

# Commentary: Mediation and Moderation: An Historical Progress Report

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The purpose of this invited commentary is to reflect on the progress we have made in the use of *mediation* and *moderation* in our research since the appearance of my paper on post hoc probing of mediator and moderator effects in pediatric psychology research (Holmbeck, 2002). As I did in my first paper on this topic (Holmbeck, 1997), I continue to distinguish between conceptual and statistical clarity when discussing mediation and moderation. Indeed, both types of clarity are critical and both are necessary; it is not sufficient to have one without the other.

My interest in mediation and moderation was stimulated initially by early papers that employed path analysis and related statistical modeling. But it was the appearance of Baron and Kenny's (1986) classic paper on the topic that really peaked my interest (this article has been cited more than 80,000 times according to Google Scholar). In the years immediately following the appearance of their work, I read with great interest papers that purported to evaluate the utility of models that included tests of mediation or moderation or both. Although researchers cited Baron and Kenny, there was often something amiss. Usually there was a mismatch between text (e.g., variable M was expected to buffer the effect of X on Y), figures (e.g., the model including X, M, and Y was drawn as a mediational model instead of a moderator model), and/or the statistics used to analyze the data (e.g., methods described in Baron and Kenny or in other related papers were not used and, most often, neither moderation nor mediation was tested). In other words, my initial interest in this area was focused on the degree to which a particular piece of research was presented with a high level of clarity, consistency across components of the paper, and coherence (Holmbeck, 1997).

Independently, and because I am not a biostatistician, I grew to appreciate papers, books, or web sites

that explained a statistical method so clearly that someone without a degree in statistics could understand it. My guess was that a very small percentage of those in our field could read, with understanding, a paper in *Psychological Methods*. Given the cursory discussion of statistical methods in Baron and Kenny's (1986) paper, I believed that it might be helpful to provide a *How to* paper on strategies for testing mediation and moderation in pediatric psychology research studies (and, fortunately, Bob Noll, the guest editor of a special issue on methodology and research design, agreed; Noll, 2002). Thus, I submitted a paper on the topic to the *Journal of Pediatric Psychology* (JPP; Holmbeck, 2002).

Where are we now? There has been a great increase in the number and level of sophistication of statistical methods available; these methods allow us to pose and answer questions that weren't even considered 20 years ago. Although I do not have the space needed to review in detail this expansion across the areas of mediation and moderation, it is clear that we have moved well beyond what Baron and Kenny discussed in 1986 and what I presented in 1997 and 2002 (e.g., see Hayes, 2018; MacKinnon, 2008; Pearl, 2012; VanderWeele, 2016). For example, we have learned that: (1) mediational models are best tested with longitudinal data, (2) outcomes in such models can be growth curves or categorical variables in addition to static continuous variables, (3) we can have multiple moderators and/or mediators in the same model, and (4) we have methods for testing mediated-moderation and moderated-mediation (Karazsia, Berlin, Armstrong, Janicke, & Darling, 2014) and methods to include a variable as both a mediator and a moderator, but at different time points within the same model (Karazsia & Berlin, 2018). Moreover, Hayes (2018) has provided us with a very useful data analytic tool

(PROCESS)—a macro that can be run with the Statistical Package for the Social Sciences (SPSS) or Statistical Analysis Software (SAS). Importantly, it used to be (in the 1980s and 1990s) that almost every published paper that purported to examine some type of mediational or moderational process was flawed in some serious way; this is no longer true, especially with regard to the level of statistical sophistication as well as the consistency between what the researchers say they will do and what they actually do.

