# PROBABILISTIC CAUSALITY

BY

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ALTHOUGH many philosophers would be likely to brand the phrase "probabilistic causality" as a blatant solecism, embodying serious conceptual confusion, it seems to me that probabilistic causal concepts are used in innumerable contexts of everyday life and science. We hear that various substances are known to cause cancer in laboratory animals—see the label on your favorite diet soft-drink can—even though there is no presumption that every laboratory animal exposed to the substance developed any malignancy. We say that a skid on a patch of ice was the cause of an automobile accident, though many cars passed over the slick spot, some of them skidding upon it, without mishap. We have strong evidence that exposure to even low levels of radiation can cause leukemia, though only a small percentage of those who are so exposed actually develop leukemia. I sometimes complain of gastric distress as a result of eating very spicy food, but such discomfort is by no means a universal sequel to well-seasoned Mexican cuisine. It may be maintained, of course, that in all such cases a fully detailed account would furnish invariable cause-effect relations, but this claim would amount to no more than a declaration of faith. As Patrick Suppes has ably argued, it is as pointless as it is unjustified (1970, pp. 7–8).

There are, in the philosophical literature, three attempts to provide theories of probabilistic causality; Hans Reichenbach (1956), I. J. Good (1961–62), and Patrick Suppes (1970) have offered reasonably systematic treatments.[1] In the vast philosophical literature on causality they are largely ignored. Moreover, Suppes makes no mention of Reichenbach's (1956), and Good gives it only the slightest note (II, p. 45)[2], though both offer brief critical remarks on some of his earlier work. Suppes makes the following passing reference to Good's theory: "After working out most of the details of the definitions given here in lectures at Stanford, I discovered that a closely related analysis of causality had been given in an interesting series of articles by I. J. Good (1961, 1962), and the reader is urged to look at Good's articles for a development similar to the one given here, although worked out in rather different fashion formally and from a different viewpoint." (1970, p. 11) Even amongst those who have done constructive work on probabilistic causality, there is no sustained discussion of the three important extant theories.

The aim of the present article is to take a close critical look at the proposals of Good, Reichenbach, and Suppes. Each of the three is, for reasons which I shall attempt to spell out in detail, seriously flawed. We shall find, I believe, that the difficulties arise from certain rather plausible assumptions about probabilistic causality, and that the objections lead to some rather surprising general results. In the concluding section, I shall briefly sketch what seem to me the appropriate ways of

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circumventing the problems associated with these three theories of probabilistic causality.

## *1. Good's Causal Calculus*

Among the three theories of probabilistic causality, Good's appears to be the least familiar to philosophers. One reason for this neglect—over and above the fact that most philosophers ignore the very concept of probabilistic causality—may be the rather forbidding mathematical style of Good's presentation. Fortunately, the aspects of his theory which give rise to the fundamental objections can be extracted from the heavy formalism and presented in a fashion which makes them intuitively easy to grasp. I shall offer two basic objections. The first objection concerns the manner in which Good attempts to assign a degree of strength to a causal chain on the basis of the strengths of the individual links in the chain. He seems to be unaware of any problem in this connection. The second objection, which Good's theory shares with those of Reichenbach and Suppes, concerns cases in which an effect is brought about in an improbable fashion. Both Good and Suppes are aware of this rather familiar difficulty, and they try to deal with it in ways which are different but complementary. I shall argue that their answers are inadequate.

The basic materials with which we shall work are familiar enough. We suppose that there are aggregates of events, denoted by $E, F, G, \ldots$ (with or without subscripts), among which certain physical probability relations hold.[3] The particular events are located in space-time. Like Suppes, but unlike Reichenbach, Good stipulates that causes temporally precede their effects—that is, temporal priority is used to define causal priority, not vice-versa. Good's aim is to examine certain types of networks of events which join an initial event $F$ to a final event $E$, usually by way of various intermediate events $G_i$, and to define a measure $\chi(E{:}F)$ of "the degree to which $F$ caused $E$" or "the contribution to the causation of $E$ provided by $F$" (I, p. 307). The specification of this measure, and various related measures, involves 24 axioms and 18 theorems, some of which are relatively abstruse.

One particularly important special case of a causal net is a *causal chain*. In a causal chain, all of the constituent events $F = F_0, F_1, F_2, \ldots, F_n = E$ are linearly ordered. It is assumed that the adjacent events $F_i$ and $F_{i+1}$ are spatio-temporally contiguous (or approximately so), that they do not overlap too much, and that $F_{i+1}$ does not depend upon the occurrence of any event in the chain prior to $F_i$ (II, p. 45). In order to arrive at the measure $\chi$ for a wider class of nets, Good defines a measure $S(E{:}F)$ of the strength of the causal chain joining $F$ to $E$. A particularly simple type of causal chain is one consisting only of the two events $F$ and $E$. A measure of the strength of a chain of this sort can be used, according to Good, to define a measure of the strength of longer chains. It is this aspect of Good's approach—the attempt to compound the strengths of the individual "links" of the chain in order to ascertain the strength of the entire chain—which is the locus of the first problem.

In order to initiate the enterprise, Good introduces a measure $Q(E{:}F)$ which is to stand for "the tendency of $F$ to cause $E$" (I, p. 307). This informal rendering in words of the import of $Q$ is, I think, seriously misleading, for $Q$ turns out to be no

more nor less than a measure of statistical relevance. Events of type $A$ are statistically relevant to events of type $B$, it will be recalled, if the occurrence of an event of type $A$ makes a difference to the probability that an event of type $B$ will occur. We say that the relation is one of positive relevance if the occurrence of a member of $A$ increases the probability that a member of $B$ will occur; we speak of negative relevance if the occurrence of $A$ decreases the probability of $B$. As we all recognize, a mere correlation does not necessarily constitute a causal relation—not even a tendency to cause. The falling barometric reading has no tendency at all to cause a storm, though the barometric reading is highly relevant statistically to the onset of stormy weather.

There are, of course, many different measures of statistical relevance. Although the first five axioms, A1–A5, do not fix the precise form of $Q$, they do show what sort of measure it is. According to A1, $Q(E{:}F)$ is a function of $P(E)$, $P(E|F)$, and $P(E|\bar{F})$ alone; according to A5, $Q(E{:}F)$ has the same sign as $P(E|F) - P(E)$; and according to A3 and A4, $Q(E{:}F)$ increases continuously with $P(E|F)$ if $P(E)$ and $P(E|\bar{F})$ are held constant, and it decreases continuously as $P(E|\bar{F})$ increases if $P(E)$ and $P(E|F)$ are held constant. $Q$ may be a real number, it may assume the value $+\infty$ or $-\infty$, or under special circumstances it may be indeterminate. However, $Q$ need not be the simplest sort of statistical relevance measure—such as $P(E|F) - P(E)$, $P(E|F)/P(E)$, $P(E|F) - P(E|\bar{F})$, or $P(E|F)/P(E|\bar{F})$—for A1–5 allow that it may be a function of all three of the above-mentioned probabilities. When Good does choose a particular form for $Q(E{:}F)$, however, he adopts one which is a function of $P(E|F)$ and $P(E|\bar{F})$, but which is independent of $P(E)$ (I, pp. 316–317). Suppes, in his definition of *prima facie cause* (1970, p. 12), and Reichenbach, in his definitions of *causal betweenness* (1956, p. 190) and *conjunctive fork* (*ibid.*, p. 159), use relevance measures which are functions of $P(E|F)$ and $P(E)$.[4] What is important in this context is that every theory of probabilistic causality employs a statistical relevance measure as a basic concept; for present purposes, the precise mathematical form which is chosen is of secondary significance. Good's relevance measure $Q(E{:}F)$ is used to furnish the strength $S(E{:}F)$ of the chain which consists only of the events $F$ and $E$ (A10; I, p. 311).

The next problem is to characterize the strength of the causal chain from $F$ to $E$ when there are other intermediate events in the chain between $F$ and $E$. We know that the strength of each link in the chain, $F_i \rightarrow F_{i+1}$, is simply $Q(F_{i+1}{:}F_i)$, and that $Q$ is a statistical relevance measure. The trick is to figure out how—if it is possible to do so at all—to compound the strengths of the individual links $s_0, s_1, \ldots, s_{n-1}$ so as to get the strength of the chain itself. Good proceeds (A11; I, p. 311) to make the simplest reasonable assumption, namely, that the strength $S(E{:}F)$ of the chain is some function $\phi(s_0, s_1, \ldots, s_{n-1})$ of the strengths of the individual links. Although brief justificatory remarks accompany some of the axioms, this one has none; perhaps Good regarded it as too obvious to need comment. In spite of its initial plausibility, this assumption seems to me untenable, as I shall now try to show by means of a simple counter-example.

Consider the following simple two-stage game. The player first tosses a fair tetrahedron with sides marked 1, 2, 3, and 4. If the tetrahedron comes to rest on any side other than 4—i.e., if side 4 shows after the toss—the player draws from a deck which contains 16 cards, 12 of which are red and 4 of which are black; if side 4 does not show, he draws from a deck containing 4 red and 12 black. On a given play, the player tosses the tetrahedron (event $F$) and it comes to rest with side 4 showing, so that the player draws from the first above-mentioned special deck (event $G$), with the result that he gets a red card (event $E$). This is a simple three-event chain,[5] and all of the constituents are events which actually obtain, as Good demands (II, p. 45). We inquire about the degree to which $F$ caused $E$.



