Social Mechanisms and Causal Inference

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Several authors have claimed that mechanisms play a vital role in distinguishing between causation and mere correlation in the social sciences. Such claims are sometimes interpreted to mean that without mechanisms, causal inference in social science is impossible. The author agrees with critics of this proposition but explains how the account of how mechanisms aid causal inference can be interpreted in a way that does not depend on it. Nevertheless, he shows that this more charitable version of the account is still unsuccessful as it stands. Consequently, he advances a proposal for shoring up the account, which is founded on the possibility of acquiring knowledge of social mechanisms by linking together norms or practices found in a society.

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A central problem confronting social research is that an association between two variables can often be explained by the hypothesis that one is a cause of the other or that both are effects of a common cause. Given the general impossibility of performing experiments and the difficulty of knowing whether all possible common causes have been taken into account, one is faced with a serious challenge to the possibility of making reliable inferences about the causes of social phenomena. It is sometimes claimed that the consideration of social mechanisms can significantly ameliorate this problem (Elster 1983, 47-48; Little 1991, 24-25; Hedström and Swedberg 1999, 9). In this article, I inquire into whether mechanisms can indeed perform this service and, if so, how.

The case on behalf of mechanisms is sometimes posed in terms of the claim that reliable causal inference in social science is impossible

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without knowledge of mechanisms, a proposition that Harold Kincaid (1996, chap. 5) has disputed. Although I agree with Kincaid in rejecting the claim that mechanisms are always necessary for causal inference in social science, I also maintain that the proposal can be made independently of that proposition. On the interpretation I suggest, the account of how mechanisms assist causal inference in social science has a positive and a negative aspect. On the positive side, we can infer that \( X \) is a cause of \( Y \) if we know that there is a mechanism through which \( X \) influences \( Y \). The negative flip side is that if no plausible mechanism running from \( X \) to \( Y \) can be conceived of, then it is safe to conclude that \( X \) does not cause \( Y \), even if the two variables are probabilistically dependent. As I explain, neither of these lines of reasoning entails that mechanisms are necessary for causal inference in social science, and consequently they are not undermined by the criticisms raised by Kincaid.

Nevertheless, I argue that this account of how mechanisms significantly assist causal inference in social science is not successful as it stands. The positive account is not helpful unless some explanation is given of how it is possible to learn about social mechanisms without running afoul of the problem of unmeasured common causes. Yet to the best of my knowledge, no such explanation has hitherto been provided. On the other hand, the effectiveness of the negative side is undermined by the ease of imagining mechanisms connecting nearly any two variables representing aspects of social phenomena.

However, I propose that the positive side of the account can be shored up, thereby generating a better explanation of the value of mechanisms to causal inference in social science. I argue that knowledge of social norms and practices can be attained in a manner that is independent of the problem of unmeasured common causes. Furthermore, by examining how several social practices are linked together, one can sometimes make a persuasive argument for a qualitative causal hypothesis to the effect that one variable is a cause of another. I illustrate this argument with several examples drawn from social science, particularly from anthropology.

WHAT ARE SOCIAL MECHANISMS?

One of the earliest explicit uses of the term mechanism in the context of social science occurred in Robert Merton’s (1968) essay, “On Sociological Theories of the Middle Range,” first published in 1949. In that
essay, Merton advocated a social theory that refrains from grand systems that attempt to encompass all aspects of human society. Instead, he proposed that social theory should focus on hypotheses concerning the dynamics of specific types of social interactions. Middle-range theories, in Merton’s account, relied heavily on mechanisms (p. 43). One of Merton’s most well-known mechanisms is the self-fulfilling prophecy, which he illustrated by means of the example of a run on a bank and, more interestingly, through the self-reinforcing aspects of racial discrimination (chap. 13).

In spite of Merton’s prominence, the idea that mechanisms should occupy a central place in social theory did not catch on in sociology, and little effort was dedicated to clarifying the notion (Hedström and Swedberg 1999, 6). Consequently, the philosophical literature on social mechanisms is not very extensive, being mostly due to a handful of authors (cf. Elster 1983, 1985, 1989; Stinchcombe 1991; Little 1991, 1998). However, there has been some revival of interest in the topic as of late (cf. Hedström and Swedberg 1996, 1999), and the renewed interest in social mechanisms has had some influence on actual research (cf. Brochmann and Hammar 1999, 19-20). One of the main motivations for interest in social mechanisms is the conviction that adequate explanations in general, and of social phenomena in particular, require an explication of underlying causal processes in addition to a recognition of patterns of correlation and dependence among aggregate-level variables (cf. Harré and Secord 1979; Elster 1989; Little 1998; Hedström and Swedberg 1999). This aspect of the social mechanisms literature, however, is not the focus of the present article, which examines the proposal that social mechanisms provide an effective basis for ameliorating the problem of confounders.

But before launching into the argument, some initial clarification of the notion of a social mechanism is in order. Mechanisms in general can be roughly characterized as sets of entities and activities organized so as to produce a regular series of changes from a beginning state to an ending one (cf. Machamer, Darden, and Craver 2000). Social mechanisms in particular are usually thought of as complexes of interactions among individuals that underlie and account

1. Merton (1968) explicitly refers to the dynamic of the self-fulfilling prophecy as a mechanism (cf. p. 182).
2. This conception of scientific explanation is expressed in rather different ways by Salmon (1984) and Hesse (1966).
3. See Bechtel and Richardson (1993) and Glennan (1996) for additional discussions of the concept of a mechanism.
for aggregate social regularities (cf. Little 1991, 13; Stinchcombe 1991, 367; Schelling 1999, 33; Gambetta 1999, 102). But there is more to social mechanisms than just individual interactions: typically, the individuals are categorized into relevantly similar groups defined by a salient position their members occupy vis-à-vis other members of the society (cf. Hernes 1999). In the description of the mechanism, the relevant behavior of an individual is often assumed to be a function of the group into which he or she is classified. For example, consider the anthropologist Bronislaw Malinowski’s (1935) account of how having more wives was a cause of increased wealth among Trobriand chiefs. Among the Trobrianders, men were required to make substantial annual contributions of yams to the households of their married sisters. Hence, the more wives a man had, the more yams he would receive. Yams, meanwhile, were the primary form of wealth in Trobriand society and served to finance such chiefly endeavors as canoe building and warfare. Although individuals play a prominent role in this account, they do so as representatives of social categories: brothers-in-law, wives, and chiefs.

The categorization of component entities into functionally defined types is not unique to social mechanisms. Biological mechanisms (e.g., that of HIV replication) are often described using such terms as enzyme and coreceptor. The terms enzyme and coreceptor resemble chief and brother-in-law in virtue of being functional: all of these terms provide some information about what role the designated thing plays in the larger system of which it is a part. The existence of social categories into which the individuals composing the mechanism are important insofar as the social roles will usually persist far longer than the particular individuals who occupy them, as is illustrated by the Trobriand example. Stable social roles, therefore, are a source of constancy for the mechanisms in which they are involved and for the higher level causal generalizations that depend on these mechanisms.

Social mechanisms are sometimes tied to the assumption that the individuals composing them are rational, say in the sense of being utility maximizers. For instance, Tyler Cowen (1999) writes, “I interpret social mechanisms . . . as rational-choice accounts of how a specified combination of preferences and constraints can give rise to more complex social outcomes” (p. 125). I shall not adopt this

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4. Two well-known examples are Thomas Schelling’s (1978, chap. 4) proposed mechanism to explain patterns of ethnic segregation in residency and Amartya Sen’s (1981) entitlement approach to famines.
Hypotheses about social mechanisms will not be restricted to ones in which the individuals in question are represented as rational agents.

In sum, social mechanisms can be characterized as follows. Social mechanisms are complexes of interacting individuals, usually classified into specific social categories, that generate causal relationships between aggregate-level variables. A mechanism will be said to be from the variable X to the variable Y if it is a mechanism through which X influences Y.

MECHANISMS AND THE PROBLEM OF CONFOUNDERS

A great deal of social science involves collecting aggregate data relevant to some phenomena of interest (e.g., through government records or surveys) and performing statistical tests to decide whether pairs of variables are probabilistically dependent conditional on sets of other variables. In some cases, the purpose of such inquiries might be solely to identify factors that can serve as useful forecasting tools, but often the goal is to discover what variables cause which others. In the social sciences, this leads directly to the thorny problem making causal inferences without the aid of experiment. The classic obstacle to such inferences is that a probabilistic dependence between two variables might be explained either by one variable being a cause of the other or by the existence of a common cause of both. We can call this the problem of confounders, where the term confounders refers to common causes, often unmeasured, that might explain an observed correlation. The challenge that the problem of confounders poses for social science is expressed nicely by Elster (1983):

By the latter [spurious explanations] one usually refers to a correlation between two variables that does not stem from a causal relation between them, but from their common relation to some third variable. . . . The danger of confusing correlation and causation is a constant problem in this mode of statistical explanation. (P. 47)

Though progress has been made in recent years on the topic of causal inference from observational data (cf. Glymour and Cooper 1999; Steel / SOCIAL MECHANISMS AND CAUSAL INFERENCE 59

5. See Henderson (1993, 1995) for a thoroughgoing defense of the position that the interpretation of beliefs and preferences need not suppose that subjects are rational. See Rosenberg (1988, 1995) for an example of the contrary view.
Pearl 2000; Spirtes, Glymour, and Scheines 2000), the problem of confounders remains both extremely difficult and of central importance to the social sciences.

Mechanisms are sometimes proposed as the basis of a partial solution to the problem of confounders. For example, according to Little (1991),

We can best exclude the possibility of a spurious correlation between variables by forming a hypothesis about the mechanisms at work in the circumstances. If we conclude that there is no plausible mechanism linking nicotine stains to lung cancer, then we can also conclude that the observed correlation is spurious. (Pp. 24-25)

The general principle concerning mechanisms and causation on which the argument in this passage rests is the following:

(M) \(X\) is a cause of \(Y\) if and only if there is a mechanism from \(X\) to \(Y\).\(^6\)

Clearly, (M) is not intended as a universally true principle regarding causality, since there is presumably some “rock bottom” level of physical causation below which no mechanisms lie. Thus, (M) should be understood as being restricted to complex systems composed of multiple, interacting components, for instance, an organism or a society.

Given (M), it follows that if we know that there is a mechanism from \(X\) to \(Y\), then we can conclude that \(X\) is a cause of \(Y\). Conversely, if we have good reason to believe that there is no mechanism from \(X\) to \(Y\), then we can conclude that \(X\) is not a cause of \(Y\). The latter of these two corollaries of (M) is illustrated by Little’s example about nicotine stains and lung cancer. Hedström and Swedberg (1999) make the same point with a different example:

Some epidemiological studies have found an empirical association between exposure to electromagnetic fields and childhood leukemia. However, the weight of these empirical results is severely reduced by the fact that there exists no known biological mechanism that can explain how low-frequency magnetic fields could possibly induce cancer. . . . The lack of a plausible mechanism increases the likelihood that the weak and rather unsystematic empirical evidence reported in this epidemiological literature, simply reflects unmeasured confounding factors rather than a genuine cause relationship. (P. 9)

Elster (1983, 47-48) provides a similar example, though with a slight twist, that we will consider below.

Thus, the argument for the importance of mechanisms to causal inference in the social sciences is that knowledge of what mechanisms are present, or absent, can rule out alternatives that are consistent with statistical data. In this way, inquiries into mechanisms are asserted to significantly ameliorate the problem of confounders in social science.

KINCAID'S OBJECTIONS

Although the proposal described in the foregoing section maintained that inquiries into social mechanisms can significantly ameliorate the problem of confounders, no claim was made to the effect that they are necessary for resolving this problem. However, that claim is sometimes made in close association with the proposal described above. For example, according to Little (1995), “it is only on the basis of hypotheses about underlying causal mechanisms that social scientists will be able to use empirical evidence to establish causal connections” (pp. 53-54, italics added). Kincaid (1996, 179-82) raises two objections to the claim that social mechanisms are necessary for causal inference in the social sciences, the first of which takes the form of a reductio ad absurdum.

In Kincaid’s (1996) discussion, the proposition at issue is that “we need to identify individualist mechanisms to confirm causal relations between social variables” (p. 179). Let us formulate this proposition in the following way:

(M*) One knows that \( X \) is a cause of \( Y \) only if one can identify at least one mechanism from \( X \) to \( Y \).

Kincaid’s reductio ad absurdum then proceeds as follows. Suppose that a mechanism relating two aggregate-level social variables is demanded to support the claim that one of the variables is a cause of the other.

Do we need it at the small-group level or the individual level? If the latter, why stop there? We can, for example, always ask what mechanism

brings about individual behavior. So we are off to find neurological mechanisms, then biochemical, and so on. (P. 179)

Given (M*), therefore, demands for mechanisms can be pressed all the way down to fundamental physics, yielding the absurd result that no causal claim can be established unless such impossible amounts of detail are provided.

One might try to defend (M*) from such objections by maintaining that it is only intended to apply to fields in which experiment is not possible and not all common causes can be measured (Little 1998, 10-12). Since controlled experiments are routine in such fields as neuroscience and molecular biology, this would nip Kincaid’s reductio ad absurdum in the bud. However, I think that a simpler line of response is to agree with Kincaid that (M*) is false but to point out that it is not required for the proposal described in the foregoing section. That proposal rested on the proposition (M), which stated that \( \text{X is a cause of Y} \) if and only if there is a mechanism from X to Y. The target of Kincaid’s reductio ad absurdum, meanwhile, is (M*). But does (M) entail (M*)? Defenders of mechanisms in social science sometimes seem to presume that it does. For example, consider the following statement of Little’s (1995):

I maintain that the central idea of causal ascription is the idea of a causal mechanism: to assert that A causes B is to assert that A in the context of typical causal fields brings about B through a specific mechanism (or increases the probability of the occurrence of B). This may be called “causal realism,” since it rests on the assumption that there are real causal powers underlying causal relations. This approach places central focus on the idea of a causal mechanism: to identify a causal relation between two kinds of events or conditions, we need to identify the typical causal mechanisms through which the first kind brings about the second. (P. 34, italics added)

Notice that the first sentence in this quotation is a clear statement of (M): there is a causal relationship just in case there is an underlying mechanism. In contrast, one interpretation of the italicized sentence is as a statement of (M*): mechanisms must be identified before we can claim to know that one variable is a cause of another. However, (M) does not entail (M*).

To see the point, imagine a person who accepts (M) but who also regards randomized controlled experiments as a reliable means of learning about cause and effect. Suppose that a randomized controlled experiment establishes that X is a cause of Y. Then the person
concludes from (M) that there is a mechanism from X to Y. Nevertheless, the person may not be able to identify any mechanism from X to Y; in short, she knows that there is a mechanism but not necessarily what this mechanism is. Therefore, such a person would not be committed to taking the stance that serves as the basis of Kincaid’s reductio ad absurdum; that is, she would not be committed to (M*). Her inability to identify a mechanism is compatible with her knowledge that there is a mechanism and, hence, with her knowledge of a causal relationship. In general, one can consistently accept (M) while rejecting (M*) by holding, reasonably enough, that the identification of mechanisms is not the only possible way to learn about cause and effect.

Kincaid’s (1996) second objection to (M*) is that there are ways of distinguishing between cause and mere correlation available to social scientists that have nothing to do with mechanisms, particularly, by conditioning on potential confounders (pp. 179-80). I agree that there are cases in which one can draw reasonable conclusions about what causes what without the aid of experiment or substantial knowledge of underlying mechanisms. However, the usefulness of conditioning on potential common causes does not undermine the proposal that mechanisms significantly aid causal inference in the social sciences, since social scientists are rarely able to measure all potential common causes. Indeed, the inability to exhaustively consider all potential common causes is a basic element of the problem of confounders, to which mechanisms are being considered as a partial solution.

In addition, evidence regarding conditional dependencies and independencies may fail to unambiguously identify causal structure even when all potential common causes have been measured. For example, consider Figure 1. Both graphs in this figure predict that X and Y are probabilistically dependent. Moreover, both predict that X and Y are independent conditional on Z. So, measuring Z would not enable us to decide whether X is a cause of Y, even if there were no other confounders.

Elster’s (1983, 48) argument for the importance of mechanisms to causal inference in social science is motivated by an example that

8. For example, see Spirtes, Glymour, and Scheines (2000, chap. 9) for a discussion of how it is possible, in certain favorable circumstances, to infer causal structure from statistical data without significant initial knowledge of underlying causal relationships and without the assurance that all common causes have been measured.

9. See Spirtes, Glymour, and Scheines (2000, chap. 4) for a thorough discussion and more complex examples of statistically indistinguishable causal graphs.
illustrates the same point. In Elster’s example, the variable X represents “the percentage of female employees who are married” and Y represents “the average number of absences per week per employee” (p. 48). Elster supposes that X and Y are positively correlated but that they are independent conditional on a third variable Z, “the amount of housework performed per week per employee” (p. 48). Both causal graphs in Figure 1 can explain this imagined statistical evidence; hence, we are unable to decide from that evidence alone whether X is a cause of Y. However, Elster suggests, since there is no plausible mechanism through which Z could influence X, we can conclude that Z is not a cause of X and, hence, not a common cause of X and Y. The only remaining alternative, therefore, is that X is a cause of Z, which in turn is a cause of Y. Thus, this example illustrates how (M) might be used to establish a positive causal conclusion that could not have been reached through the examination of statistical data alone.

In sum, Kincaid has effectively criticized (M*), the proposition that one can know that X is a cause of Y only if one can identify a mechanism from X to Y. However, (M*) is not required for the account presented in the foregoing section of how inquiries into mechanisms play a central role in causal inference in the social sciences. Nevertheless, that proposal leaves much to be desired.

NO PLAUSIBLE MECHANISM

There is, as was noted above, a positive and a negative side of the account of the importance of mechanisms to causal inference in the social sciences. The positive side rests on the premise that we can show that X is a cause of Y if we can discover a mechanism from X to Y. The negative side relies on the premise that we can infer that X is not a cause of Y if we know that there is no plausible mechanism from X to Y. It was the negative side that was illustrated by Little’s nicotine
stains and lung cancer example, Hedström and Swedberg’s example concerning electromagnetic fields and child leukemia, and Elster’s example about the proportion of female employees and number of missed workdays. In this section, I argue that the negative side of the account of the importance of mechanisms to causal inference in social science is unsuccessful.

The problem lies in the ease of imagining social mechanisms through which nearly any aggregate-level social variable can influence another. Thus, it is rarely the case that no plausible mechanism can be imagined that could connect two variables representing aspects of social phenomena. Consider, for instance, a well-known example from the sociological literature discussed by one of the contributors to Hedström and Swedberg’s (1999) volume, Diego Gambetta. The example is the association—reported in Samuel Stouffer’s (1949) *The American Soldier*—between satisfaction and opportunity for advancement among military personnel.

Surprisingly, soldiers in branches of the military offering little opportunity, such as the military police, were on average more satisfied with their positions than those in branches with greater chances for advancement, such as the army air corps. Gambetta (1999) describes five mechanisms proposed by sociologists over the years to account for how greater opportunity could cause less satisfaction (pp. 114-19). However, he does not consider the alternative possibility that opportunity has little or no negative influence on happiness and that the association found by Stouffer is due to an unmeasured common cause. For example, it is possible that extremely ambitious people are much more likely to embark on career paths that promise greater opportunities for advancement and that their lofty aspirations are also more likely to make them dissatisfied with their current stations in life. Listing possible mechanisms through which opportunity could produce unhappiness does nothing to rule out this plausible alternative. Indeed, this case illustrates how an overabundance of plausible mechanisms is a major source of difficulty for causal inference in the social sciences.

No doubt there are some pairs of variables $X$ and $Y$ representing aggregate aspects of social phenomena such that no plausible mechanism through which $X$ causes $Y$ can be imagined. However, I suspect that such cases are too few and far between for the no-plausible-mechanism strategy to be of much use in distinguishing cause from mere correlation in social science. Although Elster, Little, as well as Hedström and Swedberg illustrate their arguments with an example,
only Elster’s—a toy example not based on actual research—has any relation to social science. Despite their interest in doing so, these authors apparently found it difficult to produce a serious example of actual social research in which the inability to imagine a plausible mechanism from one social variable to another significantly aided causal inference.10

PROCESS TRACING AND INTERPRETATION

As we saw, (M) can be used to generate a positive as well as a negative account of the value of mechanisms to causal inference in social science. Having found the negative proposal wanting, let us turn to the positive one. From (M) it follows that if we know that there is a mechanism from $X$ to $Y$, we can infer that $X$ is a cause of $Y$. The difficulty is that it is unclear how we are to learn about mechanisms in a way that does not run directly into the problem of confounders, which was the problem that mechanisms were supposed to help us overcome. For example, consider Little’s (1991) discussion of how one acquires knowledge of mechanisms:

To credibly identify causal mechanisms we must employ one of two forms of inference. First, we may use a deductive approach, establishing causal connections between social factors based on a theory of the underlying process. . . . Second, we may use a broadly inductive approach, justifying the claim that a caused b on the ground that events of type A are commonly associated with event of type B. . . . But in either case the strength of the causal assertion depends on the discovery of a regular association between event types. (P. 30)

Thus, according to Little, the identification of causal mechanisms depends on prior knowledge of probabilistic dependencies among variables.11 But the problem of confounders immediately rears its ugly head at this juncture, since the probabilistic dependence might result from a common cause rather than from A being a cause of B. The

10. There is a basis for ruling out the possibility that X is a cause of Y that is frequently appealed to in social research, namely, that Y is temporally prior to X. However, this reasoning depends only on the principle that an effect cannot precede its cause in time, which one might maintain independently of any convictions regarding mechanisms. For example, see David Lewis’s (1986) essay, “Counterfactual Dependence and Time’s Arrow.” Of course, that Y is prior in time to X does not rule out the possibility of common causes of Y and X.
11. Little (cf. 1998, 213-14) reiterates the same position in more recent writings.
procedure that Little describes for identifying mechanisms, therefore, presupposes that some means already exist for resolving the problem of confounders, which has the consequence that it is unable to shed any light on how the problem of confounders might be alleviated by the identification of mechanisms.

Hence, to make the positive side succeed, it is necessary to explain how one might learn about social mechanisms in a way that does not presuppose a prior solution to the problem of confounders. That is the task that I undertake in what follows. My proposal is nicely illustrated by Malinowski’s (1935) hypothesis that the possession of many wives was a cause of wealth and influence among Trobriand chiefs. Malinowski’s evidence for this hypothesis is primarily nonstatistical; it consists of descriptions of social processes in Trobriand society that any reasonably skilled ethnographer could produce. First, there is a custom whereby brothers contribute substantial gifts of yams to the households of their married sisters—gifts that are larger than usual when the sister is married to a chief. Second, political endeavors and public projects undertaken by chiefs are financed primarily with yams. If we grant that Malinowski was right about these two points, it is difficult to avoid the obvious conclusion that the number of wives had an influence on wealth among Trobriand chiefs.

Malinowski’s reasoning is an example of what I shall call process tracing.12 Process tracing consists in presenting evidence for the existence of several prevalent social practices that, when linked together, produce a chain of causation from one variable to another. A successful instance of process tracing, then, demonstrates the existence of a social mechanism connecting the variables of interest. For instance, in the case just described, the social practices are (1) that brothers are required to contribute a substantial store of yams to the households of their sisters and (2) that yams are the primary means used by chiefs to finance their various endeavors. Supposing that Malinowski was right about (1) and (2), the conclusion that the number of wives had an influence on wealth among Trobriand chiefs is unavoidable. The most difficult part of arguing for a causal hypothesis via process tracing,

12. This term is borrowed from Little (1995, 43-44; 1998, 211). Little’s use of the term differs from mine, however. In particular, Little (1995, 43; cf. 1991, 29-31) uses the term to refer to inquiries into the causal processes leading up to particular events. In addition, Little seems to regard process tracing primarily as a means of formulating hypotheses about causal mechanisms that are then assessed by means of statistical data (cf. Little 1998, 212-13). Clearly, such a proposal is problematic when combined with the suggestion that inquiries into social mechanisms are a key strategy for resolving the problem of confounders.
therefore, usually is constructing a convincing case that the posited
social processes actually exist. Once the relevant social practices are
given, the logical implications of their joint operation often follow
straightforwardly.

Malinowski’s argument is not an isolated anomaly. Arguments
employing process tracing are often found in regions of social science
in which one is interested in questions about what causes what but in
which good statistical data are unavailable. For example, consider
Brian Ferguson’s account of the effect of the introduction of such manu-
factured items as steels tools on indigenous warfare in the context of
colonial expansion and in the political consolidation of postcolonial
states, particularly among the Yanomami. Ferguson (1990) has long
argued that European colonial expansion profoundly reshaped
indigenous warfare in the Americas and elsewhere. One of the ways
in which European contact influenced warfare was through intro-
duction of manufactured valuables, particularly such steel tools as
machetes, axes, and knives. These items were often quick to become
necessities of life, but they differed significantly from their indige-
nous analogues in that they could not be manufactured locally. More-
over, in more than a few cases, these precious items were available
only from a limited number of peripheral source points. Ferguson
(1984, 1995) argues that in such circumstances, groups close to the
source often attempt to establish a local monopoly on the flow of manu-
factured goods so as to be able to trade on advantageous terms with
their neighbors. Naturally, such monopolizing efforts were apt to
generate resentment among more remote groups, which might at-
ttempt to circumvent the monopolists or dislodge them by force.
Likewise, the would-be monopolists might resort to violence to main-
tain their privileged position.

Ferguson’s proposals concerning the effect of manufactured valu-
ables on indigenous warfare, although controversial, have been taken
seriously, and the major themes of his arguments have been taken up
13. Since Ferguson’s account of Yanomami warfare is so different from the popular
perception of the topic—according to which the supposed incessant violence of the
Yanomami is a grim portrait of our primitive ancestors—some background co-
ments are in order. The popular view of the Yanomami is primarily due, of course, to
Napoleon Chagnon’s (1968, 1974, 1988) famous depiction of them as the “fierce peo-
lple.” Most lay people, I think, would be surprised to learn that nearly every anthropolo-
gist who has seriously studied the Yanomami rejects Chagnon’s portrayal of them (see
Sponsel 1998 for a good literature review on this topic).
14. I located eight reviews of Ferguson’s book. Four are positive (Rivière 1996;
Chernela 1997; Pollock 1997; Harris 1996). One of these positive reviews was authored
by other authors. For example, the influence of competition over access to manufactured valuables on warfare has been examined in the case of native inhabitants of the Canadian Subarctic (Reedy-Maschner and Maschner 1999) and among the Amazonian group known as the Jívaro (Steel 1999).\(^{15}\) The data in such cases as these are typically of a very fragmentary nature: reports of missionaries, explorers, and ethnographers, as well as recollections of elderly informants. Even when such information is reliable, it rarely suffices for anything resembling a sophisticated statistical analysis.

Not surprisingly, then, process tracing plays an important role in the causal arguments in such cases. For example, the argument in the case of the Jívaro rests on the following claims (Steel 1999, 747-48):

1. Those planning an assassination typically consulted with influential men in their locale, and the pursuance of the killing was often contingent on receiving approval.
2. Securing access to local sources of trade goods, such as missions, was one of the primary sources of political influence among Jívaro men in the period under consideration.

Two background aspects of Jívaro society during the period of concern are also relevant here. First, according to the Jívaro theory of disease of the time, practically all death and illness are caused by malevolent sorcery. Thus, it was rare that no outstanding grievances and

by Marvin Harris (1977, 1984), a longtime champion of ecological explanations of Yanomami warfare. In his review, Harris abandons his ecological hypothesis in favor of Ferguson’s—at least as far as the Yanomami are concerned (Harris 1996, 416). A fifth review is generally positive in tone but offers no clear verdict of approval or disapproval (Heinen and Illius 1996). One review is mixed, acknowledging that Ferguson had made a major contribution to the issue and granting that Ferguson had shown that conflicts regarding steel tools have been an important cause of Yanomami warfare (Colchester 1996). Nevertheless, this reviewer remained skeptical about the importance of this cause in comparison to others. The only negative review by an anthropologist that I found was Chagnon’s (1996). I also found one very negative, brief, and sarcastic review written by a historian (Bellesiles 1998). In sum, aside from Chagnon, the reaction to Ferguson’s book among anthropologists has been mostly positive. Moreover, Yanomami specialists generally appear to regard his book as making a significant contribution that should reshape anthropological discussions of warfare. For example, in a review of the Yanomami warfare literature, Leslie Sponsel (1998) states that Ferguson’s “work should force anthropologists to reevaluate previous ethnographies as well as to evaluate and design future research in light of the distinct possibility that what was formerly believed to chronic, endemic ‘primitive’ or tribal warfare may actually have been triggered (or at least intensified) and transformed by contact (indirect or direct) with Western ‘civilization’” (p. 110).

\(^{15}\) See Ferguson (1990) and Ferguson and Whitehead (1992) for further references.
associated desires for revenge were present. Second, there was no formal social hierarchy or political structure, so that influential men became such through their own initiative and vigorous effort. In this context, one can easily imagine how fluctuations in the access to trade goods could alter the course of violence by means of 1 and 2. The man, A, wishes to take revenge on a more powerful enemy B, but first he consults with the local “great man” C. As it happens, both C and B are competing for access to a newly established mission. Thus, in spite of having no personal interest in A’s grievance against B, C agrees to actively assist A in the attack. On the other hand, imagine that B is an important trade partner of C. Then, B is very likely to strenuously oppose A’s plot to assassinate C. As with Malinowski’s hypothesis concerning the effect of wives on wealth among Trobriand chiefs, the hard part lies in making the case that the proposed social processes actually are present.

I regard process tracing as a procedure both for developing, or formulating, causal hypotheses and for providing evidence for them. Process tracing is not intended merely as a means of inventing intriguing new hypotheses, and it is clear that it must be more than this if it is to ameliorate the problem of confounders. For if the only evidence for hypotheses generated through process tracing consisted of statistical tests concerning aggregate-level variables, the problem of confounders would be confronted anew with no progress having been made toward its resolution. After all, the difficulty lies not in imagining hypotheses concerning the causes of social phenomena but in deciding which among the large number of such conceivable hypotheses is correct. Therefore, it is important to address concerns that a skeptic might have concerning the capacity of process tracing to provide evidence for causal claims.

A striking feature of Malinowski’s (1935) argument is that it is compelling yet utterly lacking in statistical sophistication of any kind. No large sample of data is produced to demonstrate a positive correlation between wealth and number of wives among Trobriand chiefs. Nor is any thought given to possible confounding common causes that could generate such a probabilistic dependence if it existed. And potential common causes are easily conceived of. For instance, one

16. See Steel (1999, 756-58) for a detailed account of an actual case of precisely this sort.
17. See Steel (1999, 759-60) for descriptions of cases of this type.
18. Thus, I follow many current philosophers of science (cf. Longino 1990; Kelly 1996) in rejecting the sharp distinction between the context of discovery and the context of justification drawn by logical empiricists (cf. Reichenbach 1949, 433-34).
could imagine that establishing alliances with other chiefs results both in having more wives (used as a means of cementing political bonds) and in greater wealth. As if by magic, Malinowski seems to have established that one variable is the cause of another without the aid of any experimental or statistical technique for dealing with the possibility of unmeasured common causes.

A skeptic might wonder whether applications of process tracing are in fact little more than tricks of smoke and mirrors masquerading as science. How does process tracing differ from mere storytelling? Surely, the problem of alternative hypotheses capable of accounting for the available evidence is not made to disappear through a description of salient social processes. To the extent that process tracing can provide a compelling argument for a causal generalization, the skeptic might continue, it relies on implicit causal generalizations already established by other means. Hence, the skeptic might conclude, if process tracing works at all, it does so only by presupposing a solution to the problem of confounders; consequently, an adequate explanation of how attention to mechanisms can ameliorate this problem has yet to be provided.

Let us consider these objections in turn. The first thing to be emphasized is that making an argument for a causal conclusion via process tracing requires marshalling evidence for claims concerning the social processes at work in the society in question. For instance, Malinowski has to convince us that the custom of brothers making significant contributions of yams to the households of their sisters is indeed a pervasive feature of Trobriand social life. Malinowski’s argument rises above the level of mere storytelling only to the extent that he succeeds in making this case.

It is also important to recognize how modest the accomplishments claimed by process tracing actually are. Without the aid of statistical data, the best one can hope to establish by means of process tracing is purely qualitative causal claims. For instance, in Malinowski’s example, all we can conclude is that there is at least one path through which the number of wives exerts a positive influence on wealth among Trobriand chiefs. Not only does this conclusion fail to specify anything about the strength of the influence generated by this mechanism, it does not even entail that the overall effect of the number of wives on wealth is positive. One would naturally presume that having more wives would mean having more members of the household to provide for, which would be expected to exert a downward influence on wealth. Clearly, statistical data concerning the average cost-
benefit ratio in yams of acquiring additional wives would be needed to decide which of these two conflicting influences was predominant, and no such data are provided by Malinowski.

It is not difficult to find cases in which process tracing is used in tandem with statistical analysis. Justin Lin and Dennis Yang’s (2000) analysis of the Chinese famine of 1959-61 provides an excellent illustration of this point. Inspired by Amartya Sen’s (1981) entitlement approach to famines, they argue that the stage was set for the famine by a combination of a heavy-handed grain procurement regime and an urban-biased rationing system. The famine was then triggered by a decline in agricultural production apparently resulting from the disastrous policies of the Great Leap Forward. A central part of their argument, therefore, is a matter of linking together aspects of the Chinese food distribution system of the period and noting their joint implications. In addition, Lin and Yang use aggregate data to estimate quantitative effects of the decline in food production and the urban-biased redistribution system on mortality rates. This example illustrates an important way in which arguments by process tracing and statistical analysis work together: process tracing is used to establish qualitative claims about causal structure, and statistical analysis is called on to estimate the strengths of these relationships.

Yet the points that establishing a causal conclusion by means of process tracing involves producing a considerable body of evidence and that process tracing, on its own, is only intended to establish qualitative causal conclusions do not fully answer the skeptic’s concerns. In particular, it has not been shown how knowledge of social practices and norms can be acquired in a way that does require a solution to the problem of confounders. The answer to this concern, I suggest, lies in the observation that what one has here are two distinct underdetermination problems; thus, one might be able to resolve one without necessarily having resolved the other.

The problem of underdetermination arises in the context of arguments by means of process tracing but in a different way than when one is attempting to make causal inferences from statistical associations and independencies among variables. In the context of process tracing, the chief difficulty is that of interpreting a social practice. For example, Malinowski (1935) faced the challenge of making an inference about a social practice of which he had no initial inkling from beginning observations of large quantities of yams being moved to and fro. No doubt, multiple possible explanations occurred to Malinowski at the start of this process. Evidence relevant to distin-
guishing between these alternatives would typically consist of observing people’s behavior and asking them about what they are doing and why, as well as what would happen to someone who behaved differently. Thus, Malinowski makes observations about the quantity of yams produced by several apparently typical men, and he makes observations about the quantity that is contributed to the households of sisters. In addition, he questions native informants about the process, relying in part on what-would-happen-if questions, such as, “What would people say if so-and-so did not contribute a significant portion of his crop to the households of his sisters?” Of course, evidence of this sort might fail to unambiguously single out a particular interpretation of the practice in question. Nevertheless, it would be hard to deny that skilled investigators can sometimes reliably acquire knowledge of important social practices by such means.19

In the context of making a causal inference from data concerning statistical associations and independencies among variables, in contrast, the primary challenge arises from the possibility that there are unmeasured common causes that can account for an observed association between the putative cause and effect. Since the two underdetermination problems are distinct, it is possible that there are situations in which one of them is resolved while the other is not. Thus, the fact that Malinowski had no solution to the problem of unmeasured common causes does not entail that he could not have successfully used process tracing to argue for the existence of a social mechanism through which the number of wives exerted a positive influence on wealth among Trobriand chiefs.20

However, the skeptic might doubt that the two cases really are so different. For would not inferences concerning which interpretation is best depend, at least implicitly, on substantive causal generalizations about human psychological and cognitive tendencies? And if that is so, then it seems that the problem of confounders has merely been shunted off to a less noticeable location. Although this is a legitimate concern, I think that it is answerable.

Some of the implicit causal generalizations involved in interpretations of social practices fall into the category of folk psychology—for example, statements such as, “If somebody wants A and he or she

20. Of course, the problem of confounders would have to be confronted if one wished to draw quantitative causal conclusions by means of process tracing in conjunction with statistical data.
thinks that B is the best way to get it, then he or she is more likely to do B.” Such generalizations can be plausibly regarded as a part of one’s background knowledge, thus requiring no special justification. Investigations into matters of what causes what never commence from a position of Cartesian doubt; some background knowledge is always presupposed. Moreover, unlike the presumption that all potential common causes of a given social phenomena have been taken into account, simple generalizations like the one above are plausible and generally unproblematic.

I suspect that simple folk psychological generalizations often suffice for relatively straightforward interpretations of social practices such as that in the Malinowski example. But I agree with Todd Jones (1999, 356-58) that there are interpretations in which less obvious and more precise psychological assumptions would be called for. In such cases, Jones’s proposal is that one should turn to modern cognitive psychology for assistance. Now, the problem of confounders can certainly be expected to arise in research regarding causal generalizations concerning human cognitive capacities. However, here the situation is different from that in social science, since controlled experiments are much more frequently a practical possibility in cognitive psychology than in social science. Consequently, in cognitive psychology the problem of confounders, though real, is less severe.

Having discussed and defended process tracing, let us return to the question of the importance of mechanisms to causal inference in the social sciences. Process tracing does not rely on the premise that causal inference is only possible provided knowledge of underlying mechanisms, which was the assumption that generated Kincaid’s reductio ad absurdum. Hence, in advocating the usefulness of process tracing for causal inference in the social sciences, I do not claim that reliable causal inference in social science would never be possible without mechanisms. Nevertheless, there is a plausible argument that process tracing has a very important role to play in learning the causes of social phenomena.

It is generally not possible to perform experiments to test causal claims that are of interest in social science; moreover, it is rarely the case that all plausible common causes can be measured. As a result, making causal inferences from statistical data with no substantive knowledge of the underlying social processes in question is an extremely difficult—though I would not say impossible—task. Process tracing, to the extent that it provides qualitative information about relevant social mechanisms, can significantly ameliorate this situa-
tion. Beginning with substantive knowledge of the qualitative causal relationships facilitates the process of using statistical data to make causal inferences insofar as it rules out entire classes of alternatives from the start. Moreover, qualitative knowledge about some of the relevant mechanisms can serve an important heuristic purpose in that it directs attention toward causally meaningful statistics. For example, Malinowski’s hypothesis suggests that among the Trobriand islanders, the cost-benefit ratio in yams of acquiring more wives is a worthwhile object of attention. What we have, then, is not an argument that mechanisms are absolutely essential to causal inference in social science. Rather, we have a variety of reasons for thinking that without process tracing, causal inference in social science would be significantly more difficult than it actually is. This conclusion is strong enough to vindicate the intuitive idea that mechanisms are of central importance to learning about cause and effect in the social sciences, while not leading to claim that reliable causal inference in social science is impossible without mechanisms.

CONCLUSION

This article has aimed to clarify the ways in which inquiries into mechanisms are helpful in dealing with one of the most significant challenges to causal inference in the social sciences, the problem of confounders. I proposed that accounts hitherto provided for the usefulness of mechanisms in ameliorating this problem can be interpreted so as to be independent of the proposition that mechanisms are necessary for causal inference, a proposition critiqued by Kincaid. Nevertheless, I argued that even given this more charitable interpretation, the proposal still does not withstand critical scrutiny. The negative side of the argument is undermined by the ease of imagining plausible mechanisms that could link nearly any two aggregate-level social variables. The positive side of the argument is ineffective unless some explanation is provided of how knowledge of mechanisms is to be acquired without a prior solution to the problem of confounders.

Consequently, I developed an account concerning how the positive side of the argument could be improved, which was based on what I called process tracing. Norms or practices present in a society can often be linked together to shed considerable light on mechanisms, knowledge that in turn significantly facilitates efforts to draw causal inferences from statistical data. Moreover, I argued that knowledge of
such norms and practices can be obtained even in circumstances in which not all potential common causes have been taken into account. This account, I hope, helps to elucidate the manner in which mechanisms play an important role in causal inference in the social sciences.

REFERENCES


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