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WHY PROPENSITIES CANNOT BE PROBABILITIES

Paul Humphreys

The notion that probability theory is the theory of chance has an immediate appeal. We may allow that there are other kinds of things to which probability can address itself, things such as degrees of rational belief and degrees of confirmation, to name only two, but if chance forms part of the world, then probability theory ought, it would seem, to be the device to deal with it. Although chance is undeniably a mysterious thing, one promising way to approach it is through the use of propensities—indeterministic dispositions possessed by systems in a particular environment, exemplified perhaps by such quite different phenomena as a radioactive atom's propensity to decay and my neighbor's propensity to shout at his wife on hot summer days. There is no generally accepted account of propensities, but whatever they are, propensities must, it is commonly held, have the properties prescribed by probability theory. My contention is that they do not and, that rather than this being construed as a problem for propensities, it is to be taken as a reason for rejecting the current theory of probability as the correct theory of chance.

The first section of the paper will provide an informal version of the argument, indicating how the causal nature of propensities cannot be adequately represented by standard probability theory. In the second section a full version of the argument will be given so that the assumptions underlying the informal account can be precisely identified. The third section examines those assumptions and deals with objections that could be raised against the argument and its conclusion. The fourth and final section draws out some rather more general consequences of accepting the main argument. Those who find the first section sufficiently persuasive by itself may wish to go immediately to the final section, returning thereafter to the second and third sections as necessary.

Section I. The Informal Argument

Consider first a traditional deterministic disposition, such as the disposition for a glass window to shatter when struck by a heavy object. Given slightly idealized circumstances, the window is certain to break when hit by a rock, and this manifestation of the disposition is displayed whenever the appropriate conditions are present. Such deterministic dispositions are, however, often asymmetric. The window has no disposition to be hit by a rock when broken, and similarly, whatever disposition there is for the air temperature to go above 80°F is unaffected by whether my neighbor loses his temper, even though the converse influence is certainly there. The reason for this asymmetry is that many dispositions are intimately connected with causal relationships, and as a result they often possess the asymmetry of that latter relationship. Thus we might expect propensities, as particular kinds of dispositions, to possess a similar asymmetry and indeed they do, although because propensities come in degrees, the situation is understandably somewhat different.

The point can be illustrated by means of a simple scientific example. When light with a frequency greater than some threshold value falls on a metal plate, electrons are emitted by the photoelectric effect. Whether or not a particular electron is emitted is an indeterministic matter, and hence we can claim that there is a propensity p for an electron in the metal to be emitted, conditional upon the metal being exposed to light above the threshold frequency. Is there a corresponding propensity for the metal to be exposed to such light, conditional on an electron being emitted, and if so, what is its value? Probability theory provides an answer to this question if we identify conditional propensities with conditional probabilities. The answer is simple—calculate the inverse probability from the conditional probability. Yet it is just this answer which is incorrect for propensities and the reason is easy to see. The propensity for the metal to be exposed to radiation above the threshold frequency, conditional upon an electron being emitted, is equal to the unconditional propensity for the metal to be exposed to such radiation, because whether or not the conditioning factor occurs in this case cannot affect the propensity value for that latter event to occur. That is, with the obvious interpretation of the notation,

$Pr(R/\bar{E}) = Pr(R/E) = Pr(R)$. However, any use of inverse probability theorems from standard probability theory will require that $P(R/E) = P(E/R)P(R)/P(E)$ and if $P(E/R) \neq P(E)$, we shall have $P(R/E) \neq P(R)$. In this case, because of the influence of the radiation on the propensity for emission, the first inequality is true, but the lack of reverse influence makes the second inequality false for propensities. To take another example, heavy cigarette smoking increases the propensity for lung cancer, whereas the presence of (undiscovered) lung cancer has no effect on the propensity to smoke, and a similar probability calculation would give an incorrect result. Many other examples can obviously be given.

Thus a necessary condition for probability theory to provide the correct answer for conditional propensities is that any influence on the propensity which is present in one direction must also be present in the other. Yet it is just this symmetry which is lacking in most propensities. We can hence draw this conclusion from our informal argument: the properties of conditional propensities are not correctly represented by the standard theory of conditional probability; in particular any result involving inverse probabilities, including Bayes' Theorem, will, except in special cases, give incorrect results.

