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COMMENTARY

A simple solution to a complex problem: Manipulate the mediator!

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Murphy (2021) argues that the interest value of industrial-organizational (I-O) science is being sacrificed in favor of fancy methods and analyses. Dissatisfaction with methodological and analytical complexity is also a theme in survey responses of Society for Industrial and Organizational Psychology (SIOP) scholars (Highhouse & Schmitt, 2013; Highhouse et al., 2020). The *reasons* for this apparent overemphasis on methodology and statistics can serve as a separate commentary, but any explanations should probably include a discussion of scarcity effects on value (i.e., psychometric complexity is overvalued because it is novel), the influence of complex management theory on I-O psychology (i.e., multiple contingencies and levels of contingencies must be modeled), and the behavioral patterns of methodological gatekeepers (see Whitaker & Guest, 2020). The focus of our commentary, however, is narrower: testing mediation.

Mediation and causation

Murphy (2021) notes that tests of mediation are notoriously difficult to interpret. He provides many reasons for this and discusses ways to avoid misinterpreting what Spencer et al. (2005) call measurement-of-mediation designs. Implicit in Murphy's discussion of mediation is that in the vast majority of I-O studies of mediation, the relation between the mediating and dependent variable is examined via passive observation (Stone-Romero & Rosopa, 2008). Mediation, however, implies causation. In simple mediation models, the independent variable causes the mediator ($X \rightarrow M$) and the mediator causes the dependent variable ($M \rightarrow Y$). Shadish et al. (2002) noted that there are three requirements for inferring causality. We list these as they apply specifically to mediation: (a) M precedes Y in time, (b), M and Y vary together, and (c) there are no plausible alternative explanations for the relationship between M and Y. Only randomized experiments satisfy all three requirements.

Therefore, measurement-of-mediation designs are vulnerable to misinterpretation. To believe the results of studies using nonexperimental designs, the reader must assume that the researcher has correctly specified the causal ordering of variables and has controlled all important confounds through design features or statistical means (Stone-Romero & Rosopa, 2008). The assumption that no omitted variables influence the $X \rightarrow M \rightarrow Y$ causal chain is referred to as the *no omitted variable assumption* or the *no confounding assumption*. (Pirlott & MacKinnon, 2016). This assumption is rarely justified and often violated. One way to provide evidence for the assumption is to follow up a study that used measurement of mediation with one that demonstrates the $X \rightarrow M \rightarrow Y$ relationship experimentally (Podsakoff & Podsakoff, 2019).

Manipulate the mediator

In the field of social psychology, attention to the overreliance on measurement-of-mediation designs led Spencer et al. (2005) to call for researchers to test mediation by establishing a causal chain through experimentation. This involves doing two sequential studies: in study 1, X is manipulated and M and X are measured, and in study 2, M is manipulated and X is measured. Pirlott and MacKinnon (2016) distinguished between directly manipulating the presence or absence of the mediator versus encouraging or discouraging the mediator in manipulation-of-mediator designs. We discuss the direct approach first.

An example of *directly manipulating the mediator* is a study by Meyer and Gellatly (1988). They examined the mediating effect of perceived performance norms on the relationship between assigned goals and performance. In the first experiment, the authors manipulated assigned goal (X; easy, difficult, impossible) and measured its effect on perceived performance norms (M) and performance (Y). In the second study, the researchers experimentally manipulated performance norms (M; low, high, no norm) to examine their effect on performance (Y). More recently, Nolan and Highhouse (2014) examined the mediating effect of perceived autonomy on the relationship between standardized hiring practices and user resistance. In their first experiment, the authors manipulated type of hiring procedure (X; highly standardized, unstandardized) to examine the effects on perceived autonomy (M) and user resistance (Y). In the second experiment, the researchers manipulated the amount of autonomy (M) in the standardized hiring procedure to examine its effect on user resistance (Y).[1]

Imai et al. (2013) point out that directly manipulating the mediator is not always possible and that even when it is, the researcher must make a strong argument that the measured and manipulated versions of the mediator are consistent (i.e., they are the same construct and would have the same effect on the outcome). In these situations, they recommend employing "designs with imperfect manipulation" (p. 18), referred to as *encouragement designs.* These involve manipulations that are directed at increasing (encouraging) or decreasing (discouraging) the value of the mediator. Pirlott and MacKinnon (2016) point to a study by Li et al. (2012), who manipulated their mediator "belief in a soul" by randomly assigning people to either write an essay suggesting that souls do exist (encouragement condition) or do not exist (discouragement condition). This approach involves strengthening or weakening the mediator, rather than fully changing the mediator.

Although manipulating the mediator is surely an improvement in terms of our ability to identify a causal mechanism, some experimentalists suggest that even establishing a causal chain falls short of demonstrating how an independent variable affects an outcome through the mediator (e.g., Imai, et al., 2013). These authors suggest that *parallel* and *crossover* designs are better equipped to identify causal mechanisms.

In an experimental mediation using a *parallel design*, a sample is randomly split in two and two randomized studies are conducted in parallel: one study manipulates X and measures M and Y, and the other study simultaneously manipulates X and M, and measures Y. This is essentially a combination of a measurement-of-mediator design and a manipulation-of-mediator design. In a specific version of a *crossover design* suited to testing mediation, participants are exposed to both conditions of the independent variable in randomized order. In each condition, at stage 1, the M and Y are measured. Then, at stage 2 participants are assigned to the opposite condition *and* they are assigned to the value of M from the first stage—and Y is measured. An important assumption of crossover designs is no carryover effects. Both parallel and crossover designs can be used with direct or encouragement manipulations.

[1]Podsakoff & Podsakoff (2019) provide other published examples of manipulating mediators.

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Anticipated objections to manipulating the mediator

Objection 1: My model is too complex

This may be true. The approaches addressed in this commentary are largely limited to investigations of only one mediator. More complex models are not amenable to experimentation in the way we have described. We would suggest, though, that before testing complex models with multiple mediators, one should start with rigorous tests of simpler mediation relationships that can establish a solid base on which complex iterations can be built.

Objection 2: Most organizational constructs cannot be manipulated

We believe that many of the organizational constructs that researchers believe are not amenable to manipulation can, with a little creatively, be manipulated. If religion researchers can manipulate belief in a soul, then surely organizational researchers can manipulate things like burnout, turnover intentions, and job ambiguity (see Breaugh & Colihan, 1994; Podsakoff & Podsakoff, 2019). *Encouragement* designs can allow us to use experimental designs when direct manipulation is difficult. However, we recognize that it is not always possible, either practically or ethically, to manipulate mediators.

Objection 3: I can't statistically estimate the indirect effect of the mediator

Podsakoff and Podsakoff (2019) noted that, using these designs, it is not possible to estimate statistically the indirect effect of the mediator or to calculate how much of the effect of X on Y can be attributed to M. This is a limitation of relying strictly on a manipulation-of-mediator approach. Despite not being able to statistically estimate the indirect effect of the mediator, we argue that creative experimental design not only allows stronger establishment of causality but also provides more nuanced information about the relationships among the variables.

Final thoughts

Although there are a number of limitations of experimental approaches that manipulate the mediator, many of these can be addressed by including within the same research program a complementary study using the measurement-of-mediation design. Both approaches to establishing a causal chain may be used to offset the limitations of each. Like Podsakoff and Podsakoff (2019), we believe that the benefits of experimental approaches to mediation outweigh the limitations. Researchers should consider this underused approach as a simple tool that can provide powerful evidence. Like Murphy (2021), we encourage I-O researchers to go back to the basics and consider whether statistically simpler solutions may be appropriate.

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