

Reflections on Mediation

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This commentary reviews the author's history with mediation. In the second part, the author attempts to explain why there is such interest in the topic. In the third part, the author comments on the five articles in this feature topic.

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In this commentary, I am going to first review my history with mediation. In the second part, I try to explain why there is such interest in the topic. Then in the third part, I comment briefly on the five most interesting articles in this feature topic.

My History

Almost certainly my first exposure to mediation came in a lecture from Tom Cook in 1969 in a class on research design. I vaguely remember Cook discussing Hyman (1955). I presume Cook gave Hyman's three conditions for mediation (he actually called it "elaboration"). These conditions roughly, though not exactly, parallel the conditions given in my subsequent articles. I certainly do not remember being all that excited or all that interested in the topic.

My next encounter with mediation came 10 years later in my book *Correlation and Causality* (Kenny, 1979), where I did call it "mediation." I probably could not have treated the topic in a more boring fashion, as I largely focused on the decomposition of a raw correlation into direct effects, mediated effects, and spuriousness. I did discuss the mediation of effects from randomized experiments, but I suspect those ideas grew out of conversations that I was having with Susan Fiske, with whom I eventually published an article on the topic (Fiske, Kenny, & Taylor, 1982).

My first serious attempt to write about mediation and explain it in a detailed way came in the book *Estimating the Effects of Social Interventions*, written with Charles Judd (Judd & Kenny, 1981a). The book discusses both mediation and moderation. (I was surprised to see we had a footnote saying we do not like the term *moderation* and prefer the term *interaction*.) It gives the four famous steps in a very condensed format. Actually, it gives the first three steps, and then after that, it mentions the fourth step. I am not entirely sure from where those steps came. Perhaps they were from the Cook lecture more than 10 years earlier, perhaps they were Judd's, but I think that they were primarily my idea.

Judd was quite disappointed that I had not provided very much detail of the mediation strategy. He kept calling me "Mr. Cryptic," no doubt a thought experienced by many readers of my articles. When he was asked to give a talk at a meeting in Berlin around 1980, he decided to expand on the mediation and moderation section in the book and

provide an example. He and I were both working with a graduate student, Michael Milburn, at that time, and quite clearly, the analyses that we had developed for his work figured heavily into the article. That conference paper became the *Evaluation Review* Judd and Kenny (1981b) article, which was largely written by Judd. That article has extensive detail on the four steps. It was Judd's idea to combine mediation and moderation in a single analysis, an idea he continues to promote in this feature topic.

While all this was going on, I had moved to the University of Connecticut. There, in 1978, I met a graduate student Stephen Needel, who was familiar with my work on mediation, exactly how I no longer remember. (Of note, Needel was working with a skilled young undergraduate named John Matthieu.) Needel was keenly interested in taking those ideas and presenting them to social psychologists. Quite honestly, at the time, I had no interest in this project. Despite this, Needel and his major advisor, Reuben Baron, started to write an article on mediation and moderation, and I became a very reluctant third author. Needel eventually dropped out of the project and became a market researcher. Baron continued work on the project, and my role was to add material on the statistical analysis. We first sent the article to *American Psychologist*, where it was rejected because reviewers felt what we were saying was known to everyone. We got similar reviews and a rejection when we first submitted the article to the *Journal of Personality and Social Psychology*. Harry Reis later became editor of that journal, we resubmitted the article, telling him that the article had been previously rejected but we wanted another chance. Reis really liked the article, and against the advice of some of his reviewers, he published the article (Baron & Kenny, 1986). History has certainly vindicated him as the article has become the most cited article ever in the journal (Quinones-Vidal, Lopez-García, Penaranda-Ortega, & Tortosa-Gil, 2004).

I had pretty much forgotten about the article after that for more than 10 years. For instance, as late as 1997, my student Cynthia Mohr had to reteach me the four steps so that I could use them in a workshop in Switzerland. For reasons described in the next section, in the last few years, I finally figured out why mediation is such an important topic. Since then, I have devoted a considerable amount of my attention to this topic, by writing articles, creating a Web site (<http://davidakenny.net/cm/mediate.htm>), and answering e-mails.

Why All the Interest in Mediation?

From reading the above, it is obvious that for most of my career, I have felt that mediation is not all that interesting a topic. I was obviously wrong. Fortunately, I had coauthors like Baron, Fiske, and Judd and students like Needel, Milburn, and Mohr who appreciated the importance of the topic and involved me in this effort.

Mediation is important because it allows us to conduct scientific investigations; that is, the interesting part of science is to explain how something comes about. A mediational analysis provides the researcher with a story about a sequence of effects that leads to something. When you examine a structural equation model, the interesting part of the model (technically, the part of a structural, as opposed to a measurement, model) that is overidentified almost always is mediation. Suffice it to say, mediation asks a fundamental question in science.

That said, why is the Baron and Kenny (1986) article so popular? After all, there were many good sources on the topic, most notably James and Brett (1984), who were published before it. I think there are both good and bad reasons why that article is so popular. On the good side, it provided clear definitions of both mediation and moderation. It also provided clear and explicit advice on how to conduct a mediational analysis. On the bad side, Baron and Kenny (1986) are much too formulaic. The article seems to imply that if a series of regression equations are estimated, the researcher has a definitive answer about mediation. What is often ignored in mediational analyses is that there is a causal or structural model and the parameters of that model estimate the mediation. Both Judd and Kenny (1981b) and Baron and Kenny (1986) explicitly state the causal assumptions that underlie a mediational analysis. Unfortunately, most published mediational analyses do not explicitly acknowledge these assumptions. If the mediational model is wrong (i.e., misspecified), the results from a mediational analysis are not so much meaningless, but rather they are misleading.