This short argument needs refinement, and so I turn to a fuller version which has a structure similar to the one just given but which is, of necessity, somewhat more complex.

Section II. The Detailed Argument

Any standard axiomatic system for conditional probability[1] will contain this multiplication principle:

$$\text{(MP)} \quad P(AB/C) = P(A/BC)P(B/C) = P(B/AC)P(A/C) = P(BA/C)$$

I emphasize here that this relationship appears not only as a direct consequence of the traditional definition of conditional probability,

[1] I take standard axiom systems for conditional probability to be those containing at least axioms of additivity, normalization, non-negativity, and the multiplication principle.

viz P(A/B) = P(AB)/P(B) but also as an axiom in probability calculi which take conditional probability as a primitive relation.[2] If we assume also the additivity axiom for conditional probabilities:

(Add) If A and B are disjoint, then P(A v B/C) = P(A/C) + P(B/C)

then as an easy consequence we have the theorem on total probability for binary events:

(TP) $P(A/C) = P(A/BC)P(B/C) + P(A/\bar{B}C)P(\bar{B}/C)$

and also Bayes' Theorem for binary events:

(BT) $P(B/AC) = P(A/BC)P(B/C)/[P(A/BC)P(B/C) + P(A/\bar{B}C)P(\bar{B}/C)]$

I note here for future reference that the only additional assumption needed to derive these second two from the first two is distributivity.

Consider now the conditional propensity function Pr(A/B), the propensity for A to occur, conditional on the occurrence of B.[3] This propensity will be interpreted initially as a single case propensity, where A and B are specific instances of event types, but nothing that is said here entails that either A or B has actually occurred or will occur. Dispositions being relatively permanent properties, they can be attributed to a system irrespective of whether the test condition, B, or the display, A, actually occurs. I shall assume throughout that the specific system which possesses the propensity remains the same, and hence omit notational devices representing the system or the structural basis of the propensity. Propensities are, however, often time-dependent, and so a fuller notation Pr_{t_i}

[2]For example, K. Popper, *The Logic of Scientific Discovery* (London: Hutchinson and Company, 1959), Appendix*iv,*v; R. Stalnaker, "Probability and Conditionals", *Philosophy of Science 37* (1970), pp. 64–80, especially p. 70.

[3]Throughout this paper, the notation 'P' will denote probability, and 'Pr' propensity.

(A_{tj}/B_{tk}) is needed, interpreted as "the propensity at t_i for A to occur at t_j, conditional upon B occurring at t_k." I shall now show that both the multiplication principle and Bayes' Theorem fail for conditional propensities. A specific example will be referred to for illustrative purposes, but the argument could be given for any case which possesses the kind of asymmetry present in the particular example. Take, then, the case of a well-known physical phenomenon, the transmission and reflection of photons from a half-silvered mirror. A source of spontaneously emitted photons allows the particles to impinge upon the mirror, but the system is so arranged that not all the photons emitted from the source.hit the mirror, and it is sufficiently isolated that only the factors explicitly mentioned here are relevant. Let I_{t2} be the event of a photon impinging upon the mirror at time t_2, and let T_{t3} be the event of a photon being transmitted through the mirror at time t_3 later than t_2. Now consider the single-case conditional propensity Pr_{t1} (./.) where t_1 is earlier than t_2, and take these assignments of propensity values:

i) $Pr_{t1}(T_{t3}/I_{t2}B_{t1}) = p > 0$

ii) $1 > Pr_{t1}(I_{t2}/B_{t1}) = q > 0$

iii) $Pr_{t1}(T_{t3}/\bar{I}_{t2}B_{t1}) = 0$

where, to avoid concerns about maximal specificity, each propensity is conditioned on a complete set of background conditions B_{t1} which include the fact that a photon was emitted from the source at t_0, which is no later than t_1. The parameters p and q can have any values within the limits prescribed. We need one further assumption for the argument. It is:

(CI) $Pr_{t1}(I_{t2}/T_{t3}B_{t1}) = Pr_{t1}(I_{t2}/\bar{T}_{t3}B_{t1}) = Pr_{t1}(I_{t2}/B_{t1})$

That is, the propensity for a particle to impinge upon the mirror is unaffected by whether the particle is transmitted or not. This assumption plays a crucial role in the argument, and will be discussed in the next section.

