus deep would reduce a propensity statement to a shorthand form for a set of statements predicting outcomes of possible trials on various setups.

Skepticism. I deliberately emphasize propensity in general as an occult property to draw attention to a deep and far-reaching tacit error in modern philosophy of science and in modern epistemology, namely the assumption that the propriety of knowledge claims depends upon their being justified, or at least justifiable. This assumption, coupled with the traditional empiricist assumption that experience is the only legitimate source of warrant for knowledge claims about the world, led to a widespread denunciation of serious speculation about what lies below the surface of things. Scientific knowledge was presented as a more or less satisfactory ordering of reports of experience. Where scientists' speculations gave deep theories, thinkers in this empirical justificationist tradition reinterpreted those theories, to their ontological impoverishment. Propensity theory clashes with this tradition, since propensity statements share with deep theoretical statements an incapacity to meet the demands of empirical justificationism without evacuative reinterpretation. Hence propensity theory may be said to presuppose the falsity of empirical justificationism.

Let me explain this point a little more fully. Empirical justificationism *initially* presupposes two types of scientific (or, simply, cognitive) claim — particular (singular) and general (universal); and two types of scientific (nonlogical) term — observational and theoretical (or nonobservational). Putting these two together leads to a further division of universal claims into empirical laws, using only observational terms; theories, using only nonobservational terms; and bridging laws (or correspondence rules), using both sorts. There are difficulties in carrying through all these distinctions in a thorough fashion (see, for example, Suppe, 1972), but let us not consider these just now. Next, empirical justificationism presupposes that singular statements employing observational terms can in principle be checked for truth or falsity by inspection of the real world by a competent observer who can report sensibly what he sees. Again there are serious problems here, some of which spill over from the difficulty of carrying through the distinctions above in a thorough fashion — for example, the problem that observation reports are theory-impregnated and hence are remote from raw observation (Popper, 1935; Agassi, 1966a). Others are fresh problems, such as Descartes's objection to experience as a final and authoritative arbiter on matters of fact: namely, that any particular putative observer at any particular time may be deluded, dreaming, drunk, drugged, or simply mistaken (Descartes, 1741). Descartes's problem is usually met by some such device as Popper's suggestion that intersubjectivity replace objectivity as a desideratum for scientific claims to satisfy. Of course, that quite dethrones experience as arbiter and places the solution of problems of perception, etc., into the domain of social conventions of justification. (For further discussion of this point see my 1969b, 1971a, 1974, and Agassi, 1971.) Goodman (1955) shows it may be impossible to justify the use of one predicate rather than another similarly justified yet startlingly different predicate in a true description of an object. Goodman's riddle is usually presented as a riddle of induction, but it has repercussions at the simple level of reporting experience. I do not believe that any of the attempts to solve Goodman's riddle within the framework of a logic of confirmation have had success: the riddle seems capable of solution only in the context of a socially acceptable standard for justifiable (excusable) behavior.

Assuming, for the moment, that all those problems can be taken care of, empirical justificationism asserts that for a claim to know a universal statement (law, theory) to be properly made, the statement must enjoy support from other justified statements, principally singular observation reports. Now it is quite clear that a universal statement is not proved by an accumulation of particular statements which report instances of it — except where the instances exhaust the possibilities. This point was first made by Aristotle (*Posterior Analytics*). Hume is famous for his refutation of induction (see his 1748) in which he appears to me to confuse Aristotle's objection with an objection raised by Maimonides (1194): experience cannot tell us that what we take to be the laws of nature will not alter in the future (say, tonight). Coupled with Goodman's riddle (which is a fresh amalgam of Aristotle's and Maimonides' objections) these objections are decisive against any strong form of empirical justificationism (see my 1969b, 1971b). If it is difficult for empirical laws to satisfy the demands of empirical justificationism, it is even more difficult for theories to do so, since they are further removed from raw observation. The same holds for propensities. But for propensities, one further difficulty arises: the problem of potency. Many propensity claims pick out potencies or potentialities of systems-in-contexts. The tough-minded version of empirical justificationism has always disallowed potencies as real, since potency is a metaphysical notion with no counterpart among the concepts of the mechanics of inert matter (compare Wisdom, 1971). This point will be taken up in the next section.

Popper has suggested giving up justificationism, thus allowing once more deliberate speculation about what the world is made of fundamentally. Speculation would not of itself lead afresh to essentialism or intellectualism, since these metaphysical viewpoints are themselves justificationary, though not empiricist. Empiricists had hoped to curb these metaphysical excesses by demanding *empirical justification*, but the demand itself has proved excessive. It is clear from the history of the most creative moments in science that at least part of the task of scientists is freely to propose hypotheses concerning the hidden structure and properties of physical systems and environments which may explain their observable structure and properties — and perhaps even help us to control their future behavior in some respects. Perhaps excesses would be sufficiently curbed and rationality — hence intellectual respectability — sufficiently safeguarded, if all hypotheses were to be held open to criticism, including empirical criticism where any can be devised. (For the background of this theory of rationality and for further discussion of the role of criticism in rationality see Popper, 1945; Bartley, 1962, 1964;

Agassi, 1966b, 1969; Jarvie and Agassi, 1967; Agassi, Jarvie, and Settle, 1970; Settle, 1971a, 1974; Settle, Jarvie, and Agassi, 1974.)