On the other hand, we still have work to do—and most of this, in my view, is conceptual. We can use the most sophisticated statistics available and provide a very impressive presentation of the findings, but the research will not be a significant contribution if the conceptual piece is not addressed with care. Simply put, the model being tested must *make sense*. With respect to this issue, the conceptual problems that were present in the late 1980s and early 1990s are still present in the literature today. There is no better place to look for such conceptual difficulties than in research focused on mediation and moderation in the area of coping. As I noted in my 1997 paper (Holmbeck, 1997), mediational models are often (and unfortunately) tested based on a temporal, rather than a causal, understanding of the relations between the variables in the model. For example, one might posit a mediational model where a stressor occurs, a person copes with the stressor, and then an outcome occurs after these coping attempts. Although this may be how these events unfold over time (i.e., stressor, followed by coping, followed by outcome), this is *not* necessarily an appropriate basis for a mediational model. Regardless of whether or not a researcher has data to support a causal model, the theory that underlies a mediational model is inherently *causal*. For a mediational model to *make sense*, one should be able to hypothesize that the independent variable (IV) *causes* change in the mediator which, in turn, *causes* change in the dependent variable (DV). When approaching mediation in this way, it will usually not make sense to hypothesize that a stressor *causes* changes in how one copes which, in turn, *causes* changes in some distal outcome. It probably makes more sense to hypothesize that coping will moderate an IV→DV association such that certain coping strategies will either buffer or exacerbate an association between a stressor and an outcome or that the IV→DV association will only be significant when a particular coping strategy is used more frequently.

Recently, Marsac et al. (2017) hypothesized that coping and appraisals could serve as mediators of associations between early and later posttraumatic stress symptoms. This is a rare case in which the researchers provided a compelling case for the impact of the IV on hypothesized coping-related mediators. Specifically,

they were interested in whether or not “PTSS (posttraumatic stress symptoms) result in more coping attempts or a different type of coping” (p. 789). It would also be interesting if these authors tested the viability of the competing hypothesis that these coping- and appraisal-related variables function as moderators (as I described above). Similarly, there may have been a missed opportunity in studies by Langford et al. (2017) and Himmelstein, Puhl, and Quinn (2018) who test coping only as a mediator; it would be interesting to see the results when coping is tested as a moderator.

Another issue that has arisen in this literature is whether or not there needs to be a significant association between the IV and the DV for one to claim mediation (Hayes, 2018). Although, statistically speaking, it makes sense that only the indirect effect needs to be significant for one to claim that mediation has occurred, this may not make sense conceptually. For example, if one finds a significant mediator (e.g., negative rumination) within the context of a randomized controlled trial (RCT) focused on a treatment for depression in youth with type 1 diabetes, this finding would indicate that reductions in the level of negative ruminations may be a pathway through which the treatment has an impact on future levels of depressive symptoms. I would argue that, in this case, the finding would have even more weight if the treatment condition (i.e., the IV) was also related to the outcome (i.e., the DV), since it would be desirable for there to be a treatment effect on the outcome of interest. In other words, if the treatment is not associated with a decrease in depressive symptoms, one would probably be less interested in possible mediators of this nonsignificant treatment effect (see Goldsmith et al., 2018; Kraemer, Wilson, Fairburn, & Agras, 2002, for more discussion of mediation and moderation in the context of RCTs).

In this brief commentary, I have attempted to review where we have come since I published my paper on mediation and moderation in *JPP* in 2002. Clearly, there have been many exciting statistical advances that make it possible to test new models that will advance our knowledge. But I have also provided some cautionary remarks regarding the importance of assessing the degree to which our models “make sense” from a conceptual perspective. I recommend that researchers attempt to explain their models by imagining hypothetical children and *walking* them through the model. For example, if children experience serious stressors, these children are likely to experience negative outcomes of some sort, particularly if they cope in a way that makes the outcome even more likely (moderation). It may not make sense to argue that the stressor alters the manner in which the children cope which then exacerbates the level of the outcome (mediation). To prevent conceptual lapses, we need more papers

that focus on the conceptual components of our models, so as to clarify the underlying mechanisms or pathways that explain how or why our variables of interest will be interrelated in the manner hypothesized. Indeed, there are far more papers that approach mediation and moderation from a statistical perspective than from a conceptual perspective. In the future, I look forward to seeing more contributions arising from the conceptual side of the equation.

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