Causality

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Abstract

Making correct causal claims is important for research and practice. This chapter explains what causality is, and how it can be established via experimental design. Because experiments are infeasible in many applied settings, researchers often use "observational" methods to estimate causal models. In these situations it is likely that model estimates are compromised by endogeneity. The chapter discusses the conditions that engender endogeneity and methods that can eliminate it.

On the importance of causal research for practice

The ultimate aim of all research efforts is to develop of theory. A theory is an abstract description of a naturally occurring phenomenon, having independent and dependent variables along with a *causal* explanation of why and under what conditions the variables are related (Bacharach, 1989; Dubin, 1976; Kerlinger, 1986).

Having useful theories is critical for the practice of management. Theories can inform practice in a number of ways: How should organizations be structured? On what individual-

difference characteristics should individuals be hired for a particular position? How should interventions be conducted to best train leadership? Which type of strategy should a company choose when entering a particular market? Should a company internationalize? Organizations need to have informed answers to these questions, whether it is from their managers (who were trained by academics at business schools), consultants, or directly from academics. Thus, the producers of knowledge have a very big responsibility to correctly inform practice by using the best available methods to develop theory.

Unfortunately, as recent reviews have shown, the research that management scholars publish, even in top academic journals, is rife with threats to its validity (Antonakis, Bendahan, Jacquart, & Lalive, 2010; Hamilton & Nickerson, 2003); research that is not rigorously done cannot possibly be relevant to practice (Vermeulen, 2005). In the review that I conduced with my colleagues we found that 90% of published papers had at least three or more threats to validity (I introduce these below). It is thus critical that researchers pay more attention to the problems of causality. I provide a brief overview of causality in this chapter; for more in-depth discussion, readers should refer to more technical treatment on the topic (Antonakis, et al., 2010; Antonakis, Bendahan, Jacquart, & Lalive, in press; Meyer, 1995; Morgan & Winship, 2007; Shadish, Cook, & Campbell, 2002; Winship & Morgan, 1999).

What is causality?

Assume the following model:

$$y = \beta_0 + \beta_1 x + e \qquad \text{Eq. 1}$$

Here, the independent variable *x* is a dummy variable coded 0 (to indicate group_0 and 1 to indicate group_1). We are interested to know whether the groups differ on *y*, as estimated in the coefficient β_1. To establish whether β_1 captures the true causal effect of *x* on *y*, the following three conditions must be met (Holland, 1986; Kenny, 1979):

(1) *x* is a temporal antecedent to *y* (though this is a necessary but not sufficient

condition);

(2) *y* and *x* covary in a statistically significant way (which is straightforward to establish statistically);

(3) the relation between *x* and *y* is not eliminated by other causes (i.e., *x* does not correlate with *e*, which captures all unobserved causes of *y* that are omitted from the model).

Counterfactuals and the experiment

Using an experimental design will ensure that *x* does not correlate with *e*. In the experiment, observations units (*i*) are randomly assigned to a treatment ($group_1$) or a control group ($group_0$). Random assignment ensures that prior to the administration of the treatment that the groups are, on the whole (i.e., the group level) roughly equivalent. Thus, if there is a difference in the group mean levels of *y* after the treatment has been administered, then this difference can only come from one thing: The treatment. It is very unlikely that anything else explains the difference; thus, the *e* term in the model is *random* and truly orthogonal to *x*, and that is what the ordinary least squares regression estimator assumes (which will only occur if the *i*'s have been *randomly* assigned to the groups). Thus, we can observe the counterfactual because the groups were interchangeable at the beginning. A counterfactual is what we would have observed in $group_1$ had it not received the treatment (i.e., $group_0$); alternatively it is what we would have observe in $group_0$ had it received the treatment (i.e., $group_1$).

The problem of endogeneity

In management research, particularly at the macro-level, it is difficult if not impossible for researchers to randomly assign observations units (e.g., divisions, companies) to a treatment or control group. Thus, researchers typically compare groups that differ on a "treatment" (e.g., firms that export versus firms that do not) to see whether group membership predicts an outcome (note, for brevity, I refer to *x* as indicating group membership; however, *x* can also be continuous).

The problem with nonexperimental research is that the groups could differ on other (unmeasured) factors, which could explain variance in the outcome. If these factors are not measured, then *x* will correlate with *e*; *x* is thus said to be *endogenous*, which means that the estimate β_1 will be inconsistent (i.e., it will not converge to the true value with an increasing sample size). If *x* is endogenous and correlates with other predictors in the model, the endogeneity problem in *x* will be "passed-on" to these other predictors too, threating the viability of the entire model.

Threats to validity of tested models

The three causal conditions may seem simple enough to satisfy in experimental settings; however, they are difficult to satisfy in nonexperimental settings. The validity of findings in non-experimental research is, in the most general sense, threatened by *omitted variables*. For example, with respect to attempting to satisfy condition 1 of causality, researchers often measure *x* before *y* by hoping too to eliminate the problem of *common-method variance*--a well-known validity threat in management research when using self-reported measures (Podsakoff, MacKenzie, & Podsakoff, 2012); this problem refers to respondents maintaining consistency across measures and this consistency may inflate or reduce the correlations between variables. A typical remedy researchers use is thus to separate the measurement of *x* and *y*, by measuring *x* before *y*. Yet, if *x* and *y* share a common cause *z* at Time 1 (t1), then y_{t2} will still be caused by *z* because *z* correlates with itself over time (i.e., z_{t1} correlates with z_{t2}). Failing to control for the common cause *z* at Time 2 (i.e., failing to measure and include in the model) will engender endogeneity in *x*; that is, *x* will correlate with *e* (i.e., *z*), the omitted cause of *y*.

Another very common problem in management research is the incorrect estimation of multilevel (Hierarchical Linear Models, HLM) models by *omitting fixed-effects*. Multilevel models are those whereby units are sampled over time (panel data, e.g., *f* firms sampled over

time) or where sample units are nested under a higher level entity (pseudopanel data, e.g., firms sampled in *c* countries). The fixed-effect is in this case the constant unobserved effect of *f* or *c* on the dependent variable (the sample units have something in common due to *f* or *c*). This fixed-effect may correlate with the dependent variable or with the independent variables. Unfortunately, this fixed-effect is usually not modeled by researchers estimating HLM-type models; and adding to the problem is that those who use HLM often use the term "fixed-effect" to refer to the constraint made to the within panel varying coefficients to having an equal effect on within panel dependent variable over time (or over the panel identifier). The problematic constraint that HLM models make, however, is that they constrain the true fixed-effect to be orthogonal to the regressors. This constraint, as tested with a Hausman (1978) test is often rejected. If the fixed-effect correlates with the regressors, and if it is constrained to be orthogonal to them, then this is tantamount to having an omitted variable. Thus, researchers must include dummy variable predictors (of the panel identifier), which precludes including *f* or *c* level predictors, or include the panel means of all within panel regressors along with the *f* or *c* level predictors (Mundlak, 1978).

Another threat to validity is *omitted selection*; here, groups are compared that have self-selected to be treated (e.g., companies chose to export or not). Thus, one can only compare exporters and non-exporters that are roughly equivalent, that is, have the same probability of receiving the treatment. A related threat has to do with unobserved variance in the choice (disturbance of the choice equation) correlating with the disturbance of the dependent variable. Another interesting cause of endogeneity is *simultaneity*, where *x* causes *y* and vice-versa. The simplest case of endogeneity is *measurement error*, which is well known to researchers in management, but which is often ignored. Finally, apart from estimate consistency, researchers should care about consistency of inference, by ensuring that the variance of the estimates is correctly estimated via robust or cluster robust estimation for the

case of heteroscedastic or clustered residuals (Huber, 1967; White, 1980); biased standard errors threaten the validity of parameter tests if they are not i.i.d. (identically and independently distributed).

Causal models in non-experimental research

There are many ways to eliminate the aforementioned threats to validity (see Antonakis, et al., 2010; Cook, Shadish, & Wong, 2008; Shadish & Cook, 2009). Readers should refer to more advanced literature discussing *instrumental variable models*, estimated by two-stage least squares or maximum likelihood (Cameron & Trivedi, 2009), *fixed-effects models* (Wooldridge, 2009), *propensity score models* (D'Agostino, 1998), *selection models* (Heckman, 1979), *regression discontinuity models* (Cook, 2008), and others (Shadish, et al., 2002).

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