Argument 1: MP fails for propensities

From TP we have

$$\Pr_{t1}(T_{t3}/B_{t1}) = \Pr_{t1}(T_{t3}/I_{t2}B_{t1})\Pr_{t1}(I_{t2}/B_{t1}) + \Pr_{t1}(T_{t3}/\bar{I}_{t2}B_{t1})$$
$$\Pr_{t1}(\bar{I}_{t2}/B_{t1})$$

and substituting in the values of the propensities from i),ii),iii) above,

$$\Pr_{t1}(T_{t3}/B_{t1}) = pq + 0 = pq$$

From CI we have $\Pr_{t1}(I_{t2}/T_{t3}B_{t1}) = \Pr_{t1}(I_{t2}/B_{t1}) = q$

Hence using MP we have

$$\Pr_{t1}(I_{t2}T_{t3}/B_{t1}) = \Pr_{t1}(I_{t2}/T_{t3}B_{t1})\ \Pr_{t1}(T_{t3}/B_{t1}) = pq^2$$

But from MP directly we have

$$\Pr_{t1}(I_{t2}T_{t3}/B_{t1}) = \Pr_{t1}(T_{t3}I_{t2}/B_{t1}) = \Pr_{t1}(T_{t3}/I_{t2}B_{t1})\Pr_{t1}(I_{t2}/B_{t1})$$
$$= pq$$

We thus have

$$pq^2 = pq$$

i.e. $p = 0$, $q = 0$, or $q = 1$, which is inconsistent with i) or with ii).

Argument 2: Bayes Theorem fails for propensities

Take as assumptions BT and i),ii),iii) above. Then substituting in those values to BT we have

$$\Pr_{t1}(I_{t2}/T_{t3}B_{t1}) = pq/[pq + 0] = 1$$

But from CI we have

$$\Pr_{t1}(I_{t2}/T_{t3}B_{t1}) = \Pr_{t1}(I_{t2}/B_{t1}) = q < 1.$$

These arguments clearly suggest that inversion theorems of the classical probability calculus are inapplicable in a straightforward way to propensities. I shall now consider some of the most important ways which might be suggested for avoiding the arguments given above.

SECTION III. OBJECTIONS, REPLIES, AND DISCUSSION

Objection: The argument depends crucially upon the assumption CI. Rejecting a substantial part of classical probability theory is too great a price to pay, and hence we should abandon CI.

Reply. It is clearly not enough to rely upon the intuitive plausibility of CI. That principle can, however, be justified directly in the following way. The particle has a certain propensity within the given system to impinge upon the mirror. Suppose that we were to manipulate the system's conditions so that no particle hitting the mirror was in fact transmitted, say by rendering opaque the rear of the mirror. Would that alter the propensity for the particle to impinge upon the mirror? Given what we know about such systems, it clearly would not, and we could, if desired, support that claim by showing that the relative frequency of particles impinging on the mirror was unaffected by manipulations in the conditioning factor T when all other factors were kept constant as far as possible. Similarly, were we to manipulate the conditions so that all particles hitting the mirror were transmitted, say by rendering the mirror transparent, this too would leave the propensity for impinging unaltered. Given these facts, the events T_{t3} and $\bar{T}_{t3}$ are irrelevant to the propensity for I_{t2}, and they can be omitted from the factors upon which the propensity is conditioned without altering its value. Some further remarks are required here. It is essential not to impose an epistemological interpretation on CI. It is undoubtedly true that in our example transmission of the particle is *evidence for* the earlier incidence of the particle on the mirror, but we are not concerned with evidential connections, nor with any other epistemological relationships. The conditional propensity constitutes an objective relationship between two events and any increase in our information about one when we learn of the other is a completely separate matter. The tendency to interpret CI evidentially must therefore be resisted. Nor should we think of CI in terms of the relative frequencies with which one event is accompanied by another. Propensity values can, in many cases, be measured by relative frequencies, but the essence of a propensity account is that it puts primary emphasis upon the system and conditions which generate the frequencies and only secondarily upon the frequencies themselves. The issues of interest for a propensity calculus are not ones stemming from the passive observation of frequencies, but the activist ones of which frequency values remain unchanged under actual or hypothetical experimental interventions. No distinction is made within frequency interpretations of probability theory between mere associations of events and genuine causal connections, but this distinction is critical for propensities and cannot be ignored.

One final point needs to be discussed in this connection. In order to avoid having to justify the assumption CI for each case individually, we might want to refer to a general principle of the form:

(CI′) If Y is causally independent of X,
then $\Pr(Y/XZ) = \Pr(Y/Z)$ for all Z.

My own view is that such a general principle can be justified and used in place of the special assumption CI. To do this would, however, require a lengthy excursion into some controversial issues in probabilistic causality which are not central to the point under discussion here. In particular, it would require a general justification of a variational account of causation which is applicable to indeterministic systems. I am confident that the argument given above in favor of CI is sufficiently compelling for our present purposes, and so I shall remain with it.

Objection. The asymmetry present in the example is due to temporal asymmetry and is not therefore a property of the propensities themselves.

Reply. It is true that it is difficult to separate the asymmetry of single-case propensities from the asymmetry of temporally ordered events. However, a precisely similar argument to that of Section II can be given for propensities having event types as relata, and within which no temporal ordering occurs essentially. Consider the example mentioned earlier of the neighbor who harangues his wife on hot summer days. If we let T = tirade at wife and I = intensely hot day, where now no temporal subscripts are required, and retain the propensity assignments (i), (ii), and (iii) of Section II, then it is possible to repeat the arguments of that section *mutatis mutandis,* and show that the multiplication principle and Bayes' Theorem fail for general propensities as well. The failures thus clearly stem from the nature of propensities and not from the nature of time.

This response also shows that one cannot avoid the argument by insisting that it is meaningless or inadmissible to condition upon future events. For that objection would not dispose of the argument as applied to general propensities which are not temporally dependent. Furthermore, for temporally dependent single-case propensities, given any meaningful propensity assertion under this view which is conditioned only upon earlier events, there will exist an application of Bayes' Theorem, and an application of the multiplication axiom, which take that meaningful propensity assertion and transform it into a meaningless claim. Indeed, any application of Bayes' Theorem to temporally ordered events will fail the meaning-preservation criterion, and the restriction of probability theory required to satisfy that criterion would eliminate use of the theorem entirely for single-case propensities.

Objection. The problem lies with the use of conditional probabilities P(B/A) to represent propensities. Instead probability conditionals of the form P(A → B) should be used. As we know,[4] the two behave differently outside trivial cases, and so the fault lies in the mode of representation and not in the probability calculus.

Reply. This response can, I think, best be construed as a positive suggestion for an alternative approach to representing propensities. For example, some versions of causal decision theory have used the difference between conditional probabilities and the probability of conditionals to avoid Newcomb problems, by invoking a

[4]See D. Lewis, "The Probability of Conditionals and Conditional Probabilities," *The Philosophical Review 85* (1976), pp. 297–315, in particular pp. 300–302.

principle of causal independence which is similar to CI′ above, so that when A has no causal influence on B, $P(A \rightarrow B) = P(B)$.[5] If such an approach is taken, however, it would have to be sharply separated from the subjectivist interpretations of the probability function with which it is usually associated, for as I construe them, propensity values are objective properties of physical and social systems.[6] Because the properties of conditional propensities are so intimately connected with those of probabilistic causality, and there is currently available no comprehensive theory of the latter for the singular case, I am unfortunately unable at present to offer a positive account of the nature of conditional propensities.

Discussion. How do we arrive at the propensity assignments i), ii), and iii)? Because the argument depends only upon whether the propensities do or do not have extremal values, we can invoke the following two special principles both of which appear to be correct for single case propensities, (although each would be subject to measure-theoretic nuances within a Kolmogorovian framework). The first principle is: if an instance $X_{t'}$ of an event type X never occurs with an instance Y_t of an event type Y, then the conditional propensity of $X_{t'}$ conditional on Y_t is zero, for any such pair of

[5]For example, A Gibbard and W. Harper, "Counterfactuals and Two Kinds of Expected Utility," pp. 125–162 in *Foundations and Applications of Decision Theory, Volume 1,* C. Hooker, J. Leach, and E. McClennen, eds., (Dordrecht: D. Reidel 1978).

[6]David Lewis, in his "A Subjectivist's Guide to Objective Chance" in *Studies in Inductive Logic and Probability, Volume 2,* R. C. Jeffrey, ed., (Berkeley: University of California Press, 1980), has provided what is probably the most fully developed theory relating chance and credence. One brief point should be made in connection with Lewis' theory. For him, chance is credence objectified and (hence) chance obeys the laws of probability theory. Conditional chance is then defined in the usual manner. This entails, I believe, that such an account of chance based on subjective probabilities cannot capture the causal aspects of conditional propensities, even with the restrictions of admissible evidence imposed by Lewis. I certainly do not want to claim that the very rough sketch I have provided here of propensities is the only one possible, but it does suggest that carrying over the properties of subjective probability to chances will result in certain characteristic features of the latter being lost. A similar point can be made about the suggestion that we can define an absolute propensity measure as c(A $=_{df}$ Pr(A/T), where T is any certain event, and then define a conditional probability measure in the usual way using c.

instances. The second principle is: if an instance of an event type X occurs together with an instance of an event type Y, and an instance of event type Y occurs without an instance of event type X, then the propensity for $X_{t'}$ conditional on Y_t lies strictly between zero and one, for any such pair of instances. (Both principles assume that all other background factors have been conditioned into Pr(./.).) The first principle secures iii), the second principle secures ii), and the first half of the second principle secures i).

Would it be possible to reject some assumption other than CI and preserve MP and BT? The only other candidates are finite additivity and distributivity (which is needed to derive TP and BT.) Although there are well-known reasons for doubting the universal application of distributivity to quantum probabilities there is, I think, no good reason for supposing that it fails for propensities in general. The failure of finite additivity would be as conclusive a reason as the failure of the multiplication axiom to reject the classical probability calculus, and its failure would merely compound the difficulties for the traditional theory. However, the argument given above is so clearly directed against inversion principles that any considerations involving other parts of the calculus seem to be quite separate. The account thus ought not to be viewed as a pragmatic argument based on considerations of simplicity or convenience, but as showing directly the falsity of the multiplication principle and Bayes' theorem.

It is perhaps ironic that the first fully general version of Bayes' Theorem was formulated by Laplace in order to calculate the probability of various causes which may have given rise to an observed effect.[7] Laplace was concerned with legitimizing a probabilistic version of Newtonian induction, of inferring causes from their effects, and given his deterministic views, only an epistemic interpretation of the theorem made sense for him. But when our concern is with objective chance, such inductive interests are of secondary importance, and once the metaphysical aspects of chance are separated from the epistemological, Laplace's interpretation no longer seems quite so compelling.

[7]In Pierre Simon, Marquis de Laplace, *Theorie Analytique des Probabilities* (Paris: Courcier, 1820) (3rd Edition), pp. 183–184.

SECTION IV. CONSEQUENCES

What is the epistemological status of probability theory? It seems to occupy a peculiar position somewhere between the purely mathematical and the obviously scientific. The subject matter of the theory, if matter there be, has been identified with, among other things, finite class frequencies, degrees of rational belief, limiting relative frequencies, propensities, degrees of logical confirmation, and measures on abstract spaces, to name only some of the most important. This diversity of interpretations has been matched by the range of views on the nature of the theory itself. It has been taken as a generalization of classical logic, as an abstract mathematical theory, as an empirical scientific theory, as a theory of inference perhaps distinct from but certainly underpinning the theory of statistical inference, as a theory of normative rationality, as the source of models for irregular phenomena, as an interpretative theory for certain parameters in scientific theories, as the basis for an analysis of causality, and as the reference point for definitions of randomness. Yet underlying this remarkable range of views is an equally remarkable agreement about the correct structure of the calculus itself. In particular, empiricists and rationalists may differ about the source of the probability values used in applications of the theory, but there is little disagreement about the truth of the theory—indeed, it would not be an exaggeration to say that the theory of probability is commonly regarded as though it were necessarily true.