Commentary on the Articles in This Feature Topic

I begin with the article by Muller, Yzerbyt, and Judd (2008) because as I have said, I have been and continue to be a student of Judd. The reader needs to realize that this article builds on Judd and Kenny (1981b) and not the more familiar work by James and Brett (1984) and Baron and Kenny (1986). In that approach, the mediating and moderating effects of a variable are simultaneously estimated. It should be noted that Kraemer, Wilson, Fairburn, and Agras (2002) have argued that a variable can be either a mediator or moderator but not both. Muller et al. (2008) and I take a very different view. I have one other point about Muller et al. They have assumed that X is a dichotomy and effects coded (1, -1). If these conditions do not hold, the interpretation of the coefficients would need to be modified.

The second article I comment on is by Taylor, MacKinnon, and Tein (2008). It is fitting that David MacKinnon is one of the authors in this feature topic because he, more than anyone else, has worked out many of the messy details in the estimation and testing of mediational hypotheses. (One interesting fact is that one of MacKinnon's undergraduate instructors was Charles Judd.) Consistent with work on regular or two-path mediators, the bootstrapping method provides the most powerful test of the indirect effect.

The Wood, Goodman, Beckmann, and Cook (2008) article reviews tests of mediation and provides reasonable guidelines for future research. It raises a key consideration of whether the researcher should specify complete or partial mediation. Of historical interest, Judd and Kenny (1981b), as well as the initial version of Baron and Kenny (1986), emphasized complete mediation. The editor and reviewers strongly suggested that we not insist on the fourth step for mediation, a request to which we agreed.

It can be argued that if one believes in complete mediation, then the third step in Baron and Kenny should be modified because it is unnecessary to control for the causal variable when estimating the effect of the mediator on the outcome. This is the position of James and Brett (1984). I would still follow the advice of Baron and Kenny and estimate that

path. Why estimate a path which you think will be zero? First, you estimate it to determine that in fact it is zero. Second, you might be wrong and you have partial mediation, and by estimating, the effect the path from the mediator to the outcome would be unbiased.

The article by Cheung and Lau (2008) provides a useful extension of mediation analysis by conducting a latent variable analysis. I wish to make two technical points. First, the Sobel (1982) and related tests presume that the two effects (commonly denoted as a and b) are independent. In fact, when multiple regression is used, they are independent. However, in other instances (e.g., logistic regression, latent variable models, and multilevel modeling), the two tests are not independent. The effect of this nonindependence is likely to be small, but it does require investigation. Second, for latent variable models, the total effect estimated from a model without the mediator is usually not comparable to the direct effect estimated from a model in which the mediator is included. The reason is that the measurement model (i.e., the factor loadings) is different in the two models, and so the unit of measurement of the latent variables is not exactly the same. For this reason, it would seem preferable to estimate the total effect by computing for the complete model by summing the direct and indirect effects.

The final article in this commentary is Stone-Romero and Roposa (2008), which is by far the most controversial article in this issue. There is one major point of their article with which I strongly agree. Mediation analysis is a form of causal analysis. As I have noted earlier in this commentary, all too often persons conducting mediational analysis either do not realize that they are conducting causal analyses or they fail to justify the assumptions that they have made in their casual model. Those assumptions discussed by Stone-Romero and Roposa (2008) are discussed in Baron and Kenny (1986) and extensively detailed in Judd and Kenny (1981b).

However, I strongly disagree with Stone-Romero and Roposa (2008) that randomized experiments are the only legitimate design for drawing causal conclusions. I disagree for several reasons. First, the requirement of manipulations precludes the study of many variables where manipulation is either not ethically or practically possible. If we can only study variables that we manipulate, we have a science that artificially limits itself from studying as causes variables that cannot be manipulated. One of the key points in Baron and Kenny (1986) is that very often mediators are internal, psychological, process variables that can only be measured and not manipulated. Second, internal validity is indeed weaker for nonrandomized studies, but it can be enhanced in several different ways that have been discussed by myself and others. Third, modern theories of causality, most notably that of Pearl (2000), argue that valid causal inferences can be made by laypersons, by structural equation modelers, as well as by experimentalists. Third, the two-experiment approach advocated by Stone-Romero and Roposa (2008), as well as others (Spencer, Zanna, & Fong, 2005), misses the point that the mediator is a different variable in those two studies. In one case, it is a manipulated variable and the other case it is a measured variable. There can be serious construct validity issues. Instead of the two-experiment strategy, researchers would be better advised to use the innovative design suggested by Smith (1982) in which all three mediation variables are manipulated and measured. Fourth, the randomized experiment is discussed as if it were infallible. Randomized studies, especially those conducted outside of the laboratory, are rarely perfectly implemented. Missing

data, cross-contamination, and poor treatment fidelity are very common. No research design is perfect.

Final Comment

I would have never guessed that mediational analyses would become so common that a sizeable percentage of articles in the social sciences employ some form of mediational analysis. Investigators are keenly interested in testing mediational hypotheses, and the articles in this feature topic should help us to advance our understanding of this most important topic. A common theme in all five articles is that mediation is not a thoughtless routine exercise that can be reduced down to a series of steps. Rather, it requires a detailed knowledge of the process under investigation and a careful and thoughtful analysis of data.

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