In the first place, we must construe the situation in such a way that $F$ is positively relevant to $G$ and $G$ is positively relevant to $E$; otherwise, as Good shows in theorem T2 (I, p. 311), there is no causal chain. Let us therefore assume that $P(G|\bar{F}) = 0$; that is, the only way to get a chance to draw from the special deck is to enter the game and toss the tetrahedron. Thus, $P(G|F) > P(G|\bar{F})$ and $Q(G{:}F) \neq 0$. We can now use the theorem on total probability

$$P(E|F) = P(G|F)\cdot P(E|F.G) + P(\bar{G}|F)\cdot P(E|F.\bar{G}) \tag{1}$$

to calculate the probability of drawing a red card (event $E$) given that the player has tossed the tetrahedron (event $F$). Since causal chains, as defined by Good, possess the Markov property,

$$P(E|G) = P(E|F.G) \text{ and } P(E|\bar{G}) = P(E|F.\bar{G}), \tag{2}$$

the theorem on total probability can be rewritten in a simplified form:

$$P(E|F) = P(G|F)\cdot P(E|G) + P(\bar{G}|F)\cdot P(E|\bar{G}). \tag{3}$$

Using this equation and the stipulated probability values, we find that $P(E|F) = {}^{10}/_{16}$.

For purposes of comparison, let us consider another game which is just like the foregoing except that different decks of cards are used. The player tosses a fair tetrahedron (event $F'$) and if side 4 shows he draws from a deck containing 14 red cards and 2 black cards (event $G'$). If the tetrahedron comes to rest on side 4, the player draws from a deck containing 10 red cards and 6 black cards (event $\bar{G}'$). In this game, the probability of drawing a red card (event $E'$) equals ${}^{11}/_{16}$. It is easily seen that, in this game, as in the other, the toss of the tetrahedron is positively relevant to the draw from the favored deck, and the draw from that deck is positively relevant to getting a red card. In Good's notation, $Q(G'{:}F') > 0$ and $Q(E'{:}G') > 0$. Assume that a player of the second game had tossed the tetrahedron with the result that side 4 shows, and that he has drawn a red card from the favored deck. We have two causal chains: $F \rightarrow G \rightarrow E$ (first game) and $F' \rightarrow G' \rightarrow E'$ (second game). Let us compare them.

We must now take account of the particular form $Q$ assumes in Good's causal calculus; it is given in T15 (I, p. 317) as

$$Q(E{:}G) = \log \frac{P(\bar{E}|\bar{G})}{P(\bar{E}|G)} = \log \frac{1 - P(E|\bar{G})}{1 - P(E|G)} \tag{4}$$

In the first game, $P(\bar{E}|\bar{G}) = {}^3/_4$ and $P(\bar{E}|G) = {}^1/_4$; hence, $Q(E{:}G) = \log 3$. In the second game, $P(\bar{E}'|\bar{G}') = {}^3/_8$ and $P(\bar{E}'|G') = {}^1/_8$; thus $Q(E'{:}G') = \log 3$. Clearly

$Q(G:F) = Q(G:F')$, since $P(G|F) = P(G'|F')$ and $P(G|\overline{F}) = P(G'|\overline{F}')$. Therefore, the corresponding links in the two chains have equal strength. We have already noted, however, that $P(E|F) \neq P(E'|F')$—that is, the probability that a player who tosses the tetrahedron in the first game will draw a red card is not equal to the probability that a player who tosses the tetrahedron in the second game will draw a red card. It is easily seen, moreover, that the statistical relevance of $F$ to $E$ is not the same as the statistical relevance of $F'$ to $E'$. We begin by noting that the only way in which a red card can be drawn in either game is by a player who has commenced the game by tossing the tetrahedron; consequently, $P(E|\overline{F}) = P(E'|\overline{F}') = 0$. Using the previously established values, $P(E|F) = 10/16$ and $P(E'|F') = 13/16$, we find that $P(E|F) - P(E|\overline{F}) = 10/16$, while $P(E'|F') - P(E'|\overline{F}') = 13/16$. Given both the difference in probability and the difference in statistical relevance between the first and last members of the two chains, it seems strange to say that the causal strengths of the two chains are equal. If, however, $\phi$ is made a function of the $Q$-values of the individual links, this is the consequence we are forced to accept.[6]

In order to bring out the import of this argument, I should like to apply it to an example which is a bit less artificial and more concrete than the tetrahedron-cum-card game. Suppose that two individuals, Joe Doakes and Jane Bloggs, suffer from sexual disabilities. Joe is impotent and Jane is frigid. Each of them decides to seek psychotherapy. There are two alternative types of therapy available, directive or nondirective. When Joe seeks out a psychotherapist (event $F$), there is a probability of $3/4$ that he will select a directive therapist and undergo that type of treatment (event $G$), and a probability of $1/4$ that he will select a non-directive therapist and undergo that type of treatment (event $\overline{G}$). If he is treated by a directive therapist, there is a probability of $3/4$ that he will be cured (event $E$), and if he is treated by a non-directive therapist, there is a probability of $1/4$ that he will be cured. Given these values, there is a probability of $10/16$ that he will be cured, given that he undertakes psychotherapy.

When Jane seeks out a psychotherapist (event $F'$), there is a probability of $3/4$ that she will select a directive therapist (event $G'$), and a probability of $1/4$ that she will select a non-directive therapist (event $\overline{G}'$). If she is treated by a directive therapist, there is a probability of $7/8$ that she will be cured (event $E'$), and if she is treated by a non-directive therapist, the probability of a cure is $5/8$. Given these values, there is a probability of $13/16$ that she will be cured, given that she undertakes psychotherapy.

Joe and Jane each undertake psychotherapy, each is treated by a directive psychotherapist, and each is cured. Thus, we have two causal chains, $F \rightarrow G \rightarrow E$ and $F' \rightarrow G' \rightarrow E'$. We may assume, moreover, that both chains have the Markov property, formulated in equations (2) above, for it is reasonable to suppose that people who undergo psychotherapy on account of these problems do so voluntarily, so $G = F.G$ and $G' = F'.G'$. The question is, on what basis, if any, would we be warranted in claiming that the two chains have the same strength—i.e., that the degree to which the seeking out of psychotherapeutic treatment caused the cure is the same for both.

The appropriate response, it seems to me, is that not enough information has been given to answer the question about the relative strengths of the two chains. We need to know, at the very least, what the probability of a cure would be if



psychotherapy were not undertaken. We could, of course, make the patently unrealistic assumption that the probability of a cure in the absence of psychotherapy is zero in both cases. This assumption gives this psychotherapy example precisely the same probabilistic structure as the tetrahedron-cum-cards example. Given this assumption, it seems intuitively unreasonable to say that the psychotherapy contributed equally strongly to the cures in the two cases, for the degree of relevance of the cause to the effect differs in the two cases. If we make other assumptions —e.g., that there is quite a high probability (say $3/4$) of remission without psychotherapy in the case of frigidity, but a low probability (say $1/100$) of remission without psychotherapy in the case of impotence, then it seems intuitively clear that the causal contribution to the cure in the case of Joe is much greater than it is in the case of Jane. For Jane's problem, the probability of cure if she undergoes therapy ($13/16$) is only slightly higher than the probability of spontaneous remission ($12/16$), but for Joe's problem, the probability of a cure if he undergoes psychotherapy ($10/16$) is much greater than the probability of spontaneous remission ($1/100$). Other assumptions about spontaneous remission could alter the situation dramatically. In the light of these considerations, I am extremely skeptical about the possibility of arriving at a suitable measure of the strength $S$ of a causal chain in terms of any function of the $Q$-values of its individual links. I agree with Good that the strength of a causal chain cannot be measured in terms of the statistical relevance of the initial member $F$ to the final member $E$ alone, but I do not believe that this factor can be ignored. To attempt to determine the strength of the chain without comparing the probability of $E$ when $F$ is present with the probability of $E$ when $F$ is absent seems quite futile; we can evidently alter the strength of a causal chain by altering nothing about the chain except the probability $P(E|\overline{F})$.

In order to assign $Q$-values to the links of a chain $F \rightarrow G \rightarrow E$, we may use the following probability values (see equation (4)):

$$P(G|F),\ P(G|\overline{F}),\ P(E|G),\ P(E|\overline{G}). \tag{5}$$

Since causal chains are assumed to have the Markov property, we can use these values with equation (3) to compute the values of $P(E|F)$ and $P(E|\overline{F})$:

$$P(E|F) = P(G|F)\cdot P(E|G) + P(\overline{G}|F)\cdot P(E|\overline{G}) \tag{6}$$

$$P(E|\overline{F}) = P(G|\overline{F})\cdot P(E|G) + P(\overline{G}|\overline{F})\cdot P(E|\overline{G}) \tag{7}$$

As our examples have shown, the relation between $P(E|F)$ and $P(E|\overline{F})$ is not a function of the $Q$-values of the links of the chain. We must therefore conclude that the transition from the four basic probability values (5) to the two statistical relevance measures sacrifices information which is essential to the determination of the strength of the causal chain.

It is important to recognize that this consequence is *not* a result of the particular measure of statistical relevance adopted by Good; it is, instead, a result of rather general features of statistical relevance relations. If a relevance measure is defined as $P(B|A)/P(B|\overline{A})$, or as any function of that ratio, it is easy to construct counterexamples along precisely the same lines as those given above (simply by adjusting the make-ups of the two decks of cards in the game) to show that the relevance of

$A$ to $B$ and the relevance of $B$ to $C$ do not determine the relevance of $A$ to $C$. The same may be said for any relevance measure defined in terms of the difference $P(B|A) - P(B|\bar{A})$.

I obviously have not considered every possible form of statistical relevane relation, but it seems intuitively evident that the foregoing sorts of arguments can be applied to statistical relevance relations quite generally. Thus, at this point, I would conjecture the general proposition that the strengths of causal chains cannot be measured by the statistical relevance relations of their adjacent members alone. In this sense, it seems, *the strength of a causal chain cannot be a function of the strengths of its individual links*. It appears, again as a general conjecture, that one needs the individual probability values which are used to define the relevance measures in order to deal adequately with the strengths of the causal chains. Too much information is thrown away when these probability values are combined to form statistical relevance measures.

