

Beyond Baron and Kenny: Statistical Mediation Analysis in the New Millennium

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Understanding communication processes is the goal of most communication researchers. Rarely are we satisfied merely ascertaining whether messages have an effect on some outcome of focus in a specific context. Instead, we seek to understand *how* such effects come to be. What kinds of causal sequences does exposure to a message initiate? What are the causal pathways through which a message exerts its effect? And what role does communication play in the transmission of the effects of other variables over time and space? Numerous communication models attempt to describe the mechanism through which messages or other communication-related variables transmit their effects or intervene between two other variables in a causal model. The communication literature is replete with tests of such models.

Over the years, methods used to test such process models have grown in sophistication. An example includes the rise of structural equation modeling (SEM), which allows investigators to examine how well a process model that links some focal variable *X* to some outcome *Y* through one or more intervening pathways fits the observed data. Yet frequently, the analytical choices communication researchers make when testing intervening variables models are out of step with advances made in the statistical methods literature. My goal here is to update the field on some of these new advances. While at it, I challenge some conventional wisdom and nudge the field toward a more modern way of thinking about the analysis of intervening variable effects.

Total, Direct, and Indirect Effects

In an intervening variable model, variable *X* is postulated to exert an effect on an outcome variable *Y* through one or more intervening variables, sometimes called *mediators*. Given a sample of data, *X*'s *total effect* on *Y*, denoted in Figure 1A as *c*, can be represented in a number of ways, such as an OLS regression coefficient in

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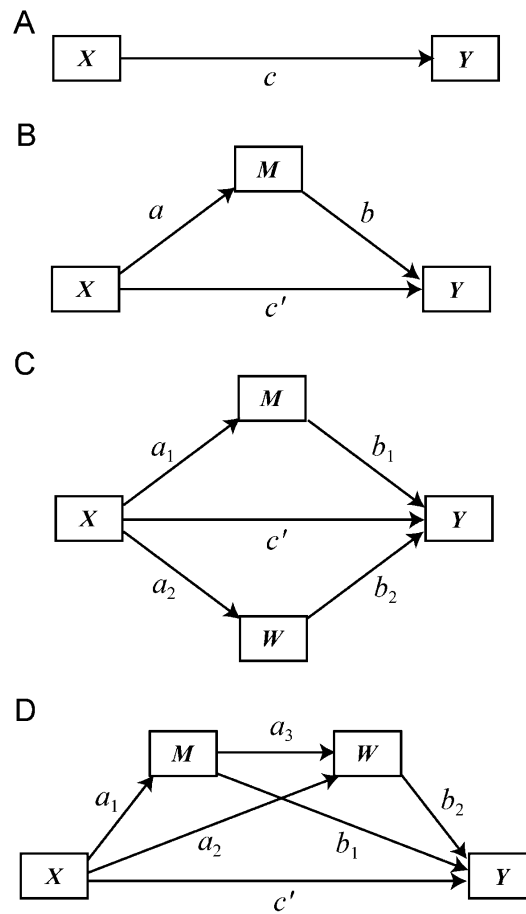


Figure 1 The total effect of X on Y (A), a simple mediation model (B), a single-step multiple mediator model (C), and a multiple-step multiple mediator model (D).

standardized or unstandardized form, or as a path coefficient from a maximum likelihood-based method such as structural equation modeling. This total effect, interpreted as the expected amount by which two cases that differ by one unit on X are expected to differ on Y , may come to be through a variety of forces both *direct* and *indirect*. Figure 1B, C, and D represent a number of possibilities, although of course there are many others one could imagine.

Figure 1B is the simplest of all intervening variable models, the *simple mediation model*. In this model, a is the coefficient for X in a model predicting M from X , and b and c' are the coefficients in a model predicting Y from both M and X , respectively. In the language of path analysis, c' quantifies the *direct effect* of X , whereas the product of a and b quantifies the *indirect effect* of X on Y through M .¹ If all three variables are observed, then $c = c' + ab$ (in latent variable models or models of dichotomous outcomes, this will not always be true). Simple algebra shows that the indirect effect, ab , is just the difference between the total and direct effect of X : $ab = c - c'$. The indirect effect is interpreted as the amount by which two cases who differ by one unit on X are expected to differ on Y through X 's effect on M , which in turn affects Y . The direct effect is interpreted as the part of the effