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Statistics and Causal Inference

PAUL W. HOLLAND*

Problems involving causal inference have dogged at the heels of statistics since its earliest days. Correlation does not imply causation, and yet causal conclusions drawn from a carefully designed experiment are often valid. What can a statistical model say about causation? This question is addressed by using a particular model for causal inference (Holland and Rubin 1983; Rubin 1974) to critique the discussions of other writers on causation and causal inference. These include selected philosophers, medical researchers, statisticians, econometricians, and proponents of causal modeling.

KEY WORDS: Causal model; Philosophy; Association; Experiments; Mill's methods; Causal effect; Koch's postulates; Hill's nine factors; Granger causality; Path diagrams; Probabilistic causality.

1. INTRODUCTION

The reaction of many statisticians when confronted with the possibility that their profession might contribute to a discussion of causation is immediately to deny that there is any such possibility. "That correlation is not causation is perhaps the first thing that must be said" (Barnard 1982, p. 387). Possibly this evasive action is in response to all of those needling little headlines that pop up in the most unexpected places, for example, "If the statistics cannot relate cause and effect, they can certainly add to the rhetoric" (Smith 1980, p. 998).

One need only recall that a well-designed randomized experiment can be a powerful aid in investigating causal relations to question the need for such a defensive posture by statisticians. Randomized experiments have transformed many branches of science, and the early proponents of such studies were the same statisticians who founded the modern era of our field.

This article takes the view that statistics has a great deal to say about certain problems of causal inference and ought to play a more significant role in philosophical analyses of causation than it has heretofore. In addition, I will try to show why the statistical models used to draw causal inferences are distinctly different from those used to draw associational inferences.

The article is organized as follows. First, statistical models appropriate for associational and causal inferences will be discussed and compared. Then they will be applied to various ideas about causation that have been expressed by several writers on this subject. One difficulty that arises in talking about causation is the variety of questions that are subsumed under the heading. Some authors focus on the ultimate meaningfulness of the notion of causation. Others are concerned with deducing the causes of a given effect. Still others are interested in understanding the details of causal mechanisms. The emphasis here will be on *measuring the effects of causes* because this seems to be a place

where statistics, which is concerned with measurement, has contributions to make. It is my opinion that an emphasis on the effects of causes rather than on the causes of effects is, in itself, an important consequence of bringing statistical reasoning to bear on the analysis of causation and directly opposes more traditional analyses of causation.

2. MODEL FOR ASSOCIATIONAL INFERENCE

The model appropriate for associational inference is simply the standard statistical model that relates two variables over a population. For clarity and for comparison with the model for causal inference described in the next section, however, I will briefly review association here. If I seem overly explicit in describing the model it is only because I wish to be absolutely clear on the fundamental elements of the theory presented here.

The model begins with a *population* or universe U of "units." A unit in U will be denoted by u . Units are the basic objects of study in an investigation. Examples of units are human subjects, laboratory equipment, households, and plots of land. A *variable* is simply a real-valued function that is defined on every unit in U . The value of a variable for a given unit u is the number assigned by some measurement process to u . A population of units and variables defined on these units are the basic elements of the models for both association and causation presented here. They correspond to the mathematical concepts of a set and real-valued functions defined on the elements of the set. They are the primitives of the theory and will not be further defined.

Suppose that for each unit u in U there is associated a value $Y(u)$ of a variable Y . Suppose further that Y is a variable of scientific interest in the sense that one wishes to understand why the values of Y vary over the units in U . Y is the *response variable* because of its status as a "variable to be explained." In making associational inferences one is satisfied with discovering how the values of Y are associated with the values of other variables defined on the units of U . Let A be a second variable defined on U . Distinguish A from Y by calling A an *attribute* of the units in U . Logically, however, A and Y are on an equal footing, since they are both simply variables defined on U .

All probabilities, distributions, and expected values involving variables are computed over U . A probability will mean nothing more nor less than a proportion of units in U . The expected value of a variable is merely its average value over all of U . Conditional expected values are averages over subsets of units where the subsets are defined by conditioning in the values of variables. It is in this sense that the models described here are population models.

The role of time needs to be mentioned here. Popula-

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tions of units exist within a time frame of some sort, and the measurements of characteristics of units that variables represent must also be made at particular times. For associational inference, however, the role of time is simply to affect the definition of the population of units or to specify the operational meaning of a particular variable. As we will see, in causal inference the role of time has a greater significance.

The most detailed information one can have in the model just described is the values of $Y(u)$ and $A(u)$ are all u in U . The *joint distribution* of Y and A over U is specified by $\Pr(Y = y, A = a) = \text{proportion of } u \text{ in } U \text{ for which } Y(u) = y \text{ and } A(u) = a$.

The *associational parameters* are determined by this joint distribution. For example, the conditional distribution of Y given A is specified by $\Pr(Y = y | A = a) = \Pr(Y = y, A = a) / \Pr(A = a)$. This conditional distribution describes how the *distribution* of Y values changes over U as A varies. A typical associational parameter is the regression of Y on A , that is, the conditional expectation $E(Y | A = a)$.

Associational inference consists of making statistical inferences (estimates, tests, posterior distributions, etc.) about the associational parameters relating Y and A on the basis of data gathered about Y and A from units in U . In this sense, associational inference is simply descriptive statistics.

3. RUBIN'S MODEL FOR CAUSAL INFERENCE

Because experimentation is such a powerful scientific and statistical tool and one that often introduces clarity into discussions of specific cases of causation, I unabashedly draw on the language and framework of experiments for the model for causal inference. It is not that I believe an experiment is the *only* proper setting for discussing causality, but I do feel that an experiment is the *simpliest* such setting. The purpose is to construct a model that is complex enough to allow us to formalize basic intuitions concerning cause and effect. The point of departure is the analysis of causal effects given in Rubin (1974, 1977, 1978, 1980). It will be sufficient for our purposes, however, to deal with a simplified, population-level version of Rubin's model. This simplified model was used in Holland and Rubin (1980) to analyze causal inference in retrospective, case-control studies used in medical research and in Holland and Rubin (1983) to analyze Lord's "analysis of covariance" paradox. I refer to this as "Rubin's model" even though Rubin would argue that the ideas behind the model have been around since Fisher. I think that Rubin (1974) was the place where these ideas were first applied to the study of causation.

This model also begins with a population of units, U . Units in the model for causal inference are the objects of study on which causes or treatments may act. The terms *cause* and *treatment* will be used interchangeably, and the notion that these terms convey is an important part of the model. It is important to realize that by using the terms cause and treatment interchangeably I do not intend to limit the discussion to the activities within a controlled

randomized study. I do it to emphasize an idea that I believe receives insufficient attention in general discussions of causation. This is the fact that the effect of a cause is *always* relative to another cause. For example, the phrase "A causes B" almost always means that A causes B relative to some other cause that includes the condition "not A." The terminology becomes rather tortured if we try to stick with the usual causal language, but it is straightforward if we use the language of experiments—treatment (i.e., one cause) versus control (i.e., another cause). In Section 7 I will discuss the fundamental question of what kinds of things can be causes. The key notion, however, is the *potential* (regardless of whether it can be achieved in practice or not) for exposing or not exposing each unit to the action of a cause. For causal inference, it is critical that each unit be *potentially exposable* to any one of the causes. As an example, the schooling a student receives can be a cause, in our sense, of the student's performance on a test, whereas the student's race or gender cannot.

For simplicity it shall be assumed in this article that there are just two causes or levels of treatment, denoted by t (the treatment) and c (the control). Let S be a variable that indicates the cause to which each unit in U is exposed; that is, $S = t$ indicates that the unit is exposed to t and $S = c$ indicates exposure to c . In a controlled study, S is constructed by the experimenter. In an uncontrolled study, S is determined to some extent by factors beyond the experimenter's control. In either case, the critical feature of the notion of cause in this model is that the value of $S(u)$ for each unit *could have been different*.

The variable S is analogous to the variable A in Section 2, but with the essential difference that $S(u)$ indicates exposure of u to a specific cause, whereas $A(u)$ can indicate a property or characteristic of u . In this case the value of $A(u)$ could not have been different.

The role of time now becomes important because of the fact that when a unit is exposed to a cause this must occur at some specific time or within a specific time period. Variables now divide into two classes: pre-exposure—those whose values are determined prior to exposure to the cause; post-exposure—those whose values are determined after exposure to the cause.

The role of a response variable Y is to measure the effect of the cause, and thus response variables must fall into the post-exposure class. This gives rise to another critical element of the model. The values of post-exposure variables are potentially affected by the particular cause, t or c , to which the unit is exposed. This is nothing less than the statement that causes have effects, which is the very heart of the notion of causation. For the model to represent faithfully this state of affairs we need not a *single* variable, Y , to represent a response but *two* variables, Y_t and Y_c , to represent two potential responses. The interpretation of these two values, $Y_t(u)$ and $Y_c(u)$ for a given unit u , is that $Y_t(u)$ is the value of the response that would be observed if the unit were exposed to t and $Y_c(u)$ is the value that would be observed *on the same unit* if it were exposed to c .

The notation $Y_t(u)$ and $Y_c(u)$ is sometimes confusing

because a variable usually represents a measurement of some sort and a measurement is usually thought of as the result of a process that is applied to a unit. This is not really correct. For post-exposure variables the measurement is applied to the pairing (u, t) (i.e., u after exposure to t) or to (u, c) (i.e., u after exposure to c). A notation that more nearly expresses this joint dependence of Y on u and the exposed cause is $Y_t(u) = Y(u, t)$ and $Y_c(u) = Y(u, c)$. I shall use the Y_t , Y_c notation, however, since it leads to simpler expressions.

The effect of the cause t on u as measured by Y and relative to cause c is the difference between $Y_t(u)$ and $Y_c(u)$. In the model this will be represented by the algebraic difference

$$Y_t(u) - Y_c(u). \quad (1)$$

I shall call the difference (1) the causal effect of t (relative to c) on u (as measured by Y). Expression (1) is the way that the model for causal inference expresses the most basic of all causal statements. It says that treatment t causes the effect $Y_t(u) - Y_c(u)$ on unit U (relative to treatment c) or more simply that

$$t \text{ causes the effect } Y_t(u) - Y_c(u). \quad (2)$$

Causal inference is ultimately concerned with the effects of causes on specific units, that is, with ascertaining the value of the causal effect in (1). It is frustrated by an inherent fact of observational life that I call the Fundamental Problem of Causal Inference.

Fundamental Problem of Causal Inference. It is impossible to observe the value of $Y_t(u)$ and $Y_c(u)$ on the same unit and, therefore, it is impossible to observe the effect of t on u .

The emphasis is on the word *observe*. The impossibility of observing both $Y_t(u)$ and $Y_c(u)$ is self-evident in some examples and less clear in others. For example, if the unit u is a specific fourth grader, t represents a novel year-long program of study of arithmetic, c represents a standard arithmetic program, and Y is a score on a test at the end of the year, then it is evident that we could observe either $Y_t(u)$ or $Y_c(u)$ but not both. We will never observe what the effect of t was on u . On the other hand, if u is a room in a house, t means that I flick on the light switch in that room, c means that I do not, and Y indicates whether the light is on or not a short time after applying either t or c , then I might be inclined to believe that I can know the values of both $Y_t(u)$ and $Y_c(u)$ by simply flicking the switch. It is clear, however, that it is only because of the plausibility of certain assumptions about the situation that this belief of mine can be shared by anyone else. If, for example, the light has been flicking off and on for no apparent reason while I am contemplating beginning this experiment, I might doubt that I would know the values of $Y_t(u)$ and $Y_c(u)$ after flicking on the switch—at least until I was clever enough to figure out a new experiment!

The implicit threat of the Fundamental Problem of Causal Inference is that causal inference is impossible. But we should not jump to that conclusion too quickly. By assert-

ing that the simultaneous observation of $Y_t(u)$ and $Y_c(u)$ is impossible I do not mean that knowledge relevant to these values is completely absent. It will depend on the situation considered. There are two general solutions to the Fundamental Problem, which for the sake of convenience I will label the *scientific solution* and the *statistical solution*.

The scientific solution is to exploit various homogeneity or invariance assumptions. For example, by studying the behavior of a piece of laboratory equipment carefully a scientist may come to believe that the value of $Y_t(u)$ measured at an earlier time is equal to the value of $Y_c(u)$ for the current experiment. All he needs to do now is to expose u to t and measure $Y_t(u)$ and he has overcome the Fundamental Problem of Causal Inference. Note, however, that this hypothetical scientist has made an untestable homogeneity assumption. By careful work he may convince himself and others that this assumption is right, but he can never be absolutely certain. Science has progressed very far by using this approach. The scientific solution is a commonplace aspect of our everyday life as well. We all use it to make the causal inferences that arise in our lives. These ideas are amplified in Sections 4.1 and 4.2.

The statistical solution is different and makes use of the population U in a typically statistical way. The *average causal effect*, T , of t (relative to c) over U is the expected value of the difference $Y_t(u) - Y_c(u)$ over the u 's in U ; that is,

$$E(Y_t - Y_c) = T. \quad (3)$$

T defined in (3) is the average causal effect. By the usual rules of probability (3) may also be expressed as

$$T = E(Y_t) - E(Y_c). \quad (4)$$

Although this does not look like much, (4) reveals that information on *different* units that *can be observed* can be used to gain knowledge about T . For example, if some units are exposed to t they may be used to give information about $E(Y_t)$ (because this is the mean value of Y_t over U), and if other units are exposed to c they may be used to give information about $E(Y_c)$. Formula (4) is then used to gain knowledge about T . The exact way that units would be selected for exposure to t or c is very important and involves all of the usual considerations of good statistical design of experiments. The important point is that the statistical solution replaces the impossible-to-observe causal effect of t on a specific unit with the possible-to-estimate *average causal effect* of t over a population of units. These ideas will be developed further in Sections 4.3 and 4.4.

The usefulness of either the scientific or the statistical solution to the Fundamental Problem of Causal Inference depends on the truth of different sets of untestable assumptions. In Section 4 I will discuss some of the typical assumptions that are often used to overcome the Fundamental Problem of Causal Inference.

It is useful to have a notation to express the fact that the causal indicator variable S determines which value, Y_t or Y_c , is observed for a given unit. If $S(u) = t$, then $Y_t(u)$ is observed, and if $S(u) = c$, then $Y_c(u)$ is observed. Thus

the observed response on unit u is $Y_{S(u)}(u)$. The *observed response variable* is, therefore, Y_S . Hence, even though the model contains three variables, S , Y_t , and Y_c , the process of observation involves only two, that is, S , Y_S . The distinction between (a) the measurement process, Y , that produces the response variable; (b) the two versions of the response variable Y_t and Y_c that corresponds to which cause the unit is exposed (and in terms of which causal effects are defined); and (c) the *observed* response variable Y_S , is very important and, often, is not made in discussions of causation. These distinctions never arise in the study of simple association, but they are crucial to the analysis of causation.

It is useful to review the model for associational inference and Rubin's model side by side to emphasize their differences. Both involve a population of units, U , and both involve two *observable* variables: (A , Y) for association and (S , Y_S) for causation. This is all, however, that they have in common. Whereas A and Y are simply variables defined on the units of U , S and Y_S presuppose a more complicated structure in order for them to apply to real situations. Two or more causes (or treatments) must be exposable to all of the units, and the response Y must be a post-exposure variable in order for the observed response Y_S to be defined. Associational inference involves the joint or conditional distributions of values of Y and A , and causal inference concerns the values $Y_t(u) - Y_c(u)$ on individual units. Causal inferences proceed from the observed values of S and Y_S and from assumptions that address the Fundamental Problem of Causal Inference but that are usually untestable. Causal inferences do not necessarily involve statistical inferences, but associational inferences almost always do.

4. SOME SPECIAL CASES OF CAUSAL INFERENCE

This section considers some simple special cases of Rubin's model for causal inference. The purpose is to show how specific assumptions added to the model allow causal inferences of particular types.

4.1 Temporal Stability and Causal Transience

One way of applying the scientific solution to the Fundamental Problem of Causal Inference is to assume that (a) the value of $Y_c(u)$ does not depend on *when* the sequence "apply c to u then measure Y on u " occurs and (b) the value of $Y_t(u)$ is not affected by the prior exposure of u to the sequence in (a). When these two assumptions are plausible it is a simple matter to measure $Y_t(u)$ and $Y_c(u)$ by sequential exposure of u to c then t , measuring Y after each exposure. The first assumption is *temporal stability*, because it asserts the constancy of response over time. The second assumption is *causal transience*, because it asserts that the effect of the cause c and the measurement process that results in $Y_c(u)$ is transient and does not change u enough to affect $Y_t(u)$ measured later. These two assumptions often apply to physical devices and are routinely made by all of us in everyday life—for example, in the "light switch" example mentioned earlier.

4.2 Unit Homogeneity

A second way of applying the scientific solution to the Fundamental Problem is to assume that $Y_t(u_1) = Y_t(u_2)$ and $Y_c(u_1) = Y_c(u_2)$ for two units u_1 and u_2 . This is the assumption of *unit homogeneity*. It, too, is often applicable to work done in scientific laboratories and is also a causal workhorse of everyday life. The causal effect of t is taken to be the value of $Y_t(u_1) - Y_c(u_2)$. One way that laboratory scientists convince themselves that the units are homogeneous is to prepare them carefully so that they "look" identical in all relevant aspects. This, of course, cannot prove that the unit homogeneity assumption is valid, but it can make this assumption plausible.

4.3 Independence

In my discussion of the statistical solution to the Fundamental Problem, I did not give any specification to the way that units might be selected for observation of Y_t or Y_c . I only indicated that it was very important. Of course, the most well-known way that this occurs in experimental work is by randomization, and this section is concerned with that topic.

The supposition in using the statistical solution is that the population U does not consist of one or two units but is "large" in some sense. The observed data for each unit are values of the pair of variables (S , Y_S).

The average causal effect T is the difference between the two expected values $E(Y_t)$ and $E(Y_c)$. The observed data (S , Y_S), however, can only give us information about

$$E(Y_S | S = t) = E(Y_t | S = t) \quad (5)$$

and

$$E(Y_S | S = c) = E(Y_c | S = c). \quad (6)$$

It is important to recognize that $E(Y_t)$ and $E(Y_t | S = t)$ are *not* the same thing and need not have the same values in general [similarly for $E(Y_c)$ and $E(Y_c | S = c)$]. To state this difference in words, $E(Y_t)$ is the average value of $Y_t(u)$ over all u in U , where $E(Y_t | S = t)$ is the average value of $Y_t(u)$ over only those in u in U that were exposed to t . There is no reason why, in general, these two averages should be equal. For example, if $S(u) = t$ for all units for which $Y_t(u)$ is small, then $E(Y_t | S = t)$ will be smaller than $E(Y_t)$.

There is, however, an assumption that, if plausible, makes these two expected values equal. It is the assumption of *independence*. When units are assigned at random either to cause t or to cause c , certain physical randomization processes are carried out so that the determination of which cause (t or c) u is exposed to is regarded as statistically independent of all other variables, including Y_t and Y_c . This means that if the physical randomization is carried out correctly, then it is plausible that S is independent of Y_t and Y_c and all other variables over U . This is the *independence assumption*. If this assumption holds, then we have the basic equations

$$E(Y_t) = E(Y_t | S = t) \quad (7)$$

and

$$E(Y_c) = E(Y_c | S = c). \quad (8)$$

Hence under the independence assumption the average causal effect T satisfies the equation

$$T = E(Y_s | S = t) - E(Y_s | S = c). \quad (9)$$

The data (S, Y_s) can now be used to estimate T by taking the difference between the average value of the observed response Y_s for the units with $S = t$ and for the units with $S = c$. Hence, if randomization is possible, the average causal effect T can always be estimated. If U is large, T can be estimated with high accuracy.

It is useful to have a name for the right side of Equation (9) even when the assumption of independence does not hold. I will call it the *prima facie causal effect* of t (relative to c) and denote it by

$$T_{PF} = E(Y_t | S = t) - E(Y_c | S = c), \quad (10)$$

which is algebraically equal to the following function of the regression of Y_s on S :

$$T_{PF} = E(Y_s | S = t) - E(Y_s | S = c). \quad (11)$$

The term *prima facie causal effect* is adapted from Suppes (see Sec. 5) and used here to distinguish (11) from the *true* average causal effect, T , defined in Equation (3). The prima facie causal effect is an associational parameter for the joint distribution of the observable pair (Y_s, S) . In general, the average causal effect T does not equal the prima facie causal effect T_{PF} . The assumption of independence, however, does allow the conclusion that $T = T_{PF}$, that is, Equation (9).

4.4 Constant Effect

The value of the average causal effect T is of potential interest for its own sake in certain types of studies. It would be of interest to a state education director who wanted to know what reading program would be the best to give to all of the first graders in his state. The average causal effect of the best program would be reflected in increases in statewide average reading scores.

The average causal effect T is an *average* and as such enjoys all of the advantages and disadvantages of averages. For example, if the variability in the causal effects $Y_t(u) - Y_c(u)$ is large over U , then T may not represent the causal effect of a specific unit, u_0 , very well. If u_0 is the unit of interest, then T may be irrelevant, no matter how carefully we estimate it!

The assumption of *constant effect* is that the effect of t on every unit is the same, and under this assumption we have the equation

$$T = Y_t(u) - Y_c(u), \quad \text{for all } u \text{ in } U. \quad (12)$$

Hence under the assumption of constant effect T is the causal effect for every unit in U . This assumption is also called *additivity* in statistical models for experiments because the treatment t adds a constant amount T to the control response for each unit.

The assumption of constant effect makes the value of the average causal effect relevant to every unit and, therefore, allows T to be used to draw causal inferences at the unit level.

The assumption of constant effect can be partially checked in the same way that the additivity assumption is usually investigated. For example, U can be divided into subpopulations U_1, U_2, \dots , and on each U_i the average causal effect can be estimated, T_1, T_2, \dots . If the T_i 's vary, the constant effect assumption cannot hold. If the T_i 's do not vary, then the constant effect assumption may be plausible.

The constant effect assumption is implied by the unit homogeneity assumption; that is, if $Y_t(u_1) = Y_t(u_2)$ and $Y_c(u_1) = Y_c(u_2)$, then clearly $Y_t(u_1) - Y_c(u_1) = Y_t(u_2) - Y_c(u_2)$. Hence we may view the constant effect assumption as a *weakening* of the assumption of unit homogeneity.

If we make only the constant effect assumption we may not conclude that the prima facie causal effect, T_{PF} , in (10) equals the average causal effect, T , in (3). To see this observe that under constant effect we have

$$Y_t(u) = Y_c(u) + T \quad (13)$$

for all units, u . Hence

$$E(Y_t | S = t) = T + E(Y_c | S = t), \quad (14)$$

so

$$T_{PF} = T + \{E(Y_c | S = t) - E(Y_c | S = c)\}. \quad (15)$$

The term in braces in (15) is not 0 in general, that is, if the independence assumption is not true.

It is easy to show that the stronger assumption of unit homogeneity does imply equality between T and T_{PF} .

4.5 Causal Inference in Nonrandomized Observational Studies

It is beyond the scope of this article to apply the model for causal inference to nonrandomized studies. This has been done extensively, and the reader is referred to Rubin (1974, 1977, 1978), Rosenbaum (1984a,b,c), Rosenbaum and Rubin (1983a,b, 1984a,b, 1985a,b), and Holland and Rubin (1980, 1983). An important emphasis in these papers is on the ways that *pre-exposure* variables can be used to replace the independence assumption with less stringent *conditional* independence assumptions that are useful in observational studies. Rosenbaum and Rubin referred to one such assumption as “strong ignorability.”

5. COMMENTS ON SELECTED PHILOSOPHERS

So much has been written about causality by philosophers that it is impossible to give an adequate coverage of the ideas that they have expressed in a short article. This section views some of these ideas in the context of Rubin's model for causal inference given in Sections 3 and 4. It makes no attempt to be exhaustive or even representative.

Aristotle distinguished four “causes” of a thing in his *Physics*: The *material* cause (that *out of which* the thing is made), the *formal* cause (that *into which* the thing is made), the *efficient* cause (that *makes* the thing), and the

final cause (that for which the thing is made). It is his notion of efficient cause that is relevant to our discussion and to most discussions of causation that grow out of inquiries into the methods of science. Locke (1690) proposed these definitions: "That which produces any simple or complex idea, we denote by the general name 'cause', and that which is produced, 'effect'." Although it is evident that these definitions refer to the same kinds of things that concern the model in Section 3, they do little more than suggest that the model is not out of line with an ancient philosophical tradition. It should be noted, however, that Aristotle emphasized the *causes* of a thing rather than the effects of causes. Locke seems a little more even-handed. Bunge (1959) gave a very accessible discussion of the history of many ideas about the essential meaning of causation.

5.1 Hume

When we turn to the analysis of causation given by Hume (1740, 1748) we find a critical basis for examining Rubin's model. Hume's analysis of causality is generally regarded to be an important contribution to the literature of this subject. Hume emphasized that causation is a relation between experiences rather than one between facts. He argued that it is not empirically verifiable that the cause produces the effect, but only that the experienced event called the cause is invariably followed by the experienced event called the effect. Hume's empirical stance can be regarded as sympathetic with the classical statistical view that the role of statistics is to draw inferences about unobserved quantities on the basis of observed facts. He was also very clear about the role of untestable assumptions in drawing causal conclusions.

Hume's analysis recognized three basic criteria for causation: (a) spatial/temporal contiguity, (b) temporal succession, and (c) constant conjunction. In the analysis of the idea that *A* causes *B* this means that (a) *A* and *B* are contiguous in space and time, (b) *A* precedes *B* in time, and (c) *A* and *B* always occur (or do not occur) together.

In terms of Rubin's model the first two of Hume's criteria are easily accommodated. The criterion of spatial/temporal contiguity is expressed in the model by the action of the cause and the measurement of the effect all taking place on a common entity, the unit. Since real entities must exist in space and time the contiguity criterion is satisfied and possibly clarified by the model. Temporal contiguity is relevant to the degree that the time period involved affects the unit. Spatial contiguity is often defined by the unit itself and may not involve simple "nearness."

The issue of temporal succession is shamelessly embraced by the model as one of the defining characteristics of a response variable. The idea that an effect might precede a cause in time is regarded as meaningless in the model, and apparently also by Hume.

Hume's notion of constant conjunction is more difficult simply because it might not hold for many reasons. In terms of the model there are two types of reasons why it might not hold. One of these involves "measurement error," and the other is more fundamental and involves the structure

of the model. Measurement error often creates violations of constant conjunction in real scientific investigations. We may think we have a case of "*A* and not *B*" but we really have a case of "*A'* and not *B*" for some *A'* that we mistook for *A* (similarly for examples of "not *A* and *B*"). In the model these "errors of measurement" can involve both the causes and the response variable that determines the effect. The other, more fundamental way that constant conjunction can fail in the model is for the constant effect assumption to fail to hold, that is, for the causal effects $Y_t(u) - Y_c(u)$ to vary with the unit *u*. Hence, if we disregard those cases of nonconstant conjunction that are due to measurement error, we see that Hume's third criterion requires the constant effect assumption to hold in our model. Hume would probably argue that any weakening of this assumption would allow cases that he would not call "causation" into the model. We will have to be satisfied that at least Hume's analysis fits into the model and let others judge the utility of the constant effect assumption. I should point out that the distinction between constant and variable causal effects (a) is often not easy to prove one way or the other in a particular case and (b) has been at the heart of at least one important controversy in the history of statistics (see Sec. 6).

What I see that is missing from Hume's analysis is any notion that the effect of cause is always relative to another cause. The notion that a cause could have been different from what it was and that it is this difference that defines the effect is completely missing from Hume. In Hume's analysis causes are not delineated in any way. Anything can be a cause. The importance of this point will be emphasized in Section 7. Finally, Hume does not identify the idea of an experiment as related to or important for causation.

5.2 Mill

John Stuart Mill is rather different in this regard. Mill (1843) was positively disposed toward experiments.

Observation, in short, without experimentation (supposing no aid from deduction) can ascertain sequences and co-existences, but cannot prove causation. (p. 253)

... we have not yet proved that antecedent to be the cause until we have reversed the process and produced the effect by means of that antecedent artificially, and if, when we do so, the effect follows, the induction is complete. . . . (p. 252)

Mill is credited with codifying and elaborating on several methods of experimental inquiry that had been put forth by Sir Francis Bacon 250 years earlier. Mill identified four general methods, which I now discuss.

The Method of Concomitant Variation. This method flies in the face of the distinctions that I have drawn between association and causation.

Whatever phenomenon varies in any manner, whenever another phenomenon varies in some particular manner, is either a cause or an effect of that phenomenon, or is connected with it through some fact of causation. (p. 464)

I think that as a method of science the widespread use of this method is indisputable. Most scientists would agree

that where there is correlational smoke there is likely to be causalional fire. Most would not, however, go as far as Mill's statement of the method.

Of course, even if Rubin's model does apply, the *correlation* between the observed variables S and Y_s does not say much about the causal effects or even the average causal effect, because the correlation of Y_s and S is simply another way of expressing the *prima facie* causal effect, T_{PF} .

More generally, not everything can be a "cause" in the sense used in the model, but Mill's method of concomitant variation can be applied to cases for which only association is appropriate. That this can result in nonsense discussions of causation is well known.

Method of Difference. This method is almost an exact statement of what we mean by a causal effect, even though it is couched in a more general language and its proposed use is to identify causes and effects.

If an instance in which the phenomenon under investigation occurs, and an instance in which it does not occur, have every circumstance in common save one, that one occurring in the former; the circumstances in which alone the two instances differ, is the effect, or the cause, or an indispensable part of the cause of the phenomenon. (p. 452)

If we restrict our attention to the following interpretation of the elements of this quotation we see a fairly straightforward definition of causal effect: "phenomenon under investigation" occurs— $Y = 1$; "phenomenon under investigation" does not occur— $Y = 0$; "the circumstance in which the instances differ"—when present = t , when absent = c . Then $Y_t(u) = 1$ denotes the fact that when the circumstance was present the phenomenon occurs, and $Y_c(u) = 0$ denotes the fact that when the circumstance was absent the phenomenon did not occur. The equality of all other circumstances is modeled by considering the same unit. Thus $Y_t(u) - Y_c(u) = 1$, so the causal effect of the circumstance on the unit is 1 and corresponds to Mill's statement that the circumstance is "the cause or an indispensable part of the cause of the phenomenon."

Mill also considered reversing the process to look for causes of given effects. This is a well-known scientific technique—for example, it occurs often in epidemiological studies of public health problems. It is beyond the scope of this article to apply the model to such a case, but some work along this line can be found in Hamilton (1979) and Holland and Rubin (1980).

The Method of Residues. This method also applies fairly simply to the model. Its statement is

Subduct from any phenomenon such part as is known by previous inductions to be the effect of certain antecedents, and the residue of the phenomenon is the effect of the remaining antecedents. (p. 460)

To place this into the context of the model let the antecedents (i.e., causes) be denoted by a = those whose effect is known and b = the remaining antecedents.

The causal effect of ab relative to a is simply $Y_{ab}(u) - Y_a(u)$, which is the residue Mill tells us to compute. I regard Mill's method of residues to be a nearly explicit, early statement of the definition of causal effect.

The Method of Agreement. Usually this method is dis-

cussed first because it is so clearly a part of scientific investigations. I have left it to the end because it requires the introduction of the notion of a "null effect." The method is stated as follows:

If two or more instances of a phenomenon under investigation have only one circumstance in common, the circumstance in which alone all the instances agree, is the cause (or effect) of the given phenomenon. (p. 451)

Although it looks like a method for identifying the cause of a phenomenon, it is clear to anyone who has ever used the method of agreement that all that the method really does is to *rule out* possible causes. It is this aspect of the method of agreement that fits into the model.

If, as in the discussion of the method of difference, we let $Y = 1$ (or 0) denote the occurrence (or not) of "the phenomenon under investigation," and then if the phenomenon occurs when the cause t occurs and also when the cause t does not occur, that is, c , we have

$$Y_t(u) = 1 \quad \text{and} \quad Y_c(u) = 1,$$

so

$$Y_t(u) - Y_c(u) = 0.$$

Hence the causal effect of t is 0; that is, t is a cause with a *null effect*. The principle of causality states that every phenomenon has a cause; that is, every effect has a cause. Every practicing experimentalist can attest to the fact that the reverse is not true—experiments fail. Causes do not necessarily have effects. Null effects are the stuff from which null hypotheses are made!

My conclusion is that Mill's thinking, being driven by an experimental model, is in reasonably close agreement with the model of Section 3. He is close to the idea that the effect of a cause is always relative to another cause, unlike Hume. Like Hume, however, he does not restrict the notion of cause in any way. For Hume and Mill any phenomenon can be a cause. Finally, like Hume, Mill does not consider variation (i.e., either unit inhomogeneity or variable causal effects) in any serious way.

5.3 Suppes

Variation is an explicit consideration in Patrick Suppes's (1970) *probabilistic theory of causality*. Suppes's goal was to improve upon Hume's analysis, specifically the constant conjunction criterion, because

... in restricting himself to the concept of constant conjunction, Hume was not fair to the use of causal notions in ordinary language and experience. (p. 10)

Like Hume, Suppes puts no restriction on what causes and effects *are* save only that they be expressible as events that occur in time. Thus Suppes uses the language of stochastic processes to formalize his theory. He explained the intuitive idea of his theory as follows:

Roughly speaking, the modification of Hume's analysis I propose is to say that one event is the cause of another if the appearance of the first event is followed with a high probability by the appearance of the second, and there is no third event that we can use to factor out the probability relationship between the first and second events. (p. 10)

Suppes expressly adopted the temporal succession cri-

terion that all causes precede their effects in time. He first defined a *prima facie cause* of an event as an event that temporally precedes it and that is positively associated with it. He then defined a *spurious cause* of an effect (i.e., an event) as a *prima facie cause* of the effect that is, in fact, conditionally independent of the effect given a second event that is temporally prior to the *prima facie cause* and that is conditionally positively associated with the effect given the *prima facie cause*. This is what he meant by “factoring out” a probability relationship. A *genuine cause* is a *prima facie cause* that is not spurious.

More precisely Suppes's definitions are as follows:

(S1) If $r < s$ denote two time values, the event C_r is a *prima facie cause* of the event E_s , if

$$\Pr(E_s | C_r) > \Pr(E_s). \quad (16)$$

(S2) C_r is a *spurious cause* of E_s if C_r is a *prima facie cause* of E_s and for some $q < r < s$ there is an event D_q such that

$$\Pr(E_s | C_r, D_q) = \Pr(E_s | D_q) \quad (17)$$

and

$$\Pr(E_s | C_r, D_q) \geq \Pr(E_s | C_r). \quad (18)$$

(S3) C_r is a *genuine cause* of E_s if C_r is a *prima facie cause* of E_s but C_r is not a *spurious cause* of E_s .

In all of these definitions the probabilities of the events used in the conditioning statements are assumed to be positive. Suppes also considered other issues, such as direct and indirect causes, but (S1)–(S3) are the main elements of his theory.

It is clear that Suppes's analysis is quite different from that given in Section 3. He defined the cause of an effect rather than the effect of a cause. Like Hume and Mill he placed no general restriction on the nature of a cause other than that it be expressible as an event that occurs prior in time to the effect. There is no explicit place for units in Suppes's stochastic process model—they are buried in the probability space on which the events he considered are defined. Hence Suppes does not have the machinery to express the effect of a cause in a particular case. His model describes average behavior, not individual behavior.

At bottom, Suppes's notion of a genuine cause is simply a correlation between a cause and effect that will not go away by “partialling out” legitimate competing causes. In a sense then for Suppes all genuine causes are only temporarily so as they await the cleverness of the analyst to identify the proper conditioning event that will render null their association with the effect. Although this may, indeed, describe much informal scientific practice, it does not appear to me to get to the heart of the notion of causation, which, I believe, Rubin's model does.

Suppes's theory, however, does capture some useful ideas, and because it is stated with precision it is a fairly easy task to relate these ideas to Rubin's model.

In what follows, all probabilities and expectations are computed over the population U of units.

Earlier, his notion of a *prima facie cause* was translated

into the *prima facie causal effect* as follows. The association between the observed response Y_S and the causal indicator S can be measured by the difference in the average value of the response between the units exposed to t and those exposed to c . We have called this the *prima facie causal effect* of t (relative to c), that is,

$$T_{PF} = E(Y_S | S = t) - E(Y_S | S = c). \quad (19)$$

We have seen that the association between cause and effect that defines a *prima facie cause* is a causal effect under certain conditions that have wide use in science, but T_{PF} is not always a causal effect. This is why Suppes defined *prima facie causes*.

I will finish this section by showing what happens when we apply Suppes's notion of a spurious cause to the context of a randomized experiment. This will shed some light on the relation of his theory to Rubin's model.

If the response variable Y is a 0/1 indicator, then we may keep the discussion in terms of the event terminology that Suppes used. Thus $\{Y_S = 1\}$ corresponds to E_s and $\{S = t\}$ corresponds to C_r , and I will discuss the meaning of the event D_q subsequently.

Consider Equation (17) from (S2). For a randomized experiment it is

$$\Pr(Y_S = 1 | S = t, D_q) = \Pr(Y_S = 1 | D_q). \quad (20)$$

By using the usual rules for handling conditional probabilities we may express (20) as follows:

$$\begin{aligned} &\{\Pr(Y_t = 1 | S = t, D_q) - \Pr(Y_c = 1 | S = c, D_q)\} \\ &\quad \times \Pr(S = c | D_q) = 0. \end{aligned} \quad (21)$$

Hence the only way that Equation (20) can hold is for either

$$\Pr(S = t | D_q) = 1 \quad (22)$$

or

$$\Pr(Y_t = 1 | S = t, D_q) = \Pr(Y_c = 1 | S = c, D_q). \quad (23)$$

If D_q is an event that occurs prior in time to the exposure of the units to t or c , then I will assume that D_q is determined by the values of *pre-exposure* variables defined on the units in U . Now suppose that the assumption of independence holds so that S is statistically independent of Y_t , Y_c and of the pre-exposure variables that define D_q . Furthermore, suppose that

$$0 < \Pr(S = t) < 1, \quad (24)$$

so each unit has positive probability of being exposed to either cause. The independence assumption and (24) then imply that (22) cannot hold and that Equation (17), therefore, reduces to

$$\Pr(Y_t = 1 | D_q) = \Pr(Y_c = 1 | D_q). \quad (25)$$

Because Y is an indicator variable we can rewrite (25) in terms of an average causal effect; that is,

$$T(D_q) = E(Y_t - Y_c | D_q) = 0. \quad (26)$$

The average causal effect $T(D_q)$ in (26) is the average

causal effect over all units in U for which the event D_q occurs. Hence we see that Suppes's condition (17) for a spurious cause reduces to the condition

$$T(D_q) = 0 \quad (27)$$

in a randomized experiment. The other condition that Suppes required in (S2) is inequality (18), which is, in the present context, equivalent to

$$\Pr(Y_s = 1 | S = t, D_q) \geq \Pr(Y_s = 1 | S = t). \quad (28)$$

Under randomization this becomes

$$\Pr(Y_t = 1 | D_q) \geq \Pr(Y_t = 1). \quad (29)$$

If we put (29) and (27) together with the condition that t be a *prima facie* cause we find that the treatment in a randomized experiment is a spurious cause of the effect if and only if it has a positive average causal effect, but a subpopulation of units can be identified on the basis of pre-exposure variables (a) on which the average causal effect is 0 and (b) for which the response under t is more likely to occur than it is for all of U . I think that part (a) is more accurately described as a null effect in the subpopulation and part (b) is unrelated to the notion of cause. The existence of a subpopulation on which the effect is null while the overall effect is positive is an example of nonconstant conjunction in Hume's sense. It would be called an *interaction* by most statisticians.

6. COMMENTS FROM A FEW STATISTICIANS

This section is devoted to a brief examination of the writings of a few statisticians to see in what way the idea of multiple versions of the response, that is, Y , and Y_c , has appeared before. I find that many people have difficulty with the idea of distinguishing Y_t and Y_c from Y or Y_s and perhaps this look at earlier work may help clarify this assumption. Unfortunately, the exact idea is never stated explicitly, so there is a need for a certain amount of detective work to find it. I hope I will not be held guilty of wrongly reinterpreting the work of others.

A fairly clear statement of this idea was given by Kempthorne (1952) in a discussion of the analysis of randomized block designs. (A randomized block design is a typical agricultural experimental plan in which larger tracts of land, called blocks, are each subdivided into p plots and then one of the experimental treatments is applied at random to each of the p plots within each block.) For example, Kempthorne (1952, p. 136) first defined *yields* as follows: "We shall denote the yield with treatment k . . . on plot j . . . of block i . . . by y_{ijk} ." He then wrote:

In fact we do not observe the yield of treatment k on plot j but merely the yield of treatment k on a randomly chosen plot in the block. . . . we denote the observed yield of treatment k in block i by y_{ik} . (p. 137)

It seems evident from the two quotations that the y_{ijk} in the first refers to different versions of the response—one for each k —on each combination (i, j) of plot within block. The y_{ik} in the second quotation is the value of y_{ijk} for that plot to which treatment k is actually applied in block i .

It is not difficult to make the following translation of

Kempthorne's notation. The units are the "plots," so the units need two subscripts for identification; that is, u_{ij} is the j th plot within block i . The yield of treatment k on the unit u_{ij} is $y_{ijk} = Y_k(u_{ij})$, where $Y_k(u)$ is the value of the response that is observed if u is exposed to treatment k . The randomization process picks one of the treatments to apply to unit u_{ij} , and this can be indicated by $S(u_{ij})$; that is, if treatment k is applied to unit u_{ij} then $S(u_{ij}) = k$. The observed yield on u_{ij} is

$$y_{ijS(u_{ij})} = Y_{S(u_{ij})}(u_{ij}).$$

The plot in block i to which treatment k is applied can be denoted by j_k so that the observed yield of treatment k on block i is

$$y_{ik} = Y_k(u_{ij_k}).$$

In D. R. Cox's (1958) book on the planning of experiments he defined *true treatment effects* in an experiment in almost exactly the same way that we have defined causal effects. In an experiment with treatments T_1, T_2 , he defined the true treatment effects as the difference between "the observation obtained on any unit when, say, T_1 is applied" and "the observation that would have been observed had, say, T_2 been applied" (p. 15). Hence Cox appears to have accepted the idea that the response of a unit could be one value, $Y_t(u)$, if the unit were exposed to t and another, possibly different value, $Y_c(u)$, if the unit were exposed to c . Cox also made the assumption of constant effect in defining true treatment effects. His reasons for this are not clear but appear to be primarily technical rather than conceptual. He did not reject the idea of variable causal effects, however, and discussed ways in which causal effects might depend "on the value of some supplementary measurement that can be made on each unit" (p. 18).

Curiously, R. A. Fisher, who founded the modern theory of experimental design, never dealt directly with the idea of multiple versions of the response. Instead, he gave examples that are so laced with specific details that it is not always clear what level of generality he meant to convey. For example, in the first article in which Fisher (1926) attempted to set out the principles of the design of field experiments in agriculture we find this question in a discussion of a hypothetical experiment to evaluate the apparent productive value of treating a given acre of ground with a manurial treatment:

What reason is there to think that, even if no manure had been applied, the acre which actually received it would not still have given the higher yield? (p. 504)

It is fairly clear in this quotation that he could consider the possibility that had a different treatment (i.e., no manure) been applied to the field the resulting yield might have been the same. This clearly concerns the null hypothesis of no treatment effect and, more generally, Fisher came closest to the idea of multiple versions of the response in his discussions of the relationship between the null hypothesis and randomization.

The earliest explicit reference that I have found to multiple versions of the response is Neyman (1935). In his paper (read before the Industrial and Agricultural Re-

search Section of the Royal Statistical Society in March of 1935) Neyman gave an explicit statement of the idea of multiple versions of the response (which is for Neyman the yield from an experimental plot of land in an agricultural experiment). Unfortunately, Neyman's discussion also introduced the notion of a stochastic element that is added to Y to allow for "technical errors" that are due to inaccuracies of experimental technique. If we ignore this problem of measurement error and assume zero "technical errors," then Neyman's definition of a "true yield" explicitly refers to multiple versions of the response. "Thus $X_{ij}(k)$ will mean the 'true' yield of the k th object obtainable from the plot (i, j) " (p. 110; by "object" Neyman means treatment). His notation is very similar to that used by Kempthorne. To put it into the notation of Section 3, the units are the plots, u_{ij} , and $X_{ij}(k) = Y_k(u_{ij})$, where $Y_k(u)$ is defined as in the previous discussion of Kempthorne.

Neyman also had an explicit expression for the average value of $X_{ij}(k)$ over all of the units, u_{ij} . It is $X..(k)$. In the notation of Section 3 this is $X..(k) = E(Y_k)$. Hence it is clear that by the time Neyman was writing the idea of multiple versions of the response, one for each treatment, was established. It seems to have been used by writers concerned about the details of the effects of randomization in specific experimental plans (e.g., Cox 1958; Kempthorne 1952) but is generally not a part of the standard statistical notation of many other writers [an exception is Hamilton (1979)].

The Neyman (1935) reference is also relevant to the model in Section 3 because of the controversy between Fisher and Neyman that it engendered. The controversy revolves around the choice of null hypothesis in experiments such as randomized block designs. Fisher was quite clear that the null hypothesis that he proposed is that the causal effect (as we have defined it) is 0 for each unit. For example, in the famous discussion at the end of Neyman (1935) Fisher first quoted Neyman, as follows:

... this bias vanishes when . . . the objects compared are reacting to differences in soil fertility in exactly the same manner. . . . This is not always true. (p. 153)

Then Fisher wrote:

However, it was always true in the case for which it was required, namely, when the hypothesis to be tested was true, that differences of treatment made no difference to the yields. (p. 157)

Then Neyman, in responding to Fisher's remarks, emphasized his interest in what I would call the average causal effect.

'Our purpose in the field experiment consists in comparing numbers such as $X..(k)$, or the average true yields which our objects are able to give when applied to the whole field.' It is seen that this problem is essentially different from what Professor Fisher suggested. So long as the average yields of any treatments are identical, the question as to whether these treatments affect separate yields on single plots seems to be uninteresting and academic. (p. 173)

Fisher's sardonic reply indicates that, at least, he agreed that Neyman stated their differences clearly. "It may be foolish, but that is what the z test was designed for, and the only purpose for which it has been used" (p. 173).

Evidently, I would conclude that Neyman's null hypoth-

esis is one of zero average causal effect, that is, $E(Y_c - Y_c) = 0$, whereas Fisher's is one of zero causal effect for all units, that is, $Y_c(u) - Y_c(u) = 0$ for all $u \in U$.

7. WHAT CAN BE A CAUSE?

It may seem very extreme to some to limit the notion of *cause* to the sense used in Section 3. Aristotle set the stage for this, however, by distinguishing more than one meaning to the word *cause*. It might be better to ask, what can be an "efficient cause" in his sense? Evidently even this restriction did not limit the notion of cause for such thinkers as Hume and Mill. Anything can be a cause for them—or, at least, a potential cause.

Put as bluntly and as contentiously as possible, in this article I take the position that causes are only those things that could, in principle, be treatments in experiments. The qualification "in principle" is important because practical, ethical, and other considerations might make some experiments infeasible, that is, limit us to contemplating *hypothetical experiments*. For example, in the medical and social world we might be able to conceive of an experiment, but no one would ever try to carry it out. Instead, we might have to wait for a "natural experiment" to occur. "Observational study" is the term used by statisticians (e.g., Cochran 1983) to refer to studies for which "The objective is to study the causal effects of certain agents" but "For one reason or another the investigator can not . . . impose on . . . or withhold from the subject, a treatment whose effects he desires to discover" (p. 1).

I believe that the notion of cause that operates in an experiment and in an observational study is the same. The difference is in the degree of control an *experimenter* has over the phenomena under investigation compared with that which an *observer* has. In Rubin's model this is expressed by the joint distribution of S with Y , and Y_c . Total control can make S independent of Y , and Y_c .

It may bother some readers that I have been using the term "experiment" in a very restricted sense—though one that is common in the study of the design of experiments. For example, experiments in chemistry in which a substance is analyzed into its component ingredients or in which ingredients are combined with each other to synthesize a new substance often may not have clearly identifiable units, treatments, and response variables. My view is that in such experiments the Aristotelian notion of *material cause* is often more relevant than that of *efficient cause*, and hence such experiments are not concerned with the notion of cause that is discussed in this article.

To return to the question of what can be a cause let me consider three examples of statements that involve the word *cause* but that vary in its exact usage.

- (A) She did well on the exam because she is a woman.
- (B) She did well on the exam because she studied for it.
- (C) She did well on the exam because she was coached by her teacher.

I think that these statements, even though they are perfectly understandable English sentences, vary in the mean-

ing of the "because" in each. In each, the effect, using the term loosely, is the same—doing well on an exam. The causes, again using the term loosely, are different. In (A) the "cause" is ascribed to an attribute she possesses. In (B) the "cause" is ascribed to some voluntary activity she performed, and in (C) it is ascribed to an activity that was imposed on her.

An attribute cannot be a cause in an experiment, because the notion of *potential exposability* does not apply to it. The only way for an attribute to change its value is for the unit to change in some way and no longer be the same unit. Statements of "causation" that involve attributes as "causes" are always statements of association between the values of an attribute and a response variable across the units in a population. In (A) all that is meant is that the performance of women on the exam exceeds, in some sense, that of men.

Examples of the confusion between attributes and causes fill the social science literature. Saris and Stronhorst (1984) gave the following example of a causal hypothesis: "Scholastic achievement affects the choice of secondary school" (p. 13). These authors clearly intended for this hypothesis to state that an *attribute* of a student (i.e., scores on tests, performance in primary school) can *cause* (i.e., affect) the student's choice of a particular type of secondary school. It is difficult to conceive of how scholastic achievement could be a treatment in an experiment and, therefore, be a "cause" in the sense used in this article. A somewhat stronger statement of my point was given by Kempthorne (1978, p. 15): "It is epistemological nonsense to talk about one trait of an individual *causing* or determining another trait of the individual."

At the other extreme is Example (C). This is easily interpreted in terms of the model. The interpretation is that had she not been coached by her teacher she would not have done as well as she did. It implies a comparison between the responses to two causes, even though this comparison is not explicitly stated.

Example (B) is just one of many types of examples in which the applicability of the model is not absolutely clear, and it shows one reason why arguments over what constitutes a proper causal inference can rage without any definitive resolution.

In (B) the problem arises because of the voluntary aspect of the supposed cause—studying for the exam. It is not clear that we could expose a person to studying or not in any verifiable sense. We might be able to *prevent* her from studying, but that would change the sense of (B) to something much more like (C). We could operationally define studying as so many hours of "nose in book," but that just defines an attribute we could measure on a subject. In my opinion the application of the model to statement (B) is problematical and not easily resolved. The voluntary nature of much of human activity makes causal statements about these activities difficult in many cases.

The voluntary aspect of the "cause" in (B) is not the only source of difficulty in deciding on the applicability of Rubin's model to specific problems. It is, however, a common source of difficulty.

The general problem, I think, is in deciding when something is an *attribute* of units and when it is a *cause* that can act on units. In the former case all that can be discussed is association, whereas in the latter case it is possible, at least, to *contemplate* measuring causal effects.

One may view Fisher's (1957) attack on those who used the association between smoking and lung cancer as evidence of a "causal link" between them as an example of the difficulty in deciding whether or not smoking is an attribute or a cause. Certainly the data that began this debate are purely associational. Doll and Hill's studies (1950, 1952, 1956) ascertained only smoking status and lung cancer status on sets of subjects. Fisher argued that smoking might only be indicative of certain genetic differences between smokers and nonsmokers and that these genetic differences could be related to the development or not of lung cancer. Fisher (1957) did feel that "a good *prima facie* case had been made for further investigation."

The response to Fisher's criticism can also be viewed as attempting to show that smoking should be thought of in causal terms rather than as indicative of a genetic attribute of subjects. For example, among his responses to Fisher, McCurdy (1957) pointed out that lung cancer rates increase with the *amount* of smoking and that subjects who stopped smoking had lower lung cancer rates than those who did not. Both of these arguments can be viewed as emphasizing the causal aspects of smoking—one can do more or less of it and one might stop doing it. A discussion of the entire debate was given by Cook (1980).

8. COMMENTS ON CAUSAL INFERENCES IN VARIOUS DISCIPLINES

This section will briefly consider discussions of causation in three disciplines—medicine, economics, and "causal modeling." In each case an attempt will be made to relate the discussion to Rubin's model for causal inference, but no attempt is made to be exhaustive or even representative in the selection of topics considered.

8.1 Causation and Medicine

We begin with a simple, yet basic, example from medicine—the establishment of specific bacteria as the cause of specific infectious diseases. Yerushalmy and Palmer (1959) described the situation in the following terms:

Almost from the very beginning, when bacteria were first found to cause disease, bacteriologists felt the need for a set of rules to act as guideposts in investigation of bacteria as possible causal agents in disease. (p. 28)

These two authors described three postulates formulated by the great bacteriologist, Robert Koch, who discovered, among other things, the tuberculosis bacillus in 1882. Koch's postulates [also called the Koch–Henle postulates, Evans (1978)] are simple, no-nonsense criteria for deciding when a microscopic organism is implicated in a disease. According to Yerushalmy and Palmer (1959), "while there is no single formulation of Koch's postulates—they can be stated as consisting essentially of the following:

- I. The organism must be found in all cases of the disease in question.

- II. It must be isolated from patients and grown in pure culture.
- III. When the pure culture is inoculated into susceptible animals or man, it must reproduce the disease." (p. 30)

Rubin's model applies rather clearly to Postulates I and III. Postulate I is simply Mill's method of agreement applied to this problem. It ensures that there are no data to support a null causal effect in this case—that is, if there were bona fide cases of the disease in which the organism was not present, along with other cases of the disease in which it was, then assuming unit homogeneity we would have an estimate of zero causal effect for the presence of the organism relative to its absence. Postulate III is like the light switch example—put in the organism and the disease occurs. The validity of this postulate stems from the unstated assumption that had the animal or human not been inoculated with the culture the disease would not have been expected to occur. Note that the word "susceptible" has crept in, presumably to deal with the inevitable "non-constant conjunction" of real laboratory work—in this case, the immune system.

Koch's second postulate relates more to good experimental techniques than to causal inference. If the organism is isolated from patients and grown in pure culture, then when it comes time to inoculate animals or people with it the experimenter knows what the inoculant is in fairly exact terms. In a sense, Postulate II is a way of minimizing measurement error in the treatment (t) that is exposed to the units.

Medicine is more difficult when the biological theory is less well developed. As an example I now consider several suggestions made by Sir Austin Bradford Hill to those who might wish to separate association from causation in the study of the environment and disease. He had spent a lifetime in public health and was among the first to argue, quantitatively, for the causal link between smoking and lung cancer (Doll and Hill 1950, 1952, 1956). Hill (1965) named nine factors that he felt were useful in such work for deciding that the most likely interpretation of an observed association is causation. I will consider these in an order that differs from Hill's.

Temporality. "Which is the cart and which the horse?" (Hill 1965, p. 297). Hill felt that while the time sequence of events, cause preceding effect, might not be difficult to establish in many cases, "it certainly needs to be remembered, particularly with selective factors at work in industry" (p. 298). Clearly, temporal succession is a given for Hill.

Experiment. In this category Hill placed the occasional "natural experiment" that gives strong evidence for causation. He had in mind the effect of preventative actions taken to reduce the incidence of the disease. Do they work? If a person stops smoking does he lower his risk of lung cancer? Hill clearly views such "experiments" in the same way Mill viewed the production of an effect by artificially introducing the presumed causal agent—strong causal evidence when you can find it.

Biological Gradient. By this Hill referred to evidence that showed an increasing disease rate as exposure to the agent in question intensified. Both experiment and biological gradient may be viewed as emphasizing the causal nature of the proposed causal agent, as discussed in the previous section.

Plausibility, Coherence, Analogy. I have grouped these three together because they all refer to the prior knowledge that the epidemiologist would need to consider. Is the suspected causation biologically *plausible*? Is it *coherent* in the sense of not being seriously in conflict with known facts? Is it *analogous* to known causal relations for similar agents and diseases? These factors, although important in some cases, all reflect the state of relevant scientific knowledge and do not directly translate into aspects of the model of Section 3. In particular Hill felt that it was unwise to place undue emphasis on these because of the relatively poor state of relevant biological knowledge in many cases of interest.

Although Hill felt that the six factors listed above were important from time to time, they were the six least significant factors on his list. He felt that the three most important factors are the *strength*, *consistency*, and *specificity* of the association in question.

Strength. This is Hill's first factor—"First upon my list I would put the strength of the association" (p. 295). This may be viewed as simple acceptance of Mill's method of concomitant variation in practical terms or of the scientific utility of the *prima facie* causal effect. Although there is no guarantee for this, it is often more likely that a larger *prima facie* causal effect will hold up when a controlled study is performed than will a smaller *prima facie* causal effect. A relevant result in this regard is the inequality given in Cornfield et al. (1959) that bounds the influence of unmeasured factors on the relative risk (a form of *prima facie* causal effect).

Consistency. Hill's second significant factor concerns the generality of the association across populations of units. This might be viewed as a weakened form of constant conjunction. At the very least, an association that is present in one population and absent in another suggests variable causal effects. I think that there is a clear bias against calling variable causal effects "causal" by scientists, even though those who must deal with heterogeneous units, such as humans, will generally agree that it is usually too much to expect constant effects in the real world.

Specificity. Hill's third factor refers to specific causes having specific effects.

If . . . the association is limited to specific workers and to particular sites and types of disease and there is no association between the work and other modes of dying, then clearly that is a strong argument in favor of causation. (p. 297)

I think that specificity is related to the believability of the independence assumption. The lack of an association between the exposure of a person to a particular work place and the causes of that person's death supports the independence assumption in a relevant way (but does not prove

the assumption is valid). Since the independence assumption implies that the *prima facie* causal effect equals the average causal effect, *specificity*, in conjunction with the strong association, may well be convincing evidence of a strong causal connection. Lack of specificity, however, does not disprove the independence assumption in many cases, and this explains why lack of specificity is not regarded as a serious problem by Hill.

In short, if specificity exists we may be able to draw conclusions without hesitation; if it is not apparent, we are not thereby necessarily left sitting on the fence. (p. 297)

Of course, specificity does not *guarantee* that the independence assumption is valid, but it does not directly contradict this assumption in the way that a lack of specificity does.

8.2 Granger Causation in Economics

The primary source of data that is available to economists is so-called “time series” data in which measurements of a variable or set of variables are made repeatedly on an economic entity over time. For such data, Granger (1969) developed a particular notion of causality that some economists have found useful in their analyses.

In my opinion, however, Granger’s essential ideas involving causation do not require the time-series setting he adopted. I will try to restate his theory in terms of the types of models used in Sections 2 and 3—that is, variables defined on a population of units. Granger formulated his theory around the idea of prediction—a “cause” ought to improve our ability to predict an effect in a probabilistic system. In Granger’s theory a variable *causes* another variable; that is, the values of one variable improve one’s ability to predict the future values of another variable. The only important way that his theory used the time-series setting was to separate variables into those whose values are determined prior to, at, or after a given point in time. I will simply adopt these temporal distinctions in the definitions of the variables that arise. Granger (1969, p. 430) clearly accepted the idea of temporal succession in his analysis: “In the author’s opinion there is little use in the practice of attempting to discuss causality without introducing time.” It is the past values of a variable that cause, in Granger’s sense, the future values of another variable.

Although Granger originally formulated his theory in terms of one variable causing another, later writers (e.g., Florens and Mouchart 1985) restated it in terms of non-causality and I will follow that approach. In reformulating his theory I will also shift from his emphasis on a particular type of predictor, that is, “the optimum, unbiased, least-squares predictor” (p. 428), to the more generally applicable notion of conditional statistical independence. This means that instead of limiting attention to the inability of a specific predictor to predict the values of a variable, I will use the stronger condition that *no* predictor can predict the desired values. Although this is a stronger type of non-causality than Granger defined I do not believe that this unduly distorts Granger’s theory and it certainly generalizes its applicability—indeed, see Granger (1980).

If X , Y , and Z denote three (possibly vector-valued) variables defined on a population, then X and Y are *conditionally independent* given Z if

$$\Pr(Y = y | X = x, Z = z) = \Pr(Y = y | Z = z). \quad (30)$$

Conditional independence is a strong form of the idea that the values of X are unable to predict the values of Y , given the values of Z .

In Granger’s time-series setting, the value of Y is determined at some time point s , and the values of X and Z are determined at or prior to some other time point $r < s$. I will say that X is *not a Granger cause of Y* (relative to the information in Z) if X and Y are conditionally independent given Z . Thus X is a Granger cause of Y if different values of X lead to different predictive distributions of Y given both X and the information in Z , that is, if X helps predict Y even when Z is taken into consideration.

Viewed in this way, Granger noncausality is very much like Suppes’s notion of a spurious cause. Both involve the inability of the spurious cause to predict a future event or value given certain other information.

How might Granger’s ideas be applied to the setting in Section 3? It is natural to make the following identification of Granger’s setting with elements of Rubin’s model.

Granger	Rubin’s Model
Y	Y_s
X	S
Z	A set of pre-exposure variables also called Z .

The conditional independence condition is

$$\Pr(Y_s = y | S = t, Z) = \Pr(Y_s = y | Z),$$

and this reduces to

$$0 = \{\Pr(Y_t = y | S = t, Z) - \Pr(Y_c = y | S = c, Z)\} \times \Pr(S = c | Z). \quad (31)$$

In a randomized experiment

$$\Pr(S = c | Z) = \Pr(S = c),$$

which we assume lies strictly in $(0, 1)$. Hence Equation (31) reduces to

$$\Pr(Y_t = y | S = t, Z) = \Pr(Y_c = y | S = c, Z). \quad (32)$$

But under randomization S is independent of Y_t , Y_c , and Z , so Equation (31) becomes

$$\Pr(Y_t = y | Z) = \Pr(Y_c = y | Z), \quad (33)$$

which, in turn, implies that

$$E(Y_t | Z) = E(Y_c | Z) \quad (34)$$

for all values of Z . If we define the average causal effect on the subpopulation specified by $Z = z$ as

$$T(z) = E(Y_t - Y_c | Z = z), \quad (35)$$

then Equation (34) says that if S is not a Granger cause of Y_s relative to Z , then $T(z) = 0$ for all values of z . Hence in a randomized experiment Granger noncausality implies

zero average causal effect on all subpopulations defined by the values of Z . Conversely, it is easy to see that if t has a null effect on all units, then in a randomized experiment S will not be a Granger cause of Y_s relative to *any* Z that is a pre-exposure variable.

Although Granger causality has some intuitively satisfying properties with respect to Rubin's model, it fails, in my opinion, to get to the heart of the notion of causality in the same way that Suppes's theory of causality fails. Granger's "causes" are always only temporarily in that category. If an analyst simply gathers more information, that is, changes Z , an X that was once a Granger cause of Y might be shown to be only a spurious cause in exactly the same spirit as in Suppes's theory.

8.3 Causal Models in Social Science

No discussion of causal inference would be complete without some reference to the expanding literature on causal modeling, that is, Blalock (1971), Goldberger and Duncan (1973), Duncan (1975), and Saris and Stronkhorst (1984). Little work has been done to relate Rubin's model to those used in the causal modeling literature—an exception is Rosenbaum (1984b), in which the average causal effect in a population is related to coefficients that arise in certain linear path models. The relationship between these two types of models is a natural research topic, since both causal models and Rubin's model were developed to deal with the same problem—causal inference in nonexperimental research.

In this section I will hint at some possible points of contact between the path diagrams that are used in causal modeling and the model used in this article. I think that this is a large topic, and I can only scratch its surface here.

Path diagrams are used to represent visually causal relationships among a set of variables. For example, if X causes Y this is expressed by the diagram

$$X \rightarrow Y. \quad (36)$$

From the point of view adopted in this article some diagrams like (36) are meaningful and some are not. For example, if A is an attribute of units and Y is a response variable, then

$$A \rightarrow Y \quad (37)$$

is meaningless. On the other hand, if S indicates exposure to causes and Y_s is an observed response variable, then

$$S \rightarrow Y_s \quad (38)$$

is a meaningful diagram.

What happens when we add a third variable to this system? There are several possibilities. If A is an attribute, then it is either a pre- or post-exposure variable. In the first case we might denote this as

$$A \quad S \rightarrow Y_s \quad (39)$$

to indicate the time flow but without any arrow from A to S or Y_s . In the second case the value of A might be affected by exposure to the cause and we would need to indicate

that by subscripting A , A_t , and A_c . This suggests the diagram

$$S \rightarrow (A_s, Y_s). \quad (40)$$

It indicates that S changes the values of both A and Y . This is the situation analyzed by Rosenbaum (1984b).

The other possibility is that the third variable is an indicator, R , of a second set of causes, say t' and c' . If the R causes act on the units at the same time that the S causes do, then we can combine R and S into a single causal indicator (R, S) . Y must then be doubly subscripted to indicate the responses to the various (R, S) combinations, that is, Y_{RS} . This can be denoted by the diagram

$$(R, S) \rightarrow Y_{RS}. \quad (41)$$

The fact that the R causes and the S causes act at the same time is not really important for Diagram (41). It really says that the R causes do not affect exposure to the S causes, and vice versa. We get an essentially new case, however, when, for example, the R causes act temporally prior to the S causes and they affect the exposure of units to the S causes. This requires that S be subscripted by t' or c' , that is,

$$S_{t'}(u) \quad \text{and} \quad S_{c'}(u). \quad (42)$$

Although it is a mouthful, here is what $S_{t'}(u)$ denotes: $S_{t'}(u)$ is the S cause that u is exposed to if u was earlier exposed to the R cause t' . The following path diagram expresses this situation:



Diagram (43) indicates that R changes the values of S and Y and that S changes the value of Y . R has, potentially, both a direct and an indirect (i.e., through S) effect on Y .

An example may help clarify the meaning of (43). Suppose that we wish to measure the effect of studying certain material on the performance on a particular test. We might be able to *encourage* or *not encourage* students to study the material—these are the R causes, t' and c' . We might then be able to ascertain whether the students *did* or *did not* study the material—these are the S causes, t and c . The response variable is the score Y on the test given subsequent to these events. Diagram (43) indicates that encouragement can affect studying and possibly the test scores and that studying can affect the scores. For example, one might hypothesize that encouragement really does not affect test scores directly. This would be expressed in the model by

$$Y_{t's}(u) - Y_{c's}(u) = 0 \quad (44)$$

for all u in U and $s = t$ or c . For more on "encouragement designs" see Powers and Swinton (1984).

The essential point I wish to make about these diagrams is that they are easily interpreted in terms of Rubin's model when they are not causally meaningless. The causal model literature has not been careful in separating meaningful and meaningless causal statements and path diagrams, in

my opinion. For a similar view see Kempthorne (1978). One expects that the application of Rubin's model will help clarify the meaning of complex causal models and their path diagrams.

9. SUMMARY

This article has covered a variety of topics that involve causation, but there are a few general points that, I think, are important enough to emphasize in summary.

First of all, I believe it is very helpful to try to see what experiments (as the term is used by statisticians) tell us about causation. I have emphasized three ideas about causation on which statistical experiments focus our attention.

1. The analysis of causation should begin with studying the effects of causes rather than the traditional approach of trying to define what the cause of a given effect is.
2. Effects of causes are always relative to other causes (i.e., it takes two causes to define an effect).
3. Not everything can be a cause; in particular, attributes of units are never causes.

Let me make a few brief comments on each of these important ideas.

Traditional analyses of causation start by looking for the cause of an effect. I think that looking for causes of effects is a worthwhile scientific endeavor, but it is not the proper perspective in a theoretical analysis of causation. Moreover, I would hold that the "cause" of a given effect is always subject to revision as our knowledge about the phenomenon increases. For example, do bacteria cause disease? Well, yes . . . until we dig deeper and find that it is the toxins the bacteria produce that really cause the disease; and this is really not it either. Certain chemical reactions are the real causes . . . and so on, ad infinitum. The effect of a cause may be difficult to measure in some circumstances, but it is, at least, precisely definable—as done in Section 3. It is for this reason that I believe that formal theories of causation must begin with the effects of given causes rather than vice versa.

That an effect requires two causes for its definition is obvious in the context of an experiment but never seems to get much recognition by those who discuss causation in general terms. This is probably an important contribution of statistical thinking to discussions of causation. Experiments without control comparisons are simply not experiments. Those who think in terms of physical science experiments may have some difficulty with this idea, but I believe that it is true of any experiment.

That everything has a cause is sometimes called the law of causality, but it does not imply that everything can be a cause. The experimental model eliminates many things from being causes, and this is probably very good, since it gives more specificity to the meaning of the word *cause*. Donald Rubin and I once made up the motto

NO CAUSATION WITHOUT MANIPULATION

to emphasize the importance of this restriction. Although many people balk at the idea that causes might be limited in some way, this idea is a simple consequence of the struc-

ture of the model in Section 3. Unless both $Y_t(u)$ and $Y_c(u)$ can be defined, in principle, it is impossible to define the causal effect $Y_t(u) - Y_c(u)$. For an attribute $A(u)$ we can define $Y_a(u)$ for all u for which $A(u) = a$, and we can define $Y_b(u)$ for all u for which $A(u) = b$. Attributes are functions, however, and $A(u)$ is either a or b (or neither) but not both a and b for any unit, u . Hence $Y_a(u) - Y_b(u)$ cannot be defined for any unit, u , and attributes are not causes in the sense that causal effects cannot be defined for them.

The second set of important general points I wish to summarize concern the immediate consequences of Rubin's model. There are two consequences I wish to emphasize.

1. The difference between the *model* (S, Y_t, Y_c) and the process of observation (S, Y_s).
2. The Fundamental Problem of Causal Inference—only Y_t or Y_c but not both can be observed on any unit u .

These two consequences are really the same thing said in different ways. It is a great mistake to confuse Y_t or Y_c with Y_s , and yet this is done all the time. It is also a mistake to conclude from the Fundamental Problem of Causal Inference that causal inference is impossible. What is impossible is causal inference without making untested assumptions. This does not render causal inference impossible, but it does give it an air of uncertainty. It is the same uncertainty discussed by Hume. The strength of a model like Rubin's is that it allows us to make these assumptions more explicit than they usually are. When they are explicitly stated the analyst can then begin to look for ways to evaluate or to partially test them.

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I first learned about the causal model in Section 3 from the person I consider its originator, Donald Rubin. Don's work in this area is always a source of inspiration for me. Lindsey Churchill read an early draft of this article and made numerous suggestions that have improved and focused both my thinking and the article in substantial ways. Paul Rosenbaum has, very generously, given me the benefit of his insight into causal inference on many occasions. Ben King encouraged me to put the ideas in this article together as a General Methodology Lecture for the 1985 meetings of the ASA. My other colleagues at ETS—Henry Braun, Donald Rock, Dorothy Thayer, and Howard Wainer—are always a source of intelligence and keen criticism. Lynne Steinberg, as an ETS postdoctoral fellow during 1984–1985, spent many hours explaining to me how causation works in experimental psychology. Finally, Kathy Fairall's good nature and many skills insured the timely production of the manuscript for the 1985 meeting of the ASA.

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REFERENCES

- Barnard, G. A. (1982), "Causation," in *Encyclopedia of Statistical Sciences* (Vol. 1), eds. S. Kotz, N. Johnson, and C. Read, New York: John Wiley, pp. 387–389.

- Blalock, H. M., Jr. (ed.) (1971), *Causal Models in the Social Sciences*, Chicago: Aldine-Atherton.
- Bunge, M. (1959), *Causality and Modern Science* (3rd ed.), New York: Dover Publications.
- Cochran, W. G. (1983), *Planning and Analysis of Observational Studies*, New York: John Wiley.
- Cook, R. D. (1980), "Smoking and Lung Cancer," in *R. A. Fisher: An Appreciation*, eds. S. Fienberg and D. Hinkley, New York: Springer-Verlag.
- Cornfield, J., Haenszel, W., Hammond, E. C., Lilienfeld, A. M., Shimkin, M. B., and Wynder, E. L. (1959), "Smoking and Lung Cancer: Recent Evidence and a Discussion of Some Questions," *Journal of the National Cancer Institute*, 22, 173-203.
- Cox, D. R. (1958), *The Planning of Experiments*, New York: John Wiley.
- Doll, R., and Hill, B. (1950), "Smoking and Carcinoma of the Lung," *British Medical Journal*, 2, September 30, 739-748.
- (1952), "A Study of the Aetiology of Carcinoma of the Lung," *British Medical Journal*, 2, December 13, 1272-1286.
- (1956), "Lung Cancer and Other Causes of Death in Relation to Smoking," *British Medical Journal*, 2, November 10, 1071-1081.
- Duncan, O. D. (1975), *Introduction to Structural Equation Models*, New York: Academic Press.
- Evans, A. S. (1978), "Causation and Disease: A Chronological Journey," *American Journal of Epidemiology*, 108, 249-258.
- Fisher, R. A. (1926), "The Arrangement of Field Experiments," *Journal of Ministry of Agriculture*, 33, 503-513.
- (1957), "Letter to the Editor," *British Medical Journal*, 2, July 6, 43.
- Florens, J. P., and Mouchart, M. (1985), "A Linear Theory for Noncausality," *Econometrica*, 53, 157-175.
- Goldberger, A. S., and Duncan, O. D. (1973), *Structural Equation Models in the Social Sciences*, New York: Seminar Press.
- Granger, C. W. J. (1969), "Investigating Causal Relations by Econometric Models and Cross-Spectral Methods," *Econometrica*, 37, 424-438.
- (1980), "Testing for Causality: A Personal Viewpoint," *Journal of Economic Dynamics and Control*, 2, 329-352.
- Hamilton, M. A. (1979), "Choosing a Parameter for 2×2 Table or $2 \times 2 \times 2$ Table Analysis," *American Journal of Epidemiology*, 109, 362-375.
- Hill, A. B. (1965), "The Environment and Disease: Association or Causation," *Proceedings of the Royal Society of Medicine*, 58, 295-300.
- Holland, P. W., and Rubin, D. B. (1980), "Causal Inference in Prospective and Retrospective Studies," address given at the Jerome Cornfield Memorial Session of the American Statistical Association Annual Meeting, August.
- (1983), "On Lord's Paradox," in *Principals of Modern Psychological Measurement*, eds. H. Wainer and S. Messick, Hillsdale, NJ: Lawrence Erlbaum.
- Hume, D. (1740), *A Treatise on Human Nature*.
- (1748), *An Inquiry Concerning Human Understanding*.
- Kempthorne, O. (1952), *The Design and Analysis of Experiments*, New York: John Wiley.
- (1978), "Logical, Epistemological and Statistical Aspects of Nature-Nurture Data Interpretation," *Biometrics*, 34, 1-24.
- Locke, J. (1690), *An Essay Concerning Human Understanding*, Book II, Chapter XXVI.
- McCurdy, R. (1957), "Letter to the Editor," *British Medical Journal*, 2, July 20.
- Mill, J. S. (1843), *A System of Logic*.
- Neyman, J. (with Iwaszkiewicz, K., and Kolodziejczyk, S.) (1935), "Statistical Problems in Agricultural Experimentation" (with discussion), *Supplement of Journal of the Royal Statistical Society*, 2, 107-180.
- Powers, D. E., and Swinton, S. S. (1984), "Effects of Self-Study for Coachable Test Item Types," *Journal of Educational Measurement*, 76, 266-278.
- Rosenbaum, P. R. (1984a), "From Association to Causation in Observational Studies: The Role of Tests of Strongly Ignorable Treatment Assignment," *Journal of the American Statistical Association*, 79, 41-48.
- (1984b), "The Consequences of Adjustment for a Concomitant Variable That Has Been Affected by the Treatment," *Journal of the Royal Statistical Society, Ser. A*, 147, 656-666.
- (1984c), "Conditional Permutation Tests and the Propensity Score in Observational Studies," *Journal of the American Statistical Association*, 79, 565-574.
- Rosenbaum, P. R., and Rubin, D. B. (1983a), "The Central Role of the Propensity Score in Observational Studies for Causal Effects," *Biometrika*, 70, 41-55.
- (1983b), "Assessing Sensitivity to an Unobserved Binary Covariate in an Observational Study With Binary Outcome," *Journal of the Royal Statistical Society, Ser. B*, 45, 212-218.
- (1984a), Discussion of "On the Nature and Discovery of Structure," by J. W. Pratt and R. Schlaifer, *Journal of the American Statistical Association*, 79, 26-28.
- (1984b), "Reducing Bias in Observational Studies Using Subclassification on the Propensity Score," *Journal of the American Statistical Association*, 79, 516-524.
- (1985a), "Constructing a Control Group Using Multivariate Matched Sampling Methods That Incorporate the Propensity Score," *The American Statistician*, 39, 33-38.
- (1985b), "The Bias Due to Incomplete Matching," *Biometrics*, 41, 103-116.
- Rubin, D. B. (1974), "Estimating Causal Effects of Treatments in Randomized and Nonrandomized Studies," *Journal of Educational Psychology*, 66, 688-701.
- (1977), "Assignment of Treatment Group on the Basis of a Covariate," *Journal of Educational Statistics*, 2, 1-26.
- (1978), "Bayesian Inference for Causal Effects: The Role of Randomization," *The Annals of Statistics*, 6, 34-58.
- (1980), Discussion of "Randomization Analysis of Experimental Data: The Fisher Randomization Test," by D. Basu, *Journal of the American Statistical Association*, 75, 591-593.
- Saris, W., and Stronkhorst, H. (1984), *Causal Modelling in Non-experimental Research*, Amsterdam: Sociometric Research Foundation.
- Smith, R. Jeffrey (1980), "Government Says Cancer Rate Is Increasing," *Science*, 227, 998-1002.
- Suppes, P. C. (1970), *A Probabilistic Theory of Causality*, Amsterdam: North-Holland.
- Yerushalmay, J., and Palmer, C. E. (1959), "On the Methodology of Investigations of Etiologic Factors in Chronic Diseases," *Journal of Chronic Diseases*, 10, 27-40.



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DONALD B. RUBIN*

If

If all the world were apple pie,
 And all the sea were ink,
 And all the trees were bread and cheese,
 What should we have for drink?

—*The Real Mother Goose*

I congratulate my friend Paul Holland on his lucidly clear description of the basic perspective for causal inference referred to as Rubin's model. I have been advocating this general perspective for defining problems of causal inference since Rubin (1974), and with very little modification since Rubin (1978). The one point concerning the definition of causal effects that has continued to evolve in my thinking is the key role of the *stable-unit-treatment-value assumption* (SUTVA, as labeled in Rubin 1980) for deciding which questions are formulated well enough to have causal answers.

Under SUTVA, the model's representation of outcomes is adequate. More explicitly, consider the situation with N units indexed by $u = 1, \dots, N$; T treatments indexed by $t = 1, \dots, T$; and outcome variable Y , whose possible values are represented by Y_{tu} ($t = 1, \dots, T$; $u = 1, \dots, N$). SUTVA is simply the a priori assumption that the value of Y for unit u when exposed to treatment t will be the same no matter what mechanism is used to assign treatment t to unit u and no matter what treatments the other units receive, and this holds for all $u = 1, \dots, N$ and all $t = 1, \dots, T$. SUTVA is violated when, for example, there exist unrepresented versions of treatments (Y_{tu} depends on which version of treatment t was received) or interference between units (Y_{tu} depends on whether unit u' received treatment t or t').

FISHER'S NULL HYPOTHESIS AS A SPECIAL CASE OF SUTVA

SUTVA is automatically satisfied under the Fisher (1935) null hypothesis of absolutely no treatment effects of any kind, H_F , since under H_F the treatment labels are absolutely irrelevant: the values of outcome Y for unit u are exactly the same for all treatments,

$$H_F: Y_{tu} = Y_{t'u} \quad \text{for all } u \text{ and all pairs } t, t'. \quad (1)$$

Thus when Fisher's null hypothesis is tested, which is typically but not necessarily done only in randomized experiments using randomization tests, a particular case of SUTVA is always assumed. If H_F is rejected, all that can be said is that this representation using a very special case of SUTVA is inadequate.

For example, many common language uses of "cause"

are essentially statements of a Fisher null hypothesis. Consider

The sun causes the planets to travel in their orbits, (2)
 in which the implied treatments are "sun" and "no sun," the unit is the group of planets, and Y is an indicator for their current orbits; or

If John Doe had been born a female,
 his life would have been different, (3)

in which the implied treatments are "born as male" and "born as female," John Doe is the only unit, and Y is an indicator for his life as a male. In both Statements (2) and (3), all that is being claimed causally is that Fisher's null hypothesis is to be rejected: no matter how the units would be actually exposed to the relatively vague other treatment ("no sun" and "born as female"), the outcome would not be identical to the outcome under the existing treatment. Neither statement carries with it a precise description of the other treatment (the precise manipulations that would constitute exposure to the other treatment) nor a precise description of an alternative hypothesis under which SUTVA is satisfied but H_F is not.

Thus in the context of statement (3), the claim is simply that if John Doe were born female instead of male, whether because of some hypothetical Y to X chromosome treatment at conception, or massive doses of hormones in utero that would lead to female morphology at birth, or an at-birth sex-change operation, or so forth, John Doe's life would have been different. I accept this as a meaningful causal statement. Since maleness is an attribute of John Doe, however, Holland might not consider Statement (3) to be a meaningful causal claim, and similarly with Statement (2).

In any case, more careful consideration of the implications of SUTVA is required whenever sizes of causal effects are of interest or null hypotheses regarding typical causal effects are to be evaluated, because then actual values under more than one treatment must be contemplated. My formulation of Neyman's null hypothesis of no average causal effect differs somewhat from Holland's because I believe that versions of treatments are implicit in Neyman's discussion yet are absent from Holland's description of it.

NEYMAN'S NULL HYPOTHESIS FORMULATED TO SATISFY SUTVA

Consider the case of two fertilizers A and B , N units, which are plots of land at the time of an experiment, and

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the outcome Y , which is crop yield on the plots. Each fertilizer has m (m very large) versions $\{A_1, A_2, \dots, A_m\}$ and $\{B_1, B_2, \dots, B_m\}$ corresponding to different bags, where one bag is needed to fertilize a plot. The bags are known to vary somewhat in effectiveness, and thus SUTVA only holds exactly when all $2m$ versions of the fertilizers are represented as treatments by $2m$ outcomes (i.e., $t = A_1, \dots, A_m, B_1, \dots, B_m$). Using only two treatments, A and B , violates SUTVA because the value of Y for unit u under treatment A (or B) depends on which bag was used.

The causal question of primary interest concerns the typical yields of plots when exposed to fertilizer A relative to their yields when exposed to fertilizer B . A natural way to specify this question is to define the average A versus B differential yield for plot u as

$$\bar{Y}_{Au} - \bar{Y}_{Bu} = \frac{1}{m} \sum_{t=A_1}^{A_m} Y_{tu} - \frac{1}{m} \sum_{t=B_1}^{B_m} Y_{tu}, \quad (4)$$

and then define the causal estimand as the average A versus B differential yield,

$$\bar{Y}_A - \bar{Y}_B = \frac{1}{N} \sum_{u=1}^N (\bar{Y}_{Au} - \bar{Y}_{Bu}). \quad (5)$$

I believe that this formulation is implicit although certainly not explicit in Neyman (1935). It differs from Holland's interpretation of Neyman in that Holland uses the two-treatment formulation, which violates SUTVA because of "technical errors . . . due solely to the inaccuracy of experimental technique" (Neyman 1935, p. 110). Non-additivity of treatment effects [$Y_{tu} - Y_{t'u}$ being a function of u as well as (t, t')] arose in Neyman because of "soil errors" due to "variation in fertility of the plots."

Accepting the causal estimand defined in (4) and (5), Neyman's null hypothesis, H_N , is that the average differential effect of fertilizer A versus fertilizer B is 0,

$$H_N: \bar{Y}_A - \bar{Y}_B = 0.$$

In contrast, the Fisher null hypothesis is given by (1), where t and $t' = A_1, \dots, A_m, B_1, \dots, B_m$.

In an ideally designed randomized experiment in which bags of each type of fertilizer are randomly chosen and randomly applied to plots, it is relatively straightforward to address H_N as well as H_F , although not necessarily using identical statistical tools. But in other cases, H_N is more difficult to address than H_F —simply suppose that fertilizers A and B were randomly assigned to plots, but the bags of A and the bags of B to be used on the plots were carefully selected by the manufacturer of A .

APPLYING SUTVA TO SEX DISCRIMINATION

Careful consideration of SUTVA is especially important for clarifying questions that cannot be addressed by randomized experiments and for deciding precisely in what sense such questions can have causal answers. As a specific example, consider the following statement:

If the females at firm f had been male, their starting salaries would have averaged 20% higher. (6)

I believe Holland would claim that Statement (6) is causally meaningless because "femaleness" is an attribute. I too believe that Statement (6) is causally meaningless, but for a possibly different reason: the statement, by itself, is too vague to have a clear formulation satisfying SUTVA and thus is too vague to admit a clear causal answer. What are the units, treatments, and outcomes such that SUTVA is satisfied? I am not at all sure how to define anything except Y , which clearly involves starting salary.

One range of possibilities for making (6) more precise is generated by considering the units to be the female employees at entry and the treatments to be "female," which is well defined since the units are females, and "male," which has many possible versions ranging from some hypothetical "at conception X to Y chromosome treatment" to replacing an "F" with an "M" on a job application form. Certainly these different versions of the treatment "male" could lead to vastly different outcomes, and so SUTVA is totally implausible without agreement on which version of the treatment "maleness" is under study or agreement on a way to average over some collection of such versions.

Another possibility, and one more closely tied to potential real-world manipulations, is to consider the firm to be the unit, multivariate Y to be the starting salaries of the female employees, and the treatments to be "current hiring practices" and "hiring practices as would take place under court supervision." Or perhaps the job slots in the firm are the units, Y is the starting salary in each job slot, and applicants are the treatments: type A treatments are the female applicants and type B treatments are the male applicants, using the notation used for Neyman's null hypothesis. For related discussion of this perspective, see Pratt and Schlaifer (1984), especially the rejoinder to the discussion by Rosenbaum and Rubin (1984).

In any case, the crucial point with Statement (6) is that we are not ready to estimate, test, or even logically discuss causal effects until units, treatments, and outcomes have been defined in such a way that SUTVA is plausible.

NO CAUSATION WITHOUT MANIPULATION?

Since statisticians who study causal effects usually do so for the purpose of drawing inferences about the effects of actual manipulations to which some group of units have been or might be exposed, the motto "no causation without manipulation" is a critical guideline for clear thinking in empirical studies for causal effects. Thinking about actual manipulations forces an initial definition of units and treatments and thereby increases the likelihood of a formulation in which SUTVA is plausible. Such clarity is essential, yet commonly absent, in policy-oriented studies in which decisions to implement real-world manipulations can result from the statistician's causal inferences.

ADDITIONAL REFERENCES

- Fisher, R. A. (1935), *The Design of Experiments*, Edinburgh: Oliver & Boyd.
- Pratt, J. W., and Schlaifer, R. (1984), "On the Nature and Discovery of Structure" (with discussion), *Journal of the American Statistical Association*, 79, 9–33.



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D. R. COX*

It is a pleasure to have the chance of congratulating Holland on an exceptionally lucid article on an important topic. Indeed the issues explicitly and implicitly raised by the article seem to me more important for the foundations of our subject than discussion of the nature of probability, perennially intriguing though that may be. Philosophy is often regarded by scientists, on this side of the Atlantic at least, as an irredeemably "soft" subject, but here is a matter both of philosophical interest and also with important practical implications, for example, for the interpretation of coefficients in multiple regression equations.

The question of what can constitute a cause in this context is a key issue, and there is need for some good terminology. Cox and Snell (1981, p. 12) called variables that, in the context under consideration, should not be regarded as treatments, intrinsic variables. A subdivision into those associated with the individual person, animal, or whatever and those associated with the environment is sometimes useful. It might also be useful to distinguish between treatments and quasitreatments. In addition, the term nonspecific (Cox 1984) may be used for strata, blocks, and so forth that are normally intrinsic, but with no clearly specified unique characterization.

The notion that certain variables cannot properly be regarded as causes is most concisely encapsulated in the physicists' notion that passage of time cannot be regarded as a cause: of course, a process going on in time, such as molecular rearrangement, could be a cause, because it is possible to conceive of time passing without the rearrangement in question taking place.

In some observational studies the distinction between quasitreatment and intrinsic variables is a matter of viewpoint and may not be clear-cut. Think, for example, of an observational study of alcohol consumption related to some outcome variable.

One point deserving emphasis is the need for careful specification of what constitutes a particular treatment, including what may be subsidiary consequences. This may be crucial if technically correct but nevertheless misleading conclusions are to be avoided. In studying the effect of alcohol, is diet held fixed?

A celebrated, if possibly apocryphal, example concerns an agricultural field trial in which one treatment led to such a superior quality and quantity of product that birds for many kilometers around gathered to consume the product, leading to poor final yield. Does that treatment cause poor yield? In one sense, yes. Similar points arise in clinical trials in connection with the distinction between intention to treat and per protocol analyses. The point partly is that the difficulties of observational studies cannot be totally

avoided in randomized experiments, if one is to look in depth at interpretation. The searching discussion of Pratt and Schlaifer (1984) is very relevant.

This is related to the issue of "layers" of interpretation. Is not the reason that one expects turning a light switch to have the result it does not just direct empirical observation but a subtle and deep web of observations and ideas—the practice of electrical engineering, the theory of electrical engineering, various ideas in classical physics, summarized, in particular, in Maxwell's equations, and underneath that even ideas of unified field theory? One reason that the notion of "cause" is so important is that it carries suggestions of relations at a deeper level of interpretation than the direct observation under study.

My final comment concerns absence of interaction or presence of unit-treatment additivity. Holland suggests in Section 6 of his article that this might have been regarded as a "technical" requirement, whatever that might mean. In fact it seems to me to be of great importance from various points of view. First the condition is not wholly operationally verifiable, as Holland carefully discusses. A rigid adherent of operationalism might, therefore, regard the condition as meaningless; in fact, so far as I can see, rigid operationalism went out of favor a long time ago, both in philosophy with the decline of logical positivism and in physics with increased emphasis on quantum mechanics. Yet it represents a fine ideal, that all assumptions and concepts should be capable of direct verification, but in the present context, and in many others, partial operationalism seems to be the most one can reasonably get. This is that certain aspects of the assumption can be tested.

Thus in the present context one could detect use of an inappropriate scale, or, as soon as intrinsic variables are available, examination for treatment \times intrinsic interaction becomes feasible. Such considerations are important both for understanding and for examining possible extrapolation of the conclusions to new units. When no such further information is available the technical questions raised by Neyman for the Latin square remain (Wilks and Kempthorne 1957); that is, is the usual analysis unbiased? I think it is arguable that the analysis is unbiased in a reasonable sense (Cox 1958), but admittedly a somewhat contorted view of the question under study is needed.

In conclusion, I welcome the article as an account of underdiscussed issues of considerable importance.

ADDITIONAL REFERENCES

- Cox, D. R. (1958), "The Interpretation of the Effects of Non-additivity in the Latin Square," *Biometrika*, 45, 69–73.

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- (1984), "Interaction" (with discussion), *International Statistical Review*, 52, 1-31.
 Cox, D. R., and Snell, E. J. (1981), *Applied Statistics*, London: Chapman & Hall.
 Pratt, J. W., and Schlaifer, R. (1984), "On the Nature and Discovery of

- Structure" (with discussion), *Journal of the American Statistical Association*, 79, 9-33.
 Wilk, M. B., and Kempthorne, O. (1957), "Non-additivities in the Latin Square Design," *Journal of the American Statistical Association*, 52, 218-236.

Comment

Statistics and Metaphysics

CLARK GLYMOUR*

1. INTRODUCTION

Holland's paper is as much philosophical analysis as it is statistics. The general lines of the account of causal relations he gives are familiar to philosophers, although he does not discuss any of the philosophical literature in which they may be found. I will try to place Holland's account in the framework of contemporary philosophical discussions of causality. I agree with the general thrust of his analysis, but I think certain restrictions he imposes are unwarranted, and I will say which they are, and why I think them unjustified.

Holland's account of causality is counterfactual. A fair paraphrase of his analysis is this:

Treatment t causes individual u to have the value Y_t for variable Y rather than the value Y_c for that variable if and only if u received treatment t , u has the value Y_t , and if u had received the treatment c rather than the treatment t , then u would have the value Y_c for variable Y .

Holland imposes conditions on this analysis, conditions that can be thought of as further explications of what he means it to say:

1. It must have been possible for u to have received treatment c rather than treatment t .

2. A treatment t can only be a cause of individual u having the value Y_t rather than Y_c provided t is a treatment that is applied to that same individual, u , and c is a treatment that could have been applied to that same individual.

3. Causation is a relation between two treatments and two possible variable states. The notion of t causing Y_t , without specification of any alternative treatment, or any alternative state of Y , is not defined.

I will consider these conditions later. First, I want to address the philosophical context.

2. COUNTERFACTUALS AND CAUSALITY

Notice that the clause following the phrase "if and only if" in my paraphrase of Holland's account is a counterfac-

tual conditional. It is a sentence of the form (neglecting tense):

If X were the case then Y would be the case.

Such sentences exhibit logical features that have interested philosophical logicians for some years. Their logical features include the following:

1. Counterfactuals can be logically false:

If X were the case then X and not X would be the case.

2. Counterfactuals can logically entail one another:

If X were the case then Y would be the case

entails

If X were the case then Y or Z would be the case.

3. Counterfactuals have different logical entailment relations than do ordinary material conditionals.

If X then Y

entails

If X and Z then Y ,

but

If X were the case then Y would be the case

does not entail

If X were the case and Z were the case then Y would be the case.

("If I had struck the match just now it would have lighted" is true, but "If I had struck the match just now and there had been no oxygen in the room, it would have lighted" is false.)

There are two principal ways to give a theory of the logical structure of some piece of reasoning. Both share the presupposition that the reasoning can be represented in a formalized language. One way is to characterize the logic axiomatically, by specifying an initial set of logical

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- (1984), "Interaction" (with discussion), *International Statistical Review*, 52, 1-31.
 Cox, D. R., and Snell, E. J. (1981), *Applied Statistics*, London: Chapman & Hall.
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truths and a set of rules of derivation, such that all and only the logical truths are derivable from the axioms, and such that if an inference is valid, then the conclusion of the inference is derivable from the premises of the inference. Another way to characterize logical structure is through formal semantics. A semantic characterization specifies a class of possible interpretations of the language and what it is for a sentence in the language to be true under an interpretation. The logical truths are then those that are true under every possible interpretation; valid arguments are those for which no interpretations exist making their premises true and their conclusion false. The aim of philosophical logicians studying some logical feature of language is to represent that feature in a formalized language, to characterize it both axiomatically and semantically, and to prove that the two characterizations determine exactly the same class of logical truths and the same collection of valid arguments.

There are two well-known logical theories of counterfactual conditionals, one due to Robert Stalnaker at Cornell University (Stalnaker 1984), the other to David Lewis (Lewis 1973, 1983), who is Holland's neighbor at Princeton University. There is also a logical theory of tensed counterfactual conditionals due to Richmond Thomason (Thomason and Gupta 1980). The Stalnaker and Lewis theories differ slightly, but the semantic characterization Stalnaker gives is especially simple, and I will, therefore, use it.

Imagine that there are a collection of possible worlds, much as in science fiction stories, and that in each possible world every sentence that is not counterfactual and is in our formalized language is either true or false. Further imagine that there is a relation between possible worlds, a relation of closeness. Finally, assume that for every possible world w and every logically possible condition A , there is a unique world that is the closest world to w in which A is true. If A happens to be true in w , then w itself is that closest possible world. Then for any sentences X and Y in the language,

"If X were the case then Y would be the case" is true in world w if and only if in the closest world to w in which X is true, Y is also true.

You may well wonder what possible worlds are and which possible worlds are supposed to be closer to which others. The point is that, for the purpose of giving a logical theory, it does not matter what possible worlds are or which of them are closest to which others. The worlds and their relations can be taken seriously or as a convenient mathematical fiction; in either case, they characterize the set of logical truths and they characterize valid inferences. At the very least, talk of possible worlds and their proximities provides a vivid metaphor that is easy to think about mathematically. At most, it provides the metaphysical underpinnings of our understanding of possibility and necessity.

Lewis proposed that causal relations are counterfactual relations. He proposed that if X and Y are sentences describing the occurrence of particular events, then

X causes Y if and only if X occurs and Y occurs and if X had not occurred then Y would not have occurred.

In the semantics of counterfactuals, this becomes

X causes Y if and only if X is true in the actual world and Y is true in the actual world and in the closest (to the actual world) possible world in which X is not true, Y is not true.

Return now to Holland's characterization of causal relations. We can see that his account is straightforwardly interpreted within the semantics of counterfactuals and that his account is really a specialization of Lewis's. Counterfactual analyses of causation, such as those of Lewis and of Holland, are naturally compared with alternative accounts that characterize causal relations in terms of probability relations. Such accounts have been provided, in various ways, by Suppes (1970), Granger (1969), Reichenbach (1949), Salmon (1980), and Skyrms (1980). Probabilistic accounts of causality have the advantage that they seem to make it easy to understand how we can have knowledge of causal relations, and equally, to ease our understanding of the bearing of statistics on causal inference. Technical details aside, causal inference becomes a statistical estimation problem. They have the disadvantage that they do not always accord very well with our intuitive judgments about causal relations.

Counterfactual accounts of causality have the disadvantage that they appeal to unobservables—to what would be true if . . . , and to what goes on in possible worlds we will never see. They, therefore, present us with a mystery as to how we can know anything about causal relations. The mystery surely has a solution, and the general lines of the solution must be something like this: We are able to infer causal relations because we are able to infer counterfactual truths, and we are able to infer counterfactual truths because we make assumptions that we test against one another in rather indirect ways. Holland's article seems to me especially valuable in clarifying some of these assumptions and in explicating their relations. The philosophical community, unfortunately, has not been very energetic in addressing the mystery.

3. HOLLAND'S RESTRICTIONS

I am not convinced that the restrictions Holland imposes on causal relations are equally justified. Consider, first, the requirement that for treatment t to cause individual u to have Y , rather than Y_c it must have been possible for u to have received treatment c instead of t . Holland intends this requirement to exclude factors such as genetic constitution and attributes determined by genetic constitution (e.g., race and gender) from the category of causes. There is no treatment that would give one and the same individual a genetic structure other than the actual one. There seem to be two ideas here. One is that genetic structure is not an event, not a happening but an enduring attribute, and causes

must be events. The other is that the identity of organisms depends on their genetic structure, so any actual or possible individual who differs from me in genetic structure is not me. Thus counterfactuals whose antecedents suppose that I had a different genetic structure are nonsensical.

If we insist that only events, not attributes, can be causes, then we can still make sense of the talk of causal attributes as a *facon de parler*. We need only find for each individual and each attribute the event that was the acquisition of that attribute by that individual. In the case of genetic structure there is such an event, conception. In many of the sociological cases in which attributes are used as causes and that Holland rejects as meaningless, there are also appropriate events that are the acquisition of the attributes, and the talk of attributes as causes can, therefore, be interpreted as a harmless convenience of speech. I cannot agree that "The causal model literature has not been careful in separating meaningful and meaningless causal statements and path diagrams" (p. 958). There is little need for this sort of care.

We can identify persons across at least some alterations in genetic structure. Down's syndrome is caused by a trisomy—a bit of extra genetic material attached to a chromosome pair. If that extra bit of material were removed from the zygote, without damaging viability, the zygote would develop into a person—the very same person I should say—without Down's syndrome. Even when one cannot identify persons across changes in genetic structure, there may still be correspondences that make counterfactuals intelligible. My parents tell me that if I had been a girl, I would have been named Olga. I believe what they tell me, and I think they mean more by it than that their intent was to name their first-born "Olga" if their first-born was female. (I believe this because I believe they did things like the following: referring to the creature in my mother's uterus, they said, "If it's a girl, we will call her Olga.") The reference was not just to whatever person should be their first-born, but, as it turns out, to me, and the antecedent of the conditional is contrary to fact.) I can imagine a possible world in which I do not exist, but a female counterpart of me does. In that world she is conceived on the day I was conceived in this world, her parents in that world are mine in this, and her name is Olga. If counterparts are conceivable—and why not?—then counterfactuals that violate identity conditions are intelligible, and if counterfactuals are intelligible, then causal relations are as well.

Holland's second restriction is that the treatment that is to be called a cause must be applied to the very individual that has the variable value that is called the effect. I see no clear motivation for this restriction, and it certainly does not agree with our causal judgments and knowledge. The Big Bang caused the cosmological background radiation. A parent's acquisition of syphilis can cause a child's (congenital) syphilis, and so forth. Nothing in the counterfac-

tual analysis of causation requires such a restriction, and I am rather at a loss to find a motive for its introduction.

I am tempted to think that Holland's third restriction, which demands that a cause always be relative to a specific alternative, is an improvement on the bare counterfactual account of causal relations. The reason is this: My Uncle Schlomo smoked two packs of cigarettes a day, and I am firmly convinced that smoking two packs of cigarettes a day caused him to get lung cancer. But it may not be true that in the closest possible world in which Uncle Schlomo did not smoke two packs a day, he did not contract cancer. Reflecting on Schlomo's addictive personality, and his general weakness of will, it may well be that the closest possible world in which Schlomo did not smoke two packs of cigarettes a day is a world in which he smoked three packs a day. I can reconcile this reflection with the counterfactual analysis of causality by supposing, with Holland, that "smoking two packs of cigarettes a day caused him to get lung cancer" is elliptical speech, and what is meant, but not said, is that smoking two packs of cigarettes a day, rather than not smoking at all, caused Schlomo to contract lung cancer.

4. CONCLUSION

Probability may have begun with games of chance, but one of the principal goals of statistics has always been the determination of causal relations from both experimental and nonexperimental data. I applaud Holland's willingness to try to make the links a little clearer, and I even agree in the main with what I take to be his understanding of causal relations. I applaud as well his efforts to connect philosophy and statistics. Statistics runs with a lot of philosophy, too much of it tacit, and bad philosophy is best avoided by explicitness. I would only caution against branding discourse that does not agree with a philosophical account as "meaningless." People talk as they will, and if they talk in a way that does not fit some piece of philosophical analysis and seem to understand one another well enough when they do, then there is something going on that the analysis has not caught. That is not a failing of the speakers. It is, if anything, a failing of we who philosophize, even if we philosophize with statistics.

ADDITIONAL REFERENCES

- Lewis, D. (1973), *Counterfactuals*, Cambridge, MA: Harvard University Press.
- (1983), *Philosophical Papers* (Vol. 2), Oxford, U.K.: Oxford University Press.
- Reichenbach, H. (1949), *The Theory of Probability*, Berkeley and Los Angeles: University of California Press.
- Salmon, W. (1980), "Probabilistic Causality," *Pacific Philosophical Quarterly*, 61, 50–74.
- Skyrms, B. (1980), *Causal Necessity*, New Haven, CT: Yale University Press.
- Stalnaker, R. (1984), *Inquiry*, Boston, MA: Bradford Books.
- Thomason, R., and Gupta, A. (1980), "A Theory of Conditionals in the Context of Branching Time," *Philosophical Review*, 88, 65–90.



Statistics and Causal Inference: Comment

Clive Granger

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CLIVE GRANGER*

If causality can be equated with some measurable quantity, then statisticians should be able to devise tests for causation. I believe this to be an important topic but it is one that statisticians have been rather unhelpful about, even negligent, in the past. Only a few statisticians have attempted to discuss this difficult field, and thus I welcome Holland's article as a useful further contribution.

To appreciate his work it is important to consider first a variety of causal-type questions. If one looks at units of a population and asks why one unit has a different value of some variable than another, this is a cross-sectional question. An example is when one tries to explain why one household spends more on electricity, say, than does another household. Many causality questions, however, are of quite a different nature, such as asking why electricity demand has fallen this year or why crime rates have increased. Here, one is asking causal questions about data that are a group of time series. In cross-sectional causation one is asking why a particular unit is placed in a certain part of the distribution for the variable of interest. In temporal causality one is asking why parameters of that distribution have changed through time. The two types are very different in nature and probably require different definitions and methods of analysis.

Holland's article deals just with cross-sectional causal questions, as is clear from the discussion of association in Section 2 and the causal model in Section 3. The use of experiments to illuminate statistical questions have a venerable past and an experimental viewpoint seems to be an entirely sensible one for consideration of cross-sectional causation. I found Holland's discussion in Section 3 very helpful and largely convincing for the particular class of questions being asked. Of course, the experiment actually has to take place for the analysis; hypothetical experiments will not be relevant. According to this article, it is also required that the treatment variable actually can be controlled for all units of the population. It follows that one cannot tackle questions such as whether race or sex affects income or crime rates. Thus many causal questions cannot be tackled within this framework, such as most of those arising in history, economics, sociology, meteorology, oceanology, political science, anthropology, or law. This is, of course, a serious limitation. Examples of topical importance are the questions of whether pornography causes changes in rape rates and whether the death sentence causes decreases in murder rates.

There are some very important advantages of trying to analyze causality by experimentation. One can hold constant, or at least potentially control, many other variables that otherwise could be disturbing so that it is not necessary to condition on these variables during the analysis. Further, one does know which is the treatment variable and which

is the experiment's outcome variable. This is not always true in nonexperimental situations—for example, does crime cause poverty or does poverty cause crime? It might also be noted that the value of the treatment variable—the cause—is determined before the experiment starts, and thus before the output variable is observed, and this will be known in an experiment. There are also difficulties with experiments, however. Human subjects may behave differently in experimental situations than in the real world, making findings not easily transferable and so of limited value. Further, some "irrelevant" variables may be controlled and disturb the actual causal relationship. For example, when studying the effects of a price raise on consumption, if the hours worked by consumers and hence their incomes are kept constant, the wrong causal implication may be reached. One also cannot ask questions about two-way causation, such as poverty causing crime and crime causing poverty.

I am rather surprised that Holland concentrates his attention on the differences between the two means of Y_t and Y_c , whereas other differences in the distributions of these two variables, such as variances, could be very important to someone reaching a decision on the basis of the experiment. After all, in decision making under uncertainty, risk is as important as expected return.

My own particular interest is in temporal causality. I think that necessary conditions for a cause are that it occurs before the effect and contains unique information about it. From these ideas, it follows that knowing the cause helps forecast many aspects of the effect, and tests can be based on this simple idea. I do not see that the experimental context contradicts these ideas. I have also tried to emphasize that the purpose of causal analysis, including statistical analysis, is to try to change people's "degrees of belief," which might be conveniently summarized as a probability that a suggested causal relationship is true. These beliefs are required for decision making by economic agents. These views are expanded in two papers: Granger (1980, 1985).

The question obviously arises whether or not the experimental framework used here is also relevant for testing temporal causality. We may think of two types of experimental units—those with memory and those without. Examples of units without memory would be physical objects and possibly land or lower animals, the classical units used in experimental work. Certainly human subjects will have memory, as will many animals. If a unit has memory, it will be very difficult to devise a time sequence of experiments obeying Holland's requirements to test some theory about the effects of a price change or income level on consumption, say, because the idea of potentially being

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able to place every unit under every value of the controlled variable at every moment of time becomes less plausible. We are back to not being able to relate race or sex to income. Because of the memory, it seems that all such experiments become strictly impossible, as what happened in the past will potentially affect the outcome of the present experiment. It seems to me that most of the solutions to what Holland calls the "Fundamental Problem of Causal Inference" will no longer work in this case, including the "statistical solution," without conditioning on the past. I

am thus unclear that the experimental model is even theoretically helpful for temporal causality in the behavioral sciences. If one does condition on the past, the statistical solution may be relevant, but the basis for the inference will then be quite different from that proposed here.

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Rejoinder

PAUL W. HOLLAND

I thank all of the discussants for their very thoughtful comments. Not surprisingly, I agree more with the views expressed by Cox and Rubin than with those of Glymour and Granger, but each discussant makes important points that expand and illuminate issues that arise in the article. Space does not permit a response to every point mentioned, and the more critical comments of Glymour and Granger tend to be balanced by the comments of Cox and Rubin. Hence I will restrict my rejoinder to those issues that I feel need emphasis or to which I feel I can add a useful point of view.

In reflecting upon the discussants' remarks I realized that nowhere in the article, or elsewhere, is there a purely mathematical description of Rubin's model. Such a formulation ought to help separate the *model* itself from its *applications*. For this reason I will begin my rejoinder with a brief, mathematical statement of Rubin's model and its interpretation in terms of my article. Then I will address some of the issues raised by each discussant.

1. A MATHEMATICAL STATEMENT OF RUBIN'S MODEL

In its simplest form, stripped of all of the interpretative language, Rubin's model is a quadruple, $R = (U, K, Y, S)$, in which U and K are sets, Y is a real-valued function defined on $U \times K$, and S is a mapping from U to K . In the language of the article the meaning of the components of R is as follows. U is the population of units, and K is a set of labels or descriptions of the various causes or treatments under consideration. For any $u \in U$ and $k \in K$, $Y(u, k)$ is the value of the response that would be measured on u if u were exposed to cause k . The value of $S(u)$ is the cause or treatment to which u is actually exposed prior to the measurement of the response. In the article I used the equivalent subscript notation, that is, $Y_k(u) = Y(u, k)$, and I let $K = \{t, c\}$. Of course, in general K could contain more than two elements.

In real applications of Rubin's model other measurements besides the response Y need to be represented. I

think that all measurements should be regarded as functions defined on $U \times K$, just as Y is. If X is such a function, then $X(u, k)$ is the value of the X measurement that would be made on u if u were exposed to cause $k \in K$. One special type of measurement needs mention here. If the value of $X(u, k)$ does not depend on which cause k to which u is exposed I shall call X an *attribute* of u ; that is, $X(u, k) = X(u)$ for all $u \in U$ and $k \in K$. Important examples of attributes are (a) pre-exposure variables (Sec. 3) and (b) post-exposure variables that cannot be affected by k . Among the measurements that are *not* attributes I include other response variables besides Y and "post-treatment concomitant variables" (Rosenbaum 1984b).

The purpose of Rubin's model is to provide a language for discussing causation, and this language takes *units*, *causes*, and *responses* as primitive notions that are not defined further. These three elements, however, are not arbitrary and must satisfy the basic property that Y is defined on all of $U \times K$. The *effect* of cause t relative to c is then defined in terms of these primitive notions, that is, as $Y(u, t) - Y(u, c)$, and the observed response on each unit is also defined in terms of the elements of R , that is, $Y_S(u) = Y(u, S(u))$.

By taking units, causes, and responses as the primitives of his theory and defining effects and observed data in terms of them, Rubin's model breaks with an ancient philosophical tradition that takes "events" or "phenomena" as primitives and attempts to define what is meant by one event being *the cause* of another.

An *application* of Rubin's model requires an identification of the elements of R with features of a real-world problem. What are the units, the causes, the responses? How are units actually exposed to the action of the causes? Is Y defined on all of $U \times K$? If the identification of the elements of the real-world application with those of Rubin's model leads to a faithful representation of the real-



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An *application* of Rubin's model requires an identification of the elements of R with features of a real-world problem. What are the units, the causes, the responses? How are units actually exposed to the action of the causes? Is Y defined on all of $U \times K$? If the identification of the elements of the real-world application with those of Rubin's model leads to a faithful representation of the real-

world situation by the model, then it becomes a useful framework for making statements about cause and effect. If the representation is not faithful, then Rubin's model does not apply and cannot be used to make causal statements. The question of the "faithfulness" of a particular representation is, in my opinion, one on which people may disagree. For example, it is usually easy to make an identification of U , K , and Y with units, treatments, and a response variable in a randomized experiment, but complex observational studies can provide cases in which reasonable people might disagree as to the proper identification of the elements of the model.

2. RUBIN'S COMMENTS

What Rubin's SUTVA lacks in mellifluence it more than makes up for in utility. I view the SUTVA as a general purpose way of checking on the faithfulness of a particular specification of Rubin's model as a representation of a real-world application. Rubin's comments on SUTVA are illuminating and I would like to add a few of my own.

One might wonder how Hume's notion of "temporal succession" fits into the abstract formulation of Rubin's model given earlier, which does not involve time, explicitly. I view temporal succession as a part of the *application* of the model rather than as a part of the model itself. For example, the value of $Y(u, k)$ is supposed to depend on u and k . For this to happen the exposure of u to k must occur prior to the measurement of $Y(u, k)$. This forces temporal succession upon us. Under SUTVA the value of $Y(u, k)$ depends on (u, k) but *does not depend on anything else*. SUTVA and temporal succession are, therefore, two sides of the same coin. As Rubin points out, Fisher's null hypothesis corresponds to $Y(u, k) = Y(u)$ for all $k \in K$. I will point out that unit homogeneity (Sec. 4.2) corresponds to the parallel assumption that $Y(u, k) = Y(k)$ for all $u \in U$. Both Fisher's null hypothesis and unit homogeneity are special cases of SUTVA.

Rubin is correct in pointing out that I view as meaningless "causal" statements in which the "cause" is an attribute of the units. By this I simply mean that causal effects are not well defined in such cases, because Y is not defined on all of $U \times K$, as I discuss in Section 9. Rubin accepts such statements as meaningful in circumstances when they can be construed as rejections of Fisher's null hypothesis that are made without clear statements as to what c is or what $Y_c(u)$ is. Glymour also objects to my use of the term "meaningless" on more general grounds. Rubin and Glymour may be right, but I would call such statements "causally innocuous," since they are of such a general nature as to have no useful consequence in the real world.

Rubin's analysis of Neyman's null hypothesis is illustrative of the value of Rubin's model. By using the SUTVA, Rubin gives meaning to Neyman's notion of "technical errors," which I ignored in my analysis. I ignored technical errors because I find their source of probability to be completely artificial. For example, Neyman (1935) described the source of probability in this way.

Suppose that we repeat the experiment indefinitely without any change in vegetative conditions or of arrangement so that the k th object is always

tested in plot (i, j) . The yields from this plot will form a population, say $\pi_{ij}(k)$ and $X_{ij}(k)$ will be defined as the mean of this population. (p. 110)

In fact, we cannot perform such an experiment over and over again, so what did Neyman really intend? I think that Rubin's analysis is very neat and that it does give a meaning to Neyman's technical errors that is easy to understand and that can lead to interesting statistical analyses.

Nevertheless, I think that the problem Neyman and Fisher were addressing does not depend on the existence of technical errors and would still be there if SUTVA were satisfied with only two causes in K (as I assumed). Readers will have to judge for themselves which analysis they prefer, but I encourage Rubin to provide us with a full-blown analysis of the Latin square along the lines indicated in his discussion, as this may add another interesting chapter to this classic problem.

3. COX'S COMMENTS

It was a relief to find that Cox agrees with me that "certain variables cannot properly be regarded as causes." After reading the comments of the other discussants I was beginning to wonder if this view, which I regard as perfectly obvious, was shared by no one else.

I think Cox's term *intrinsic variable* is what I have meant by *attribute* in this rejoinder. Intrinsic variables that are "associated with the environment" can be competing, uncontrolled causes, but I do not believe that they need to be treated as such in the analysis of experiments or observational studies. After all, rainfall and soil fertility may be associated with each other in complicated ways, but it is possibly best to regard them as attributes of a given field over a given time period.

Cox raises what I regard as a very important point about "unit-treatment additivity" or, as I prefer to call it, the assumption of constant effect (Sec. 4.4). If there are no other measured variables besides Y , then it is impossible to falsify the constant effect assumption *with the data in hand*. This is true regardless of the sample size. When there is an attribute or intrinsic variable \mathbf{X} on the scene, then we may be able to falsify the constant effect assumption, but we cannot falsify the *conditional* constant effect assumption that holds conditionally for each value of \mathbf{X} , that is,

$$T(\mathbf{x}) = Y_t(u) - Y_c(u)$$

for all $u \in U$ such that $\mathbf{X}(u) = \mathbf{x}$.

It is natural to consider applying Occam's razor to such situations and to make the appropriate (conditional) constant effect assumption the starting point for analyses of such data. Such a view makes one sympathetic with Fisher's side of the Fisher/Neyman argument described in Section 6, in my opinion.

4. GLYMOUR'S COMMENTS

I am extremely grateful for Glymour's willingness to bring a philosopher's point of view to this discussion. Rubin and I have always been aware of the "subjunctive" quality of the definition of a causal effect—the "woulds," "ifs,"

and "weres" of that definition—but I was not aware of the relevance of counterfactual conditionals until I read Glymour's comments on my article. I especially like the notion of "possible worlds," since this is what I think the function Y is intended to represent. For unit u , $Y(u, \cdot)$ represents all of the relevant possible worlds for u . On the other hand, $S(u)$ and $Y(u, S(u))$ described the world that actually exists (for observational studies) or the world that will be observed (for experiments) for unit u .

I must disagree with Glymour's paraphrasing of my (i.e., Rubin's) analysis, however, and with the counterfactual analysis of causation of Lewis described by Glymour. I believe that there is an unbridgeable gulf between Rubin's model and Lewis's analysis. Both wish to give meaning to the phrase " A causes B ." Lewis does this by interpreting " A causes B " as " A is a cause of B ." Rubin's model interprets " A causes B " as "the effect of A is B ." Rubin adopts the notion of an experiment as the fundamental way of thinking about causation, studying the effects of known causes. Lewis starts with the effect and, like Aristotle, seeks to define what it means to be a cause of that effect. Can Rubin's model ever define what it means for " A to be a cause of B "? I do not think so. In Section 9, I convinced myself, at least, that statements like " A is a cause of B " are generally false and always depend on our current state of knowledge. Notice that once a statement of the form "the effect of A is B " has been experimentally verified it does not go away or become false as our knowledge of the subject increases. Old, replicable experiments never die, they just get reinterpreted.

I think that Glymour's criticisms are more directed at the way in which Rubin's model might be applied than at the model itself. For example, he interprets my use of "attribute" to refer to such things as "genetic constitution" and then points out that we might be willing to "identify persons across . . . some alterations in genetic structure." Such identification would produce an attribute that is a cause. I would see it differently. As technology evolves so do the types of causes or treatments that can be applied. This is the history of medicine, for example. The units must change, of course. In the Down's syndrome example, they change from a person to a zygote. What was an attribute at one level could be manipulated at a different level of analysis.

5. GRANGER'S COMMENTS

I agree with Granger that statisticians are all too willing to shirk the responsibility of addressing issues of causality. His work in this area captures aspects of causation that many find attractive—compare Sections 5.3 and 8.2. His point of view is quite different from that discussed in the article. To illustrate this consider Granger's example of a cross-sectional causal question, for example, "why does one household spend more on electricity . . . than does another." This is a comparison of the responses of two distinct units, for example, $Y_S(u_1)$ versus $Y_S(u_2)$, rather than a comparison of the responses of a unit under two causes, for example, $Y_t(u_1)$ versus $Y_c(u_1)$. I regard the val-

ues of $Y_S(u_1)$ and $Y_S(u_2)$ to be of no causal interest unless they shed light on the value of a causal effect such as $Y_t(u_1) - Y_c(u_1)$. The Fundamental Problem of Causal Inference (Sec. 3) must be faced and overcome in some way so that the data values $Y_S(u_1)$ and $Y_S(u_2)$ can answer causal questions. By focusing on the observed data Granger overlooks what I regard as real causal questions that must, of necessity, be couched in terms of information, of which only some can be observed.

I agree with Granger that experiments are not always possible to do in many branches of science and that even when they are, they may not actually answer the questions of interest—note Cox's fertilizer/bird example in this regard. I disagree, however, with the implication that the experimental (i.e., Rubin's) model tells us nothing about nonexperimental causal research. In my opinion, there is no difference in the conceptual framework that underlies both experiments and observational studies—Rubin's model is such a framework. In observational studies we know less about the situation than we do in experimental studies and this lack of information simply serves to make causal inferences from observational studies more speculative than they are in experiments.

Granger expresses the view that the experimental model is not helpful in problems of "temporal causality," which he defines as "causal questions about data that are a group of time series." The idea that Rubin's model is somehow incapable of accommodating time-series data is misleading. There is no reason why the response Y cannot be a function of time rather than simply a real number. Thus $Y(u, k, \alpha)$ is the value of the response that would be measured at time α on unit u if u were exposed to k . The observed data are $Y(u, S(u), \alpha)$ for all relevant α values. Causal effects are more complex than before, since they now involve comparisons of functions, that is, $Y(u, t, \cdot)$ and $Y(u, c, \cdot)$. This might be done with functionals that associate single numbers with $Y(u, t, \cdot)$. More complicated issues arise if the causes of interest are themselves functions of time; that is, K is a set of functions and $k(\alpha) \in K$ describes the "level" of a cause at time α . These added complexities have not been analyzed carefully as far as I know and ought to be pursued to clarify the problem of causal inference in a time-series setting. Careful attention to Hume's "temporal succession" is critical in such settings.

Finally, I must strongly disagree with Granger's (and I believe Glymour's) view that, for example, questions such as "race . . . affects . . . crime rates" and "the death sentence cause(s) decreases in murder rates" are on the same causal footing. In the former, "race" cannot be manipulated, whereas in the latter "the death sentence" is manipulated by governors and legislators all the time. The former is an associational statement that is not uninflammatory, and the latter is a causal statement of great public policy interest—regardless of how well or poorly it may have been studied by enthusiastic regression modelers. Granger's theory of temporal causality as expressed in Section 8.2 and in his comments contains, in my view, too generous a definition of causality. I find it, at bottom, indistinguishable from association.