There is a second difficulty, which Good's theory shares with those of Suppes and Reichenbach. Suppose, going back to the first tetrahedron-cum-card game, that the player tosses the tetrahedron and it lands on side 4. He must draw from the deck which has a paucity of red cards; nevertheless, he draws a red card. According to Good's calculus, the three events $F$, $\bar{G}$, $E$ do not form a causal chain (II, p. 45). On *any* reasonable relevance measure, $\bar{G}$ is not positively relevant to $E$, for $P(E|\bar{G}) < P(E|G)$. This result seems to me to be untenable; if $F \rightarrow G \rightarrow E$ qualifies as a causal chain when this sequence of events occurs, so must $F \rightarrow \bar{G} \rightarrow E$ when it occurs. Good has an answer to this problem; we shall examine it in connection with the theories of Reichenbach and Suppes. [7]

## 2. *Reichenbach's Macrostatistical Theory*

Unlike Good and Suppes, who attempt to provide analyses of probabilistic causality for their own sake, Reichenbach develops his analysis as a part of his program of implementing a causal theory of time. Thus, in contrast to the other two authors, he does not build into his definitions the stipulation that causes are temporally prior to effects. Instead, he attempts to construct a theory of causal relations which will yield a causal asymmetry which can then be used to define a relation of temporal priority. Two of the key causal concepts introduced in this construction are the relation of *causal betweenness* and the structure known as a *conjunctive fork*. The main use of the betweenness relation is to establish a linear time order; the conjunctive fork is employed to impose a direction or asymmetry upon the linear time order. In the present discussion, I shall not attempt to evaluate the temporal ramifications of Reichenbach's theory; instead, I shall confine my attention to the adequacy of the causal concepts as such.

Reichenbach's formal definition of causal betweenness, translated from his notation into a standard notation, reads as follows (1956, p. 190):

An event $B$ is *causally between* the events $A$ and $C$ if the relations hold:

$$1 > P(C|B) > P(C \not\rightarrow A) > P(C) > 0 \tag{8}$$



$$1 > P(A|B) > P(A|C) > P(A) > 0 \tag{9}$$

$$P(C|A.B) = P(C|B) \tag{10}$$

Together with the principle of *local comparability of time order*, the relation of causal betweenness can, according to Reichenbach, be used to construct causal nets and chains similar to those mentioned by Good in his causal calculus. Unlike Good, however, Reichenbach does not attempt a quantitative characterization of the strengths of such chains and nets. It is worth noting that formulas (8) and (9) embody several statistical relevance relations: $A$ is relevant to the occurrence of $C$, but $B$ is more highly relevant to $C$; conversely, $C$ is relevant to the occurrence of $A$, but $B$ is more highly relevant to $A$. Moreover, according to (10), $B$ screens $A$ off from $C$ and $C$ off from $A$—that is, $B$ renders $A$ and $C$ statistically irrelevant to one another. A chain of events $A \rightarrow B \rightarrow C$ thus has the Markov property (equations (2) ) which Good demanded of his causal chains.

The inadequacy of Reichenbach's definition of causal betweenness was pointed out by Clark Glymour, in conversation, a number of years ago, when he was a graduate student at Indiana University. The cases we discussed at that time were similar in principle to an excellent example, due to Deborah Rosen, reported by Suppes (1970, p. 41):

> . . . suppose a golfer makes a shot that hits a limb of a tree close to the green and is thereby deflected directly into the hole, for a spectacular birdie. . . . If we know something about Mr. [sic] Jones' golf we can estimate the probability of his making a birdie on this particular hole. The probability will be low, but the seemingly disturbing thing is that if we estimate the conditional probability of his making a birdie, given that the ball hit the branch, . . . we would ordinarily estimate the probability as being still lower. Yet when we see the event happen, we recognize immediately that hitting the branch in exactly the way it did was essential to the ball's going into the cup.

If we let $A$ be the event of Jones teeing off, $B$ the event of the ball striking the tree limb, and $C$ the event of the ball dropping into the cup at one under par for the hole, we have a violation of Reichenbach's condition (8), for $P(C|B) < P(C|A)$. The event $B$ is, nevertheless, causally between events $A$ and $C$.[8] Various retorts can be made to this purported counter-example. One could maintain (see von Bretzel, 1977, p. 182) that sufficiently detailed information about the physical interaction between the ball and the branch might enable us to raise the conditional probability of the ball going into the hole, given these precisely specified physical circumstances, above the conditional probability of Jones making a birdie given only that he [sic] tees off. As von Bretzel himself notes, this kind of response seems ad hoc and artificial, and there is no good reason to suppose that it would take care of all such counterexamples even if it were adequate for this particular one. Indeed, it seems to me that many examples can be found which are immune to dismissal on these grounds.

Rosen's colorful example involves a near-miraculous occurrence, but we do not need to resort to such unusual happenings in order to find counterexamples to

Reichenbach's definition of causal betweenness. The crucial feature of Rosen's example is that Jones makes her birdie 'the hard way.' Since much which goes on in life happens 'the hard way,' we should be able to find an abundance of everyday sorts of counterexamples; in fact, we have already considered one. When the game of tetrahedron tossing and card drawing was used in the previous section to raise the second objection to Good's causal calculus, we looked at the case in which the player drew the red card and won the prize 'the hard way.' In that case the tetrahedron came to rest on side 4, forcing the player to draw from the deck with a smaller proportion of red cards. As the original game was set up, the player's initial probability of drawing a red card is 10/16, but if he is required, as a result of his toss, to draw from the less favorable deck, his probability of drawing a red card is only ¼. Nevertheless, when the player who tosses the tetrahedron fails to show side 4, but succeeds in drawing a red card from the unfavorable deck, the draw from the unfavorable deck is causally between the toss of the tetrahedron and the winning of the prize. Drawing a red card from a deck which contains 4 red and 12 black cards can hardly be considered a near-miracle.

Once we see the basic feature of such examples, we can find others in profusion. The expression, "the hard way," is used in the game of craps, and this game provides another obvious example.[9] The shooter wins if he throws 7 or 11 on the first toss; he loses if he throws 2, 3, or 12 on the first toss. If the first toss results in any other number, that is his 'point,' and he wins if in subsequent tosses he makes his point before he throws a 7. The probability of the shooter winning in one or another of these ways is just slightly less than one-half. A player who throws 4 on his initial toss clearly reduces his chances of winning (this conditional probability is ⅓), but nevertheless he can win by making his point. Throwing 4 is, however, causally between the initial toss and the winning of the bet on that play.

A pool player has an easy direct shot to sink the 9-ball, but he chooses, for the sake of his subsequent position, the much more difficult play of shooting at the 2-ball and using it to put the 9-ball in the pocket. The initial probability of his sinking the 9-ball is much greater than the probability of getting the 9-ball in the pocket if his cue-ball strikes the 2-ball, but the collision with the 2-ball is causally betweeen the initiation of the play and the dropping of the 9-ball into the pocket. Similar examples can obviously be found in an enormous variety of circumstances in which a given result can occur in more than one way, and in which the probabilities of the result differ widely given the various alternative ways of reaching it. The attempt to save Reichenbach's definition of causal betweenness by ad hoc devices appears to be a hopeless undertaking. We shall see, however, that Good suggests a method for handling such examples, and that Rosen offers a somewhat different defense on behalf of Suppes.

Reichenbach's definition of *conjunctive fork* does not fare much better. The basic motivation for introducing this concept is to characterize the situation in which an otherwise improbable coincidence is explained by appeal to a common cause. There are many familiar examples—e.g., the explanation of the simultaneous illness of many residents of a particular dormitory in terms of tainted food in a meal they all shared. Reichenbach defines the conjunctive fork in terms of the following formulas (1956, p. 159) which I have renumbered and translated into standard notation:



$$P(A.B|C) = P(A|C)\cdot P(B|C) \tag{11}$$

$$P(A.B|\overline{C}) = P(A|\overline{C})\cdot P(B|\overline{C}) \tag{12}$$

$$P(A|C) > P(A|\overline{C}) \tag{13}$$

$$P(B|C) > P(B|\overline{C}) \tag{14}$$

In order to apply these formulas to the foregoing example, we may let $A$ stand for the illness of Smith on the night in question, $B$ the illness of Jones on the same night, and $C$ the presence of spoiled food in the dinner served at their dormitory that evening.

The following example, due to Ellis Crasnow, shows the inadequacy of Reichenbach's formulation. Brown usually arrives at his office about 9:00 a.m., fixes himself a cup of coffee, and settles down to read the morning paper for half an hour before beginning any serious business. Upon occasion, however, he arrives at 8:00, and his secretary has already brewed a fresh pot of coffee, which she serves him immediately. On precisely the same occasions, some other person meets him at his office and they begin work quite promptly. This coincidence —the coffee being ready and the other person being at his office—demands explanation in terms of a common cause. As it happens, Brown usually takes the 8:30 bus to work in the morning, but on those mornings when the coffee is prepared for his arrival and the other person shows up, he takes the 7:30 bus. It can plausibly be argued that the three events, $A$ (the coffee being ready), $B$ (the other person showing up), and $C$ (Brown taking the 7:30 bus), satisfy Reichenbach's requirements for a conjunctive fork. Clearly, however, Brown's bus ride is not a cause either of the coffee being made or the other person's arrival. The coincidence does, indeed, require a common cause, but that event is a telephone appointment made by the secretary on the preceding day.

The crucial feature of Crasnow's counterexample is easy to see. Brown arises early and catches the 7:30 bus if and only if he has an early appointment which was previously arranged by his secretary. The conjunctive fork is constructed out of the two associated effects and another effect which is strictly correlated with the bona fide common cause. When we see how this example has been devised, it is easy to find many others of the same general sort. Suppose it is realized before anyone actually becomes ill that spoiled food has been served in the dormitory. The head resident may place a call to the university health service requesting that a stomach pump be dispatched to the scene; however, neither the call to the health service nor the arrival of the stomach pump constitutes a genuine common cause, though either could be used to form a conjunctive fork.[10]

Inasmuch as two of Reichenbach's key concepts—causal betweenness and conjunctive fork—are unacceptably explicated, we must regard his attempt to provide an account of probabilistic causality as unsuccessful.

### 3. *Suppes' Probabilistic Theory*

In spite of his passing remark about Good's causal calculus, Suppes' theory bears much more striking resemblance to Reichenbach's theory than to Good's. As

mentioned earlier, Suppes and Good agree in stipulating that causes must, by definition, precede their effects in time, and in this they oppose Reichenbach's approach. But here the similarities between Good and Suppes end. Like Reichenbach, and unlike Good, Suppes does not attempt to introduce any quantitative measures of causal strength. Like Reichenbach, and unlike Good, Suppes frames his definitions in terms of measures of probability, without introducing any explicit measure of statistical relevance. It is evident, of course, that considerations of statistical relevance play absolutely fundamental roles in all three theories, but as I commented regarding Good's approach, the use of statistical relevance measures instead of probability measures involves a crucial sacrifice of information. In addition, Suppes introduces a number of causal concepts, and in the course of defining them, he deploys the relations of positive statistical relevance and screening off in ways which bear strong resemblance to Reichenbach. A look at several of his most important definitions will exhibit this fact.

In definition 1 (p. 12) an event $B$ is said to be a *prima facie cause* of an event $A$ if $B$ occurs before $A$ and $B$ is positively relevant, statistically, to $A$.[11] Suppes offers two definitions of spurious causes (pp. 23, 25), the second of which is the stronger and is probably preferable.[12] According to this definition (3), an event $B$ is a *spurious cause* of an event $A$ if it is a prima facie cause of $A$ and it is screened off from $A$ by a partition of events $C_i$ which occur earlier than $B$. We are told (p. 24), though not in a numbered definition, that a *genuine cause* is a prima facie cause which is not spurious. These concepts can easily be applied to *the* most familiar example. The falling barometer is a prima facie cause of a subsequent storm, but it is also a spurious cause, for it is screened-off from the storm by atmospheric conditions which precede both the storm and the drop in barometric reading.

There is a close similarity between Suppes' definition of spurious cause and Reichenbach's definition of conjunctive fork. It is to be noted first, as Reichenbach demonstrates (1956, pp. 158–160), that

$$P(A.B) > P(A)\cdot P(B) \tag{15}$$

follows from relations (11)–(14) above. Therefore, $A$ and $B$ are positively relevant to one another. If $A$ and $B$ are not simultaneous, then one is a prima facie cause of the other. Second, Reichenbach's relations (11) and (12) are equivalent to screening-off relations. According to the multiplication axion,

$$P(A\cdot B|C) = P(A|C)\cdot P(B|A\cdot C); \tag{16}$$

therefore, it follows from (11) that

$$P(A|C)\cdot P(B|C) = P(A|C)\cdot P(B|A\cdot C). \tag{17}$$

Assuming $P(A|C) > 0$, we divide through by that quantity, with the result

$$P(B|C) = P(B|A\cdot C), \tag{18}$$



which says that $C$ screens off $A$ from $B$. In precisely parallel fashion, it can be shown that (12) says that $\overline{C}$ screens off $A$ from $B$. But, $\{C.\overline{C}\}$ constitutes a partition, so $B$ is a spurious cause of $A$ or vice-versa.[13] Suppes does not define the concept of conjunctive fork. Since he assumes temporal priority relations already given, he does not need conjunctive forks to establish temporal direction, and since he is not concerned with scientific explanation, he does not need them to provide explanations in terms of common causes. Nevertheless, there is a considerable degree of overlap between Reichenbach's conjunctive forks and Suppes' spurious causes.

Although Reichenbach defines conjunctive forks entirely in terms of the relations (11)–(14) above, without imposing any temporal constraints, his informal accompanying remarks (1956, pp. 158–159) strongly suggest that the events $A$ and $B$ occur simultaneously, or nearly so. One might be tempted to suppose that Reichenbach wished to regard $A$ and $B$ as simultaneous to a sufficiently precise degree that a direct causal connection between them would be relativistically precluded. Such a restriction would, however, make no real sense in the kinds of examples he offers. Since the velocity of light is approximately 1 foot per nanosecond (1 nsec = $10^{-9}$ sec), the onsets of vomiting in the case of two roommates in the tainted food example (above) would presumably have to occur within perhaps a dozen nanoseconds of one another.

Reichenbach's basic intent can be more reasonably characterized in the following manner. Suppose events of the types $A$ and $B$ occur on some sort of clearly specified association more frequently than they would if they were statistically independent of one another. Then, if we can rule out a direct causal connection from $A$ to $B$ or from $B$ to $A$, we look for a common cause $C$ which, along with $A$ and $B$, constitutes a conjunctive fork. Thus, if Smith and Jones turn in identical term papers for the same class—even if the submissions are far from simultaneous—and if careful investigation assures us that Smith did not copy directly from Jones and also that Jones did not copy directly from Smith, then we look for the common cause $C$ (e.g., the paper in the fraternity file from which both of them plagiarized their papers). It is the absence of a direct causal connection between $A$ and $B$, not simultaneous occurrence, which is crucial in this context. Thus, in Reichenbach's conjunctive forks $A$ may precede $B$ or vice-versa, and hence, one may be a prima facie cause of the other.

Suppes does not introduce the relation of causal betweenness, but he does define the related notions of direct and indirect causes. According to definition 5 (1970, p. 28) an event $B$ is a *direct cause* of an event $A$ if it is a prima facie cause of $B$ and there is no partition $C_i$ temporally between $A$ and $B$ which screens $B$ off from $A$. A prima facie cause which is not direct is *indirect*. Use of such terms as "direct" and "indirect" strongly suggests betweenness relations. Suppes' definition of indirect cause clearly embodies a condition closely analogous to formula (10) of Reichenbach's definition of causal betweenness, but Suppes does not invoke the troublesome relations (8) and (9) which brought Reichenbach's explication to grief. It appears, however, that Suppes' theory faces similar difficulties.

Let us take another look at Rosen's example of the spectacular birdie. As above, let $A$ stand for Jones teeing off, $B$ for the ball striking the tree limb, and $C$ for the ball going into the cup. If this example is to be at all relevant to the

discussion, we must suppose that $A$ is a prima facie cause of $C$, which requires that $P(C|A) > P(C)$. We must, therefore, select some general reference class or probability space with respect to which $P(A)$ can be evaluated. The natural choice, I should think, would be to take the class of all cases of teeing off at that particular hole as the universe.[14] We may then suppose that Jones is a better-than-average golfer; when she tees off there is a higher probability of a birdie than there is for golfers in general who play that particular course. We may further assume that $A$ is a genuine cause of $C$, since there is no plausible partition of earlier events which would screen $A$ off from $C$. Certainly $B$ cannot render $A$ as a spurious cause of $C$, for $B$ does not even happen at the right time (prior to $A$).

There is a more delicate question of whether $A$ is a direct or indirect cause of $C$. We may reasonably assume that $B$ screens $A$ off from $C$, for presumably it makes no difference which player's shot from the rough strikes the tree limb. It is less clear, however, that $B$ belongs to a partition, each member of which screens $A$ from $C$. In other cases, birdies will occur as a result of a splendid shot out of a sand trap, or sinking a long putt, or a fine chip shot from the fairway. In these cases, it seems to me, it would not be irrelevant that Jones, rather than some much less accomplished player, was the person who teed off ($A$). It might be possible to construct a partition $B_i$ which would accomplish the required screening off by specifying the manner in which the ball approaches the cup, rather than referring merely to where the ball came from on the final shot. But, this ploy seems artificial. Just as we rejected the attempt to save Reichenbach's definition of causal betweenness by specifying the physical parameters of the ball and the branch at the moment of collision, so also, I think, must we resist the temptation to resort to similar physical parameters to find a partition which achieves screening off. We are, after all, discussing a golf game, not Newtonian particle physics, as Suppes is eager to insist. The most plausible construal of this example, from the standpoint of Suppes's theory, is to take $A$ to be a direct cause of $C$, and to deny that the sequence $A$, $B$, $C$ has the Markov property. In contrast to Good and Reichenbach, Suppes does not require causal sequences to be Markovian.

The crucial problem about $B$, it seems to me, is that it appears not to qualify even as a prima facie cause of $C$. It seems reasonable to suppose that even the ordinary duffer has a better chance of making a birdie $P(C)$ than Jones has of getting the ball in the hole by bouncing it off of the tree limb $P(C|B)$. In Suppes' definitions, however, being a prima facie cause is a necessary condition of being any kind of cause (other than a negative cause). Surely, as Suppes himself remarks, we must recognize $B$ as a link in the causal chain. The same point applies to the other examples introduced above to show the inadequacy of Reichenbach's definition of causal betweenness. Since the crap-shooter has a better chance of winning at the outset $P(C)$, than he does of winning if he gets 4 on the first toss $P(C|B)$, shooting 4 is not even a prima facie cause of his winning. Even though Suppes desists from defining causal betweenness, the kinds of examples which lead to difficulty for Reichenbach on that score result in closely related troubles in Suppes' theory.

The fundamental problem at issue here is what Rosen (1978, p. 606) calls "Suppes' thesis that a cause will always raise the probability of the effect." Although both Suppes (1970, p. 41) and Rosen (1978, p. 607) sometimes refer to it as the problem of unlikely or improbable consequences, this latter manner of



speaking can be confusing, for it is *not* the small degree of probability of the effect, given the cause, which matters; it is the *negative statistical relevance* of the cause to the occurrence of the effect which gives rise to the basic problem. While there is general agreement that positive statistical relevance is not a sufficient condition of direct causal relevance—we all recognize that the falling barometric reading does not cause a storm—the question is whether it is a necessary condition. Our immediate intuitive response is, I believe, that positive statistical relevance is, indeed, a necessary ingredient in causation, and all three of the theories we are discussing make stipulations to that effect. Reichenbach (1956, p. 201) assumes "that causal relevance is a special form of positive [statistical] relevance." Suppes makes positive statistical relevance a defining condition of prima facie causes (1970, p. 12), and every genuine cause is a prima facie cause (*ibid.*, p. 24). Good incorporates the condition of positive statistical relevance into his definition of causal chains (1961–62, II, p. 45).

In a critical note on Suppes' theory, Germund Hesslow (1976) challenges this fundamental principle:

> The basic idea in Suppes' theory is of course that a cause raises the probability of its effect, and it is difficult to see how the theory could be modified without upholding this thesis. It is possible however that examples could be found of causes that lower the probability of their effects. Such a situation could come about if a cause could lower the probability of other more efficient causes. It has been claimed, e.g., that contraceptive pills ($C$) can cause thrombosis ($T$), and that consequently there are cases where $C_t$ caused $T_{t'}$. [The subscripts $t$ and $t'$ are Suppes' temporal indices.] But pregnancy can also cause thrombosis, and $C$ lowers the probability of pregnancy. I do not know the values of $P(T)$ and $P(T|C)$ but it seems possible that $P(T|C) < P(T)$, and in a population which lacked other contraceptives this would appear a likely situation. Be that as it may, the point remains: *it is entirely possible that a cause should lower the probability of its effect.* (1976, p. 291, Hesslow's italics)

Rosen defends Suppes against this challenge by arguing,

> . . . based on the available information represented by the above probability estimates, we would be hesitant, where a person suffers a thrombosis, to blame the person's taking of contraceptive pills. But it does not follow from these epistemic observations that a particular person's use of contraceptive pills lowers the probability that she may suffer a thrombosis, for, unknown to us, her neurophysiological constitution ($N$) may be such that the taking of the pills definitely contributes to a thrombosis. Formally,
>
> $$P(T|C.N) > P(T)$$
>
> represents our more complete and accurate causal picture. We wrongly believe that taking the pills always lowers a person's probability of thrombosis because we base our belief on an inadequate and superficial knowledge of the causal structures in this medical domain where unanticipated and unappreciated neurophysiological features are not given sufficient attention or adequate weighting. (1978, p. 606)

Rosen comments upon her own example of the spectacular birdie in a similar spirit: "Suppes' first observation in untangling the problems of improbable consequences is that it is important not to let the curious event be rendered causally

spurious by settling for a superficial or narrow view." (*ibid.*, p. 608) As I have indicated above, I do not believe that this is a correct assessment of the problem. If the causal event in question—e.g., the ball striking the branch—is negatively relevant to the final outcome, it is not even a prima facie cause. A fortiori, it cannot achieve the status of a spurious cause, let alone a genuine cause. She continues:

> . . . it is the angle and the force of the approach shot together with the deflection that forms our revised causal picture. Thus we begin to see that the results are unlikely only from a narrow standpoint. A broader picture is the more instructive one. (*ibid.*)

As a result of her examination of Hesslow's example, as well as her own, she concludes that it is a virtue of Suppes' probabilistic theory to be able to accommodate "unanticipated consequences." (*ibid.*)

Rosen's manner of dealing with the problem of causes which appear to bear negative statistical relevance relations to their effects (which is similar to that mentioned by von Bretzel) might be called *the method of more detailed specification of events*. If some event $C$, which is clearly recognized as a cause of $E$, is nevertheless negatively relevant to the occurrence of $E$, it is claimed that a more detailed specification of $C$ (or the circumstances in which $C$ occurs) will render it positively relevant to $E$. I remain skeptical that this approach—though admittedly successful in a vast number of instances—is adequate in general to deal with all challenges to the principle of positive statistical relevance.

Good was clearly aware of the problem of negative statistical relevance, and he provided an explicit way of dealing with it. His approach, which differs from Rosen's, might be called *the method of interpolated causal links*. In an appendix (1961–62, I, p. 318) designed to show that his $\chi$ cannot be identified with his $Q$ he offers an example along with a brief indication of his manner of dealing with it:

> Sherlock Holmes is at the foot of a cliff. At the top of the cliff, directly overhead, are Dr Watson, Professor Moriarty, and a loose boulder. Watson, knowing Moriarty's intentions, realizes that the best chance of saving Holmes's life is to push the boulder over the edge of the cliff, doing his best to give it enough horizontal momentum to miss Holmes. If he does not push the boulder, Moriarty will do so in such a way that it will be nearly certain to kill Holmes. Watson then makes the decision (event $F$) to push the boulder, but his skill fails him and the boulder falls on Holmes and kills him (event $E$).
>
> This example shows that $Q(E|F)$ and $\chi(E:F)$ cannot be identified, since $F$ had a tendency to prevent $E$ and yet caused it. We say that $F$ was a cause of $E$ because there was a chain of events connecting $F$ to $E$, each of which was strongly caused by the preceding one.

This example seems closely related to the remark, later appended to theorem T2 (see note 7 above), to the effect that a cut chain can be uncut by filling in more of the details. Good could obviously take exception to any of the examples discussed above on the ground that the spatio-temporal gaps between the successive events in these chains are too great. He could, with complete propriety, insist that these gaps be filled with intermediate events, each of which is spatio-temporally small, and each of which is contiguous with its immediate neighbors (see I, pp. 307–308; II, p. 45). I am not convinced, however, that every "cut chain" which needs to be



welded back together can be repaired by this device;[15] on the contrary, it seems to me that size is not an essential feature of the kinds of examples which raise problems for Suppes' and Reichenbach's theories. We can find example, I believe, which have the same basic features, but which do not appear to be amenable to Good's treatment.

Consider the following fictitious case, which has the same statistical structure as the first tetrahedron-cum-card example. We have an atom in an excited state which we shall refer to as the 4th energy level. It may decay to the ground state (zeroeth level) in several different ways, all of which involve intermediate occupation of the 1st energy level. Let $P(m \rightarrow n)$ stand for the probability that an atom in the $m$th level will drop directly to the $n$th level. Suppose we have the following probability values:[16]

$$P(4 \rightarrow 3) = 3/4 \qquad P(3 \rightarrow 1) = 3/4$$
$$P(4 \rightarrow 2) = 1/4 \qquad P(2 \rightarrow 1) = 1/4 \tag{19}$$

It follows that the probability that the atom will occupy the 1st energy level in the process of decaying to the ground state is $10/16$; if, however, it occupies the 2nd level on its way down, then the probability of its occupying the 1st level is $1/4$. Therefore, occupying the 2nd level is negatively relevant to occupation of the 1st level. Nevertheless, if the atom goes from the 4th to the 2nd to the 1st level, that sequence constitutes a causal chain, in spite of the negative statistical relevance of the intermediate stage. Moreover, in view of the fact that we cannot, so to speak, "track" the atom in its transitions from one energy level to another, it appears that there is no way, even in principle, of filling in intermediate "links" so as to "uncut the chain." Furthermore, it seems unlikely that the Rosen method of more detailed specification of events will help with this example, for when we have specified the type of atom and its energy levels, there are no further facts which are relevant to the events in question. Although this example is admittedly fictitious, one finds cases of this general sort in examining the term schemes of actual atoms.[17]

There is another type of example which seems to me to cause trouble for both Reichenbach and Suppes. In a previous discussion of the principle of the common cause (1978) I suggested the need to take account of *interactive forks* as well as conjunctive forks. Consider the following example. Pool balls lie on the table in such a way that the player can put the 8-ball into one corner pocket at the far end of the table if and almost only if his cue-ball goes into the other far corner pocket. Being a relative novice, the player does not realize that fact; moreover, his skill is such that he has only a 50-50 chance of sinking the 8-ball even if he tries. Let us make the further plausible assumption that, if the two balls drop into the respective pockets, the 8-ball will fall before the cue-ball does. Let event $A$ be the player attempting that shot, $B$ the dropping of the 8-ball into the corner pocket, and $C$ the dropping of the cue-ball into the other corner pocket. Among all of the various shots the player may attempt, a small proportion will result in the cue-ball landing in that pocket. Thus, $P(C|B) > P(C)$; consequently, the 8-ball falling into one corner pocket is a prima facie cause of the cue-ball falling into the other pocket. This is as it should be, but we must also be able to classify $B$ as a spurious cause of $C$. It is not quite clear how this is to be accomplished. The event $A$, which

must surely qualify as a direct cause of both $B$ and $C$ does not screen $B$ off from $C$, for $P(C|A) = ½$ while $P(C|A.B) = 1$.

It may be objected, of course, that we are not entitled to infer, from the fact that $A$ fails to screen off $B$ from $C$, that there is no event prior to $B$ which does the screening. In fact, there is such an event—namely, the compound event which consists of the state of motion of the 8-ball and the state of motion of the cue-ball shortly after they collide. The need to resort to such artificial compound events does suggest a weakness in the theory, however, for the causal relations among $A$, $B$, and $C$ seem to embody the salient features of the situation. An adequate theory of probabilistic causality should, it seems to me, be able to handle the situation in terms of the relations among these events, without having to appeal to such ad hoc constructions.

## 4. *A Modest Suggestion*

In the preceding sections, I have invoked counterexamples of three basic types to exhibit the most serious inadequacies of the theories of Good, Reichenbach, and Suppes. The first type consists simply of situations in which a given result may come about in more than one way. Since there are usually "more ways than one to skin a cat," such counterexamples cannot be considered weird or outlandish; on the contrary, they typify a large class of actual situations. Theories of probabilistic (or any other sort of) causality which cannot handle cases of this kind are severely inadequate. The second type consists of what may generally be classified as causal interactions. In Salmon (1978, p. 692) I offered Compton scattering as an example of an interactive fork.[18] Other examples include the interaction between a ray of white light and a red filter, the deformations of fenders of automobiles which bump into one another, and the mutual arousal of lovers who exchange a passionate kiss. Examples of this sort are also far from esoteric; the notion of causal interaction is central to the whole concept of causality. The third type is illustrated by Crasnow's example, which shows how easily a bona fide common cause can give rise to a triple of events which satisfy all of Reichenbach's conditions (11)–(14) for conjunctive forks but which embody spurious causal relations. Again, the situations in which this can occur are far from exceptional.