7. *Dynamism*

Propensity theory presupposes propensities to be causes or at least partial causes of the events which the carrier of the propensity is disposed to bring about. It seems merely vacuously a property of a system relative to an environment that if the system be placed in such an environment, certain events follow. The constant conjunction theory of causation evacuates all causal links of their active or dynamic component. Moreover, it does so on empirical justificationist grounds. (One more reason for rejecting empirical justificationism?) Customarily modern philosophers of science eschew use of the ancient concept of efficient cause, replacing causal explanation (in Aristotle's sense) with deductive-nomological explanation (in Hempel's sense) (Hempel and Oppenheim, 1948; Hempel, 1961, 1964). There are some exceptions (Bunge, 1959; Agassi, 1964; Settle, 1973a; Brody, 1972), but not many, considering the trouble proponents of the covering law model have had in trying to block undesirable maneuvers. (For detailed discussion see Nagel, 1961; Eberle, Kaplan, and Montague, 1961; Kaplan, 1961; Kim, 1963; Hempel, 1964; Ackermann, 1965; Ackermann and Stenner, 1966; Omer, 1970; Morgan, 1972.)

The most serious objection to the deductive-nomological theory of scientific explanation is that it appears to provide no adequate grounds for distinguishing between those generalizations which we wish to consider as natural laws (or at least as approximations to natural laws) and those which we think to be only superficial laws (phenomenological laws), or for distinguishing both of those from accidental generalities. I follow Agassi (1964) in thinking that theories regarding what is naturally necessary (natural laws) are properly judged relative to a metaphysics or cosmology. This propriety arises not only because what is to be announced as physically necessary will first, as a matter of scientific custom (compare Bunge, 1967c, 1970c), have to pass metaphysical tests, but also because the very idea of natural necessity is metaphysical, hence already rooted in a metaphysics, or world view. Of course, it is not absurd to deny that there is a natural necessity, though the world view such a denial presupposes might be hard to live with, psychologically speaking.

In spite of the strength of the empiricist tradition stemming from

Hume, the idea of dynamic cause remains a presupposition of much discourse both in the arts — for example, law, medicine, and historiography — and in the sciences, not to mention common speech (!). In physics, for example, we have not been able quite to rid ourselves of the notion of *force* (gravitational, electromagnetic, nuclear, etc.) despite some energetic, not to say forceful and influential, analytic attempts — notably Mach's (1883). For example, the dynamic notion of gravitational collapse is used to explain both the genesis and the nemesis of giant stars or stellar systems (collapsars or "blackholes"). Of course, by no means all laws in science make use of the concepts of force or power or some other version of dynamism or efficient causation; and even in those which do the idea of drive or cause is often masked by the symbolic representations of the theories, which might help to explain why scientific explanation is so widely held to be the subsumption of instances under generalities. I do not mean to deny that some explanations in science are merely matters of such subsumption, as, for example, explanations afforded by phenomenological ("black-box") laws. Nonetheless, explanations, to be fully satisfactory, appear to need some causal or dynamic assumption in the explanations. It is not my intention here to discuss theory of scientific explanation in detail: I merely wish to stress that the causal or dynamic character of propensities brings propensity theory into conflict with the received view of theories and of scientific explanation.

In my view, the idea of propensity is closely linked to the idea of natural law: natural laws imply propensities. A statement of a propensity may perhaps be regarded as a conjectured ontological hypothesis (or at least an hypothesis rooted in an ontology) concerning some physical property of a system-environment complex, declaring or hinting at a natural dynamical relation between the system and its environment, or parts thereof, in certain, perhaps virtual, states, which would lead to the complex entering other states.

8. *Indeterminism*

Nonetheless, the gap between deep theories and reality is great, and perhaps unbridgeable. The Parmenidean rift between reality and appearances has not been bridged by any theory so far proposed, if by bridging it we mean showing how human speculations concerning the constitution and mode of behavior of the real world (of, say, a thing-in-itself) have the right to be correct. In my view, this rift may be bridged, but only after a fashion: our speculation concerning the structure and dynamism of the real world is our attempt to bridge it; and our criticisms of our speculations are efforts to eliminate poor bridges. Even our best bridges may be poor ones. In particular we may notice scientific knowledge is built up piecemeal and not even cumulatively: law statements refer simply to one level of analysis of reality (or to one degree of resolution or focus). Programs for the *integration* of bodies of theory referring to varying domains of reality remain incomplete despite a few successes, such as the integration of theories in the electrical and magnetic domains. I follow Bunge (1967c, 1970b) in regarding the reduction of theories at some levels or in some domains to theories at other levels or in other domains as still largely a program, despite Nagel's (1961) optimism, except where one theory turns out on axiomatization to be a subtheory of another. The contrary would be surprising. It would be surprising if we had so successfully grasped the core of reality that we could integrate all laws into one axiom system.