If the arguments given in the first three sections of this paper are correct, this perception of probability theory is profoundly mistaken. It is thus worth recalling how it arose. Historically, the success of Kolmogorov's axiomatization, published in German in 1933, quickly eclipsed for scientific purposes Reichenbach's axiomatization of 1932 and the frequency theories of von Mises and of Popper, published in 1928 and 1934, respectively. Philosophically, the hegemony of standard probability theory has been reinforced by its affinities with logic. The view that probability theory is an extension of classical logic was adopted by Bolzano, Boole, Venn, Lukasiewicz, Reichenbach, Carnap, and Popper, and has been supported by results showing that, in some cases, the logical structure of the probability space can be derived from the axioms of probability theory, indicating that classical sentential logic is a special case of the structure imposed upon propositions by the theory of probability.[8] This, together with the application of the theory in a manner seemingly independent of subject matter, reinforces the conception that the theory has an epistemological status akin to that of logic. Hence one arrives at the position that the correct way to utilize probability theory within science is to first separately axiomatize a purely formal theory of probability, and non-probabilistic axioms for specific scientific theories can then be added to this fixed set of probability axioms in exactly the same way that non-logical axioms are standardly added to logical axioms or rules.

This approach naturally leads to the project of "providing an interpretation for probability theory" and the widespread use of the criterion of admissibility as a condition of adequacy for any interpretation of the theory. The criterion asserts that in order to be acceptable as an interpretation of the term 'probability', at least within scientific contexts, the interpretation must satisfy a standard set of axioms of abstract probability theory or a close variant thereof. This approach of considering 'probability' as a primitive term to be interpreted by means of an implicit definition is now so widespread as to be considered mandatory for any new account of probability, to the extent that we tend to automatically lapse into calling such accounts new interpretations rather than new theories of probability.

It is time, I believe, to give up the criterion of admissibility. We have seen that it places an unreasonable demand upon one plausible construal of propensities. Add to this the facts that limiting relative frequencies violate the axiom of countable additivity and that their probability spaces are not sigma-fields unless further

[8]See, for example, K. Popper, op. cit., and H. Leblanc, "On Requirements for Conditional Probability Functions," *Journal of Symbolic Logic 25* (1960), pp. 238–242. It should be noted that Popper is somewhat ambiguous about the status of these results, for having asserted earlier that his calculus "... is formal; that is to say, it does not assume any particular interpretation, although allowing for at least all known interpretations" (ibid, p. 326) he then qualifies the results with "in its logical interpretation, the probability calculus is a genuine generalization of the logic of derivation" (ibid, p. 356).

constraints are added; that rational degrees of belief, according to some accounts, are not and cannot sensibly be required to be countably additive; and that there is serious doubt as to whether the traditional theory of probability is the correct account for use in quantum theory. Then the project of constraining semantics by syntax begins to look quite implausible in this area.[9] I do not wish to deny that the project of axiomatizing probability theory has had an enormously clarifying effect upon investigations into probability. What I do deny is that the concept of chance, as represented by propensities, is so obscure, or so abstract, that its properties are accessible only by means of a theory whose origins in equipossible outcomes and finite frequencies can all too easily be forgotten.[10]

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[9]For a discussion of the flaws in the relative frequency interpretation, and a suggested repair, see B. van Fraassen, *The Scientific Image* (Oxford: The Clarendon Press, 1980), Chapter 6, section 4; on subjective probability see B. de Finetti "On the Axiomatization of Probability Theory" in his *Probability, Induction, and Statistics* (New York: Wiley and Sons, 1972), and for the quantum-theoretical case, see A. Fine "Probability and the Interpretation of Quantum Mechanics" *British Journal for the Philosophy of Science 24* (1973), pp. 1–37, for a discussion and dissenting view.

[10]This paper has benefitted greatly from the comments and criticisms of Robert Almeder, James Cargile, James Fetzer, Ronald Giere, Donald Gillies, Clark Glymour, Richard Otte, Sir Karl Popper, Wesley Salmon, Robert Stalnaker, and an anonymous referee for this Journal. It should not be assumed that they endorse the conclusions which I have drawn above. Brief mention of the main point can be found in my paper "Is 'Physical Randomness' Just Indeterminism in Disguise?" in *PSA 1978, Volume 2,* P. Asquith and I. Hacking, eds., Philosophy of Science Association, East Lansing, 1981, p. 102, and early responses are in W. Salmon "Propensities: A Discussion Review," *Erkenntnis 14* (1979), pp. 213–214; and J. Fetzer, *Scientific Knowledge* (Dordrecht: D. Reidel, 1981), pp. 283–286.