It seems to me that the fundamental source of difficulty in all three of the theories discussed above is that they attempt to carry out the construction of causal relations on the basis of probabilistic relations among discrete events, without taking account of the physical connections among them. This difficulty, I believe, infects many non-probabilistic theories as well. When discrete events bear genuine cause-effect relations to one another—except, perhaps, in some instances in quantum mechanics—there are spatio-temporally continuous causal processes joining them.[19] It is my view that these processes transmit causal influence (which may be probabilistic) from one region of space-time to another. Thus, a golf ball flying through the air is a causal process connecting the collision with the tree branch to the dropping of the ball into the cup. Various types of wave phenomena, including light and sound, and the continuous space-time persistence of material objects, whether moving or stationary, are examples of causal processes which provide causal connections between events. In Salmon (1977), I attempted to develop an 'at-at' theory of causal influence which explicates the manner in which causal processes transmit such influence.[20] In Salmon (1975 and 1978), I have attempted to say something about the role played by causal processes in causal explanations.



There is a strong tendency on the part of philosophers to regard causal connections as being composed of chains of intermediate events, as Good brings out explicitly in his theory, rather than spatio-temporally continuous entities which enjoy fundamental physical status, and which do *not* need to be constructed out of anything else. Such a viewpoint can lead to severe frustration, for we are always driven to ask about the connections among *these* events, and interpolating additional events does not seem to mitigate the problem. In his discussion of Locke's concept of power, Hume (1748, sec. VII, Part I) seems to have perceived this difficulty quite clearly. I am strongly inclined to reverse the position, and to suggest that we accord fundamental status to processes. If only one sort of thing can have this status, I suggest that we treat events as derivative. As John Venn remarked more than a century ago, "Substitute for the time-honoured 'chain of causation,' so often introduced into discussion upon this subject, the phrase a 'rope of causation,' and see what a very different aspect the question will wear" (1866, p. 320).

It is beyond the scope of this paper to attempt a rigorous construction of a probabilistic theory of causality, but the general strategy should be readily apparent. To begin, we can easily see how to deal with the three basic sorts of counterexamples discussed above. First, regarding Rosen's example, we shall say that the striking of the limb by the golf ball is causally between the teeing-off and the dropping into the hole because there is a spatio-temporally continuous causal process—the history of the golf ball—which connects the teeing-off with the striking of the limb, and connects the striking of the limb with the dropping into the hole. Second, we can handle the pool-ball example by noting that the dropping of the 8-ball into the pocket is not a genuine cause of the cue-ball falling into the other pocket, because there is no causal process leading directly from the one event to the other. Third, we can deal with the Crasnow example by pointing out that the telephone appointment made by Brown's secretary constitutes a common cause for the coffee being ready and for the arrival of the business associate, because there is a causal process which leads from the appointment to the making of the coffee and another causal process which leads from the appointment to the arrival of the other person. However, there are no causal processes leading from Brown's boarding of the early bus to the making of the coffee or to the arrival of the other person.

In this paper, I have raised three fundamental objections to the theories of probabilistic causality advanced by Good, Reichenbach, and Suppes. Taking these objections in the reverse order to that in which they were introduced, I should like to indicate how I believe they ought to be handled.

(1) In the interactive fork—e.g., Compton scattering or collisions of billiard balls—the common cause $C$ fails to screen off one effect $A$ from the other effect $B$. In Suppes's theory, if $A$ bears the appropriate temporal relation to $B$, $A$ will qualify as a prima facie cause of $B$, but because of the failure of screening-off, it cannot be relegated to the status of spurious cause. When we pay appropriate attention to the causal processes involved, the difficulty vanishes. As we have already remarked, the presence of causal processes leading from $C$ to $A$ and from

$C$ to $B$, along with the absence of any direct causal process going from $A$ to $B$, suffice to show that $A$ is not a genuine cause of $B$.

Reichenbach's theory of common causes needs to be supplemented in two ways. First, we must recognize that there are two types of forks, conjunctive and interactive. Conjunctive forks are important, but they do not characterize all types of situations in which common causes are operative (see Salmon, 1978). Second, the characterization of both types of forks needs to incorporate reference to connecting causal processes in addition to the statistical relevance relations among triads of events. In the case of conjunctive forks, the appeal to causal processes enables us to overcome the problem raised by Crasnow's counterexample.

(2) The most difficult problem, it seems to me, involves the dictum that cause-effect relations must always involve relations of positive statistical relevance. I believe that the examples already discussed show that this dictum cannot be accepted in any simple and unqualified way; at the same time, it seems intuitively compelling to argue that a cause which contributes probabilistically to bringing about a certain effect must at least raise the probability of that effect vis à vis some other state of affairs. For example, in the tetrahedron-cum-card game, once the tetrahedron has been tossed and has landed on side 4, the initial probability of drawing a red card in the game is irrelevant to the causal process (or sequence[21]) which leads to the draw of a red card from the deck which is poorer in red cards. What matters is that a causal process has been initiated which may eventuate in the drawing of a red card; it makes no difference that an alternative process might have been initiated which would have held a higher probability of yielding a red card.

Once the tetrahedron has come to rest, one of two alternative processes is selected. There is an important sense in which it possesses an *internal* positive relevance with respect to the draw of a red card. When this example was introduced above, I made the convenient but unrealistic simplifying assumption that a draw would be made from the second deck if and only if the tetrahedron toss failed to show side 4. However, a player who has just made a toss on which the tetrahedron landed on side 4 might simply get up and walk away in disgust, without drawing any card at all. In this case, of course, he is certain not to draw a red card. When we look at the game in this way, we see that, given the result of the tetrahedron toss, the probability of getting a red card by drawing from the second deck is greater than it is by not drawing at all—thus, drawing from the second deck *is* positively relevant to getting a red card.

In order to deal adequately with this example, we must restructure it with some care. In the first place, we must choose some universe $U$ which is not identical with $F$, for otherwise $P(E|F) = P(E)$ and $P(G|F) = P(G)$, in which case $F$ is not positively relevant either to its immediate successor $G$ or to the final effect $E$. Thus, the player may choose to play the game (event $F$)—perhaps a fee must be paid to enter the game—or he may choose not to play (event $\overline{F}$). If he chooses not to play, then he has no chance to draw from either deck, and no chance to draw a red card. We must now distinguish between the outcome of the toss of the tetrahedron and the draw from the associated deck of cards. Let $G$ = the tetrahedron lands showing side 4, $\overline{G}$ = the tetrahedron does not show side 4; $H_1$ = the player draws from the first deck, $H_2$ = the player draws from the second deck, $H_3$ = the player does not draw from either deck. As before, $E$ = the player draws a red card, $\overline{E}$ = the player does not draw a red card.[22]



Now suppose that the following chain of events occurs: $F \rightarrow \overline{G} \rightarrow H_2 \rightarrow E$. We can say that each event in the chain is positively relevant to its immediate successor in the sense that the following three relations hold:

$$P_U(\overline{G}|F) > P_U(\overline{G}) \tag{20}$$
$$P_F(H_2|\overline{G}) > P_F(H_2) \tag{21}$$
$$P_{\overline{G}}(E|H_2) > P_{\overline{G}}(E) \tag{22}$$

We begin with the probabilities taken with respect to the original universe $U$ (the subscript $U$ is vacuous in (20) but it is written to emphasize the point); when $F$ has occurred, our universe is narrowed to the intersection $F \cap U$ (hence the subscript $F$ in (21) ); when $\overline{G}$ has occurred, our universe is narrowed to the intersection $\overline{G} \cap F \cap U$ (hence the subscript $\overline{G}$ in (22) ). Even though this chain has the Markov property, guarantying that

$$P_U(E|H_2) = P_{\overline{G}}(E|H_2) \tag{23}$$

the successive narrowing of the universe after each event in the chain has occurred does reverse some of the relevance relations; in particular, although

$$P_U(E|H_2) < P_U(E), \tag{24}$$

nevertheless, as (22) asserts

$$P_{\overline{G}}(E|H_2) > P_{\overline{G}}(E)$$

because the prior probability of $E$ is quite different in the two universes. The equality (23) obviously does not prevent the reversal of the inequalities between (22) and (24).[23]

A similar approach can be applied to Rosen's example of the spectacular birdie and to Good's example of Dr. Watson and the boulder. Once the player has swung on her approach shot, and the ball is traveling toward the tree and not toward the hole, the probability of the ball going into the hole if it strikes the tree limb is greater—given the general direction it is going—than if it does not make contact with the tree at all. Likewise, once Watson has shoved the rock so that Moriarty cannot get his hands on it, the probablity of Holmes's death if Moriarty pushes the boulder over the cliff is irrelevant to the causal chain.

While the foregoing approach, which combines features of Rosen's method of more detailed specification of events with Good's method of interpolation of causal links, seems to do a fairly satisfactory job of handling macroscopic chains—which can always be analyzed in greater detail—it is doubtful that it can deal adequately with the example of the decaying atom introduced above. If the atom is in the 4th energy level, it will be recalled, there is a probability of $^{10}/_{16}$ that it will occupy the 1st energy level in the process of decaying to the ground state. If it drops from the 4th level to the 2nd level in the course of its decay, then there is a probability of ¼ that it will occupy the 1st level. Thus, occupation of the 2nd level is negatively relevant to its entering the 1st level. However, in a given instance, an atom *does* go from the 4th level to the 2nd, from the 2nd level to the 1st, and then to the ground state. In spite of the fact that being in the 2nd level is

negatively relevant to being in the 1st, the causal chain *does* consist in occupation of the 4th, 2nd, 1st and ground levels. Occupation of the 2nd level is the *immediate* causal antecedent of occupation of the 1st level. The atom, in sharp contrast to the golf ball, cannot be said—subsequent to its departure from the 2nd level and prior to its arrival at the 1st level—to be "headed toward" the 1st level rather than the ground state. Interpolation of intermediate events just does not work in situations of this sort.[24]

An appropriate causal description of the atom, it seems to me, can be given in the following terms. Our particular atom in its given initial state (e.g., what we called the 4th energy level) persists in that condition for some time, and as long as it does so it retains a certain probability distribution for making spontaneous transitions to various other energy levels (i.e., $P(4 \rightarrow 3) = 3/4$; $P(4 \rightarrow 2) = 1/4$). Of course, if incident radiation impinges upon the atom, it may make a transition to a higher energy by absorbing a photon, or its probability of making a transition to a lower energy level may be altered due to the phenomenon of stimulated emission. But in the absence of outside influences of that sort, it simply *transmits* the probability distribution through a span of time. Sooner or later, it makes a transition to a lower energy level (say the 2nd), and this event, which is marked by emission of a photon of characteristic frequency for that transition, transforms the previous process into another—namely, that atom in the state characterized by the 2nd energy level. This process carries with it a new probability distribution (i.e., $P(2 \rightarrow 1) = 1/4$; $P(2 \rightarrow 0) = 3/4$). If it then drops to the 1st level, emitting another photon of suitable frequency, it is transformed into a different process which has a probability of 1 for a transition to the ground state. Eventually it drops into the ground state.

It is to be noted that the relation of positive statistical relevance does not enter into the foregoing characterization of the example of the decaying atom. Instead, the case is analyzed as a series of causal processes, succeeding one another in time, each of which transmits a definite probability distribution—a distribution which turns out to give the probabilities of certain types of interactions. Transmission of a determinate probability distribution is, I believe, the essential function of causal processes with respect to the theory of probabilistic causality.[25] Each transition event can be considered as an intersection of causal processes, and it is the set of probabilities of various outcomes of such interactions which constitute the transmitted distribution. While it is true that the photon, whose emission marks the transition from one process to another, does not exist as a separate entity prior to its emission, it does constitute a causal process from that time on. The intersection is like a fork where a road divides into two distinct branches—indeed, it qualifies, I believe, as a bona fide interactive fork. The atom with its emitted photons (and absorbed photons as well) exemplifies the fundamental interplay of causal processes and causal interactions upon which, it seems to me, a viable theory of probabilistic causality must be built. The thesis that causation entails positive statistical relevance is not part of any such theory.

(3) As we have seen, Good has undertaken a more ambitious project than Reichenbach and Suppes, for he has attempted to construct a quantitative measure of the degree to which one event causes another. I am inclined to think that this effort is somewhat premature—that we need to have a much clearer grasp of the qualitative notion of probabilistic causality before we can hope to develop a satisfactory quantitative theory. I have indicated why I believe Good's particular construction is unsatisfactory, but that does not mean that a satisfactory measure cannot be developed. As I indicated above, I think it will need to employ the individual probability values $P(E|F)$ and $P(E|\bar{F})$ instead of Good's relevance measure $Q(E:F)$, or any other measure of statistical relevance. The details remain to be worked out.



The essential ingredients in a satisfactory qualitative theory of probabilistic causality are, it seems to me: (1) a fundamental distinction between causal processes and causal interactions (2) an account of the propagation of causal influence via causal processes, (3) an account of causal interactions in terms of interactive forks, (4) an account of causal directionality in terms of conjunctive forks, and (5) an account of causal betweenness in terms of causal processes and causal directionality. The 'at-at' theory of causal influence (Salmon, 1977) gives, at best, a symmetric relation of causal connection. Conjunctive forks are needed to impose the required asymmetry upon connecting processes.

If an adequate theory of probabilistic causality is to be developed, it will borrow heavily from the theories of Reichenbach and Suppes; these theories require supplementation rather than outright rejection. Once we are in posssession of a satisfactory qualitative theory, we may be in a position to undertake Good's program of quantification of probabilistic causal relations. These goals are, I believe, eminently worthy of pursuit.

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## NOTES

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[1]Both Good and Reichenbach published earlier discussions of probabilistic causality, but both authors regard them as superseded by the works cited here.

[2]The roman numerals in references to Good refer to the respective parts of his two-part article.

[3]Throughout this article, I shall use italic capital letters to designate classes of individuals or events. I shall construe physical probabilities as relative frequencies, but those who prefer other concepts of physical probability can easily make any adjustments they deem appropriate. In certain contexts, where no confusion is likely to arise, I shall speak of the occurrence of an event $A$ instead of using the more cumbrous expression, "occurrence of an event which is a member of the class $A$."

[4]$P(E|F)$, $P(E)$, and $P(E|\bar{F})$ are independent probabilities; no one of the three can be deduced from the other two. In contexts such as the present, we shall assume that neither $P(F)$ nor $P(\bar{F})$ vanishes, for if they did, there would be problems about whether the conditional probabilities, $P(E|F)$ and $P(E|\bar{F})$, are well-defined. Given this assumption about $P(F)$ and $P(\bar{F})$, it is easily shown that:

$$P(E|F) > P(E) > P(E|\bar{F}) \text{ if } P(E|F) > P(E|\bar{F}),$$
$$P(E|F) = P(E) = P(E|\bar{F}) \text{ if } P(E|F) = P(E|\bar{F}),$$
$$P(E|F) < P(E) < P(E|\bar{F}) \text{ if } P(E|F) < P(E|\bar{F}):$$

thus, the sign of $P(E|F) - P(E|\bar{F})$ is the same as the sign of $P(E|F) - P(E)$. For purposes of developing a qualitative theory of probablistic casuality, it does not matter much whether one takes a relevance measure defined in terms of $P(E|F)$ and $P(E|\bar{F})$ or one defined in terms of $P(E|F)$ and $P(E)$, for positive

relevance, irrelevance, and negative relevance are all that matter. For purposes of developing a quantitative theory of probabilistic casuality, this choice of relevance measures is extremely important. In the cases of positive and negative relevance, the foregoing relations tell us that $P(E)$ lies strictly between $P(E|F)$ and $P(E|\bar{F})$, but from the values of these latter two probabilities, we cannot tell where $P(E)$ lies within that interval—i.e., whether it is close to $P(E|F)$, close to $P(E|\bar{F})$, or nearly midway between. If, in addition to $P(E|F)$ and $P(E|\bar{F})$, we are also given the value of $P(F)$, then we can answer that question, for

$$P(\bar{F}) = 1 - P(F) = \frac{P(E|F) - P(E)}{P(E|F) - P(E|\bar{F})}$$

Suppose that $F$ is a bona fide probabilistic cause of $E$—e.g., $F$ might be some particular disease and $E$ might be death. When an epidemiologist asks how serious a disease $F$ is, he might well be concerned with the value of $P(E) - P(E|\bar{F})$, that is, the amount by which the actual death rate in the actual population exceeds the death rate in a similar population which is free from the disease $F$. Since

$$P(E) - P(E|\bar{F}) = P(F)[P(E|F) - P(E|\bar{F})]$$

that quantity is a function of two factors, namely, $P(F)$, which tells us how widespread the disease is in the population at large, and $P(E|F) - P(E|\bar{F})$, which tells us how greatly an individual's chance of death is enhanced by contracting the disease. The same overall effect might be the result of two different situations. In the first place, $F$ might represent a disease such as cancer of the pancreas, which does not occur with especially high frequency, but which is almost always fatal when it does occur. In the second place, $F$ might represent a disease such as influenza, which occurs much more widely, but which is not fatal in nearly such a high percentage of cases. Notice that this latter consideration is measured, not by $P(E|F)$, but by $P(E|F) - P(E|\bar{F})$; we are not concerned with the probability that someone who has influenza will die, but rather with the difference made by influenza to the probability of death.

If, along with Good, we are interested in measuring the degree to which $F$ caused $E$ in an actual causal chain of events, then it seems clear that we are concerned with $P(E|F) - P(E|\bar{F})$, or some other function of these two probabilities, for we want to be able to say in individual cases to what degree $F$ contributed causally to bringing about the result $E$. Since we are not concerned primarily with the overall effect of $F$ in the population at large, measures which are functions of $P(E)$ and $P(E|F)$ or of $P(E)$ and $P(E|\bar{F})$ are not suitable.

[5]For the moment, we need not regard the tetrahedron's coming to rest with side 4 showing and the draw from the predominantly red deck as two distinct events inasmuch as one happens (by the rules of the game) if and only if the other does.

[6]If we use Good's own relevance measure $Q$, it is easily shown (see equation (4) ) that $Q(E:F) = \log {}^{16}/_{9} \neq Q(E':F') = \log {}^{16}/_{3}$. In the main text, I avoided the use of Good's $Q$, for I did not want to give the impression that I am arguing the trivial point that $Q(E:F)$ is not a function of $Q(E:G)$ and $Q(G:F)$. The main point is, rather, that in measuring the strengths of causal chains, we cannot afford to neglect the statistical relevance of the first event to the last.

[7]The requirement that each event in the chain be positively relevant to its immediate successor appears in two places—in the formal definition of causal chains (II, p. 45) and in theorem T2 (I, p. 311) which says, "$\phi$ vanishes if the chain is cut, i.e., if any of the links is of zero strength." In "Errata and Corrigenda" Good adds a gloss on T2: "It is worth noting that a 'cut' chain can often be uncut by filling in more detail." In section 4 below, I shall consider whether this strategem enables Good to escape the basic difficulty which Reichenbach and Suppes face.

[8]In most cases, of course, the shot from the tee is not the one which strikes the branch, for there are few, if any, par 2 holes. However, the fact that there are other strokes does not alter the import of the example with respect to Reichenbach's definition of causal betweenness.

[9]The basic features of this game are given clearly and succinctly by Copi (1972, pp. 481–482). A shooter whose point is 4, for example, is said to make it "the hard way" if he does so by getting a double 2, which is less probable than a 3 and a 1.



[10]The day after I wrote this paragraph, an announcement was broadcast on local radio stations informing parents that students who ate lunch at several elementary schools may have been infected with salmonella, which probabilistically causes severe gastric illness. Clearly the consumption of unwholesome food, not the radio announcement, is the common cause of the unusually high incidence of sickness within this particular group of children.

[11]In defining many of his causal concepts, Suppes uses conditional probabilities of the form $P(B|A)$. Since, according to the standard definition of conditional probability $P(B|A) = P(A \cdot B)/P(A)$, this probability would not be well-defined if $P(A) = 0$. Suppes explicitly includes in his definitions stipulations that the appropriate probabilities are non-zero. In my discussion I shall, without further explicit statement, assume that all conditional probabilities introduced into the discussion are well-defined.

[12]Suppes refers to these as "spurious in sense one" and "spurious in sense two." Since I shall adopt sense two uniformly in this discussion, I shall not explicitly say "in sense two" in the text.

[13]In an easily overlooked remark (1956, p. 159), Reichenbach says, "If there is more than one possible kind of common cause, $C$ may represent the disjunction of these causes." Hence, Reichenbach recognizes the need for partitions finer than $\{C, \bar{C}\}$, which makes for an even closer parallel between his notion of a conjunctive fork and Suppes's notion of a spurious cause.

[14]We cannot let $A$ = the universe, for then $P(C|A) = P(C)$ and $A$ could not be even a prima facie cause.

[15]Paul Humphreys has provided a theorem which has an important bearing upon the question of the mending of cut chains. In any two-state Markov chain, the statistical relevance of the first to the last member is zero if and only if at least one link in the chain exhibits zero relevance, and the statistical relevance of the first to the last member is negative only if an odd number of links exhibit negative relevance. The first member of a two-state Markov chain is positively relevant to the last if and only if no link has zero relevance and an even number (including none) of the links exhibit negative relevance. In other words, the signs of the relevance measures of the links multiply exactly like the signs of real numbers. Thus, it is impossible for a two-state Markov chain whose first member is negatively relevant to its last, or whose first member is irrelevant to its last, to be constructed out of links all of which exhibit positive relevance—just as it is impossible for the product of positive real numbers to be zero or negative. It may, however, be possible to achieve this goal if, in the process of interpolating additional events, the two-state character is destroyed by including new alternatives at one or more stages.

[16]We assume that the transition from the 3rd to the 2nd level is prohibited by the selection rules.

[17]See, for example, the cover design on the well-known elementary text, Wichmann (1967), which is taken from the term scheme for neutral thallium. This term scheme is given in fig. 34A, p. 199.

[18]This example is important because, in contrast to billiard ball collisions, there is no plausible ground for maintaining that a deterministic analysis of the interaction is possible in principle—it appears to constitute an instance of irreducibly probabilistic causation. It is possible to argue, as Bas van Fraassen kindly pointed out to me, that Compton scattering does exhibit some of the anomalous characteristics of the Einstein-Podolsky-Rosen situation, but these features of it do not, I believe, affect the simple use of the example in this context.

[19]I do not believe quantum indeterminacy poses any particular problems for a probabilistic theory of causality, or for the notion of continuous causal processes. This quantum indeterminacy is, in fact, the most compelling reason for insisting upon the need for probabilistic causation. The really devastating problems arise in connection with what Reichenbach called "causal anomalies"—such as the Einstein-Podolsky-Rosen problem—which seem to involve some form of action-at-a-distance. I make no pretense of having an adequate analysis of such cases.

[20]Nancy Cartwright has raised an important difficulty regarding this theory, which I shall discuss in a future publication, but I do not think her objection is insuperable.

[21]Actually, in this example as in most others, we have a *sequence of events* joined to one another by a *sequence of causal processes*. The events, so to speak, mark the ends of the segments of processes; they are the points at which one process joins up with another. Events can, in most if not all cases, be regarded as intersections of processes.

[22]Notice that, in restructuring this example, we have removed it from the class of two-state Markov chains, for there are three alternatives $H_i$. The theorem of Paul Humphreys, mentioned in note 15, is therefore not applicable.

[23]In this context we must take our relevance relations as signifying a relationship between such probabilities as $P(E|F)$ and $P(E)$, rather than $P(E|\bar{F})$ and $P(E|F)$, for $P_F(E|\bar{F})$ is undefined.

[24] If we take $F$ to stand for occupation of the 4th level, $G$ occupation of the 3rd level, $\bar{G}$ occupation of the 2nd level, and $E$ occupation of the 1st level, we cannot say whether or not

$$P(\bar{G}|F) > P(\bar{G})$$

for it depends upon the universe selected. If we take as our universe the set of neutral thallium atoms in highly excited states, it may be less probable that an atom in the 4th level will occupy the 2nd level than it is for thallium atoms in general in higher energy levels. But this question seems to be beside the point; we are concerned with what happens to an atom, given that it occupies the 4th level. In that case, we can say unequivocally that

$$P_F(E|\bar{G}) < P_F(E);$$

therefore, the positive relevance requirement fails even if we conditionalize at every stage.

There are at least two plausible rebuttals to this argument against the positive relevance requirement. It might seem altogether reasonable to deny that the atom provides a causal chain, for the transitions "just happen by chance"; nothing *causes* them.

I. J. Good, in private correspondence, has argued that my treatment of the example is oversimplified. He points out, quite correctly, that I have used a simplified version of his notation throughout, ignoring the more detailed notation he introduced in his articles (esp. I, p. 309). He suggests that we need to compare not only atoms in the 3rd level with atoms in the 2nd level, but also with the situation in which no atom is present at all.

I have a great deal of sympathy with this approach, but I am inclined to feel that it is better to emphasize causal transmission of a definite probability distribution than to insist upon positive relevance.

[25] In this simple example, the probabilities remain constant as the process goes on, but this does not seem to be a general feature of causal processes. In other cases, the probabilities change in a lawful way as the process progresses. A golf ball, in flight, loses energy and momentum, and this changes its probability of breaking a pane of glass interposed in its path.

## REFERENCES


Copi, Irving, 1972: *Introduction to Logic*, 4th ed., Macmillan, New York.

Good, I. J., 1961–62: "A Causal Calculus I-II," *British Journal for the Philosophy of Science*, vol XI, no. 44, pp. 305–318; vol. XII, no. 45, pp. 43–51. Errata and Corrigenda, vol. XIII, no. 49, p. 88.

Hesslow Germund, 1976: "Two Notes on the Probabilistic Approach to Causality," *Philosophy of Science*, vol. 43, pp. 290–292.

Hume, David, 1748: *An Enquiry Concerning Human Understanding*, many editions available.

Humphreys, Paul W., "Cutting the Causal Chain," forthcoming in *Pacific Philosophical Quarterly* (July 1980).

Reichenbach, Hans, 1956: *The Direction of Time*, University of California Press, Berkeley and Los Angeles.

Rosen, Deborah A., 1978: "In Defense of a Probabilistic Theory of Causality," *Philosophy of Science*, vol. 45, pp. 604–613.

Salmon, Wesley C., 1975: "Theoretical Explanation," in Stephan Körner, ed., *Explanation* (Basil Blackwell, Oxford, 1975), pp. 118–145.

Salmon, Wesley C., 1977: "An 'At-At' Theory of Causal Influence," *Philosophy of Science*, vol 44, no. 2 (June, 1977), pp. 215–224.

Salmon, Wesley C., 1978: "Why Ask, 'Why?'?—An Inquiry Concerning Scientific Explanation," *Proceedings and Addresses of the American Philosophical Association*, vol. 51, no. 6 (August, 1978), pp. 683–705.

Suppes, Patrick, 1970: *A Probabilistic Theory of Causality*, North-Holland, Amsterdam.

Venn, John, 1866: *The Logic of Chance*, Macmillan & Co., London.

von Bretzel, Philip, 1977: "Concerning a Probabilistic Theory of Causation Adequate for the Causal Theory of Time," *Synthese*, vol. 35, no. 2, pp. 173–190. Also published in Wesley C. Salmon, ed., *Hans Reichenbach: Logical Empiricist* (D. Reidel, Dordrecht & Boston, 1979), pp. 385–402.

Wichmann, Eyvind H., 1967: *Quantum Physics* (Berkeley Physics Course, vol. 4), McGraw-Hill, New York.


# DO THE LAWS OF PHYSICS STATE THE FACTS?


BY

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*0. Introduction.* There is a view about laws of nature that is so deeply entrenched that it doesn't even have a name of its own. It is the view that laws of nature describe facts about reality. If we think that the facts described by a law obtain, or at least that the facts which obtain are sufficiently like those described in the law, we count the law true, or true-for-the-nonce, until further facts are discovered. I propose to call this doctrine the *facticity* view of laws. (The name is due to John Perry.)

It is customary to take the fundamental explanatory laws of physics as the ideal. Maxwell's equations, or Schroedinger's, or the equations of general relativity are paradigms, paradigms upon which all other laws—laws of chemistry, biology, thermodynamics, or particle physics—are to be modeled. But this assumption confutes the facticity view of laws. For the fundamental laws of physics do not describe true facts about reality. Rendered as descriptions of facts, they are false; ammended to be true, they lose their fundamental, explanatory force.

To understand this claim, it will help to contrast biology with physics. J. J. C. Smart ([10]: chapter 2) has argued that biology is a second-rate science. This is because biology has no genuine laws of its own. It resembles engineering. Any general claim about a complex system, such as a radio or a living organism, will be likely to have exceptions. The generalizations of biology, or engineering's rules of thumb, are not true laws because they are not exceptionless. If this is a good reason, then it must be physics which is the second rate science. Not only do the laws of physics have exceptions; unlike biological laws, they are not even true for the most part, or approximately true.

The view of laws with which I begin—"Laws of nature describe facts about reality"—is a pedestrian view that, I imagine, any scientific realist will hold. It supposes that laws of nature tell how objects of various kinds behave: how they behave some of the time, or all of the time, or even (if we want to prefix a necessity operator) how they must behave. What is critical is that they talk about objects—real concrete things that exist here in our material world, things like quarks, or mice, or genes; and they tell us what these objects do.

Biological laws provide good examples. For instance, here is a generalization taken from a Stanford text on chordates: (Alexander [1]:179)

The gymnotoids [American knife fish] are slender fish with enormously long anal fins, which suggest the blade of a knife of which the head is a handle. They often swim slowly

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