Although it may be said that all systems of theories intended to illuminate or explain how the world appears to us model the real world to an extent or after a fashion, including existentialist theories of man, the claim of any one particular theory system (say elementary particle physics or existentialism-cum-phenomenology) to sovereignty over all is to be resisted in view of the illumination afforded our comprehension of reality by a variety of systems arising from a variety of types of analysis and comprising a variety of law statements, some qualitative, some quantitative, some deterministic, some stochastic.

In particular, the claim made by some philosophers, oblivious perhaps of the fragmented, many-layered quality of the body of scientific knowledge, the claim namely that there are only two metaphysical options regarding orderliness — strict determinism and the doctrine of sheer chance — is to be resisted.

Both strict determinism and the doctrine of sheer chance are extreme and happen to be oversimplifications: both of them oversimplify the relation between law statements belonging to different levels of analysis by supposing them all reducible to one rock-bottom level which is taken to be either deterministic or stochastic. In my view, both strict determinism, which contradicts ontic probability, and the doctrine of sheer chance, which undermines the lawfulness propensity theory relies upon, are inimical to probabilities as propensities. Hence I say that propensity theory of probability, though not propensity theory in general, presupposes the falsity of both those ontological doctrines as well as the falsity of the reductionism which they presuppose. I shall return to this point shortly.

Of course, even if all natural laws were deterministic, there might still be statistical laws in science, owing to our ignorance or to our habit of referring to just one level of analysis or of focus at once. Such laws would be superficial: to uncover probabilities as propensities, we should have to dig more deeply. The question whether some propensities, some natural hidden potencies of some systems in some environments might be probabilities is equivalent to the ontological question whether the universe is thoroughgoingly deterministic or is at least in part stochastic. This is not perhaps the place to defend my view that the universe is partially indeterministic (see my 1973b), but at least I should distinguish several senses of determinism in order to present the view more sharply. Let us call 'deterministic' a law or theory which yields a unique value for each variable in its formalism when the values of the other variables are fixed. Then we could distinguish determinism as a regulative maxim — "Look for deterministic laws or theories!" — from the epistemological claim that all the laws we know in science are deterministic; and we could distinguish both of these from ontic determinism — "All true laws of nature are deterministic."

Is propensity theory of probability consistent with ontic determinism? Of course, epistemic indeterminism — the view that as far as we know in science at the moment some laws are stochastic — is consistent with determinism both as a maxim and as a metaphysical claim. Hence, on the face of it, theories of probability should also be consistent with determinism. In my view, the realism presupposed by propensity theory and the presupposition of depth commit proponents of a propensity theory of probability to ontic indeterminism. No other theories of probability presuppose indeterminism, although none is inconsistent with it.

9. *The Crux of the Matter*

I began part II by announcing that I aimed to expose the presuppositions of that propensity theory which Popper, Bunge, and I appear to share, after having first differentiated on various grounds between that theory and several others. It is now clear that a further, perhaps more important, differentia could have been used. We may distinguish between, on the one hand, a theory of propensity which is little more than a frequentist theory embellished with an abbreviative concept — disposition — and, on the other hand, a theory which asserts propensity in the ontologically full sense I have sketched. Put another way, we may have a theory in which the propensity concept summarizes possible outcomes, contrasted with a theory in which the propensity is regarded as at least part cause of the outcomes. Of course, viewing probability as a frequency is not *incompatible* with viewing probability as a propensity. On the contrary, the two may go well together. Bunge has gone further and suggested that what he calls "physical probability theory" should be viewed as a union of three models:

"M_1: Propensity interpretation (quantified objective possibility) . . .
M_2: Randomness interpretation (objective chance) . . .
M_3: Statistical interpretation (relative observed frequency)" (1967a, pp. 90–91).

Not all thinkers wish to enhance a frequency interpretation (a union of Bunge's M_2 and M_3?) by the addition of a causally interpreted propensity theory, no doubt feeling that to allow that probability construed as a frequency is a dispositional property of the objects or generating conditions concerned goes far enough. Perhaps here is the crux of the matter: *Is the introduction of propensity theory of probability to be allowed to reintroduce* (or at least to emphasize) *an active or dynamic ontology at the root of science?* For my part, I hope so.

There is, of course, much clarificatory work to be done on such an ontology and on its bearing on probability. I could try here only to draw attention to it and to its bearing on propensity theory of probability. I hope I have done enough to provoke more discussion. My view that philosophy may be creatively relevant to science (see my 1971b) gives me optimism that further work may shed yet more light on probabilistic theories in physics and elsewhere as well as on ordinary language discussions of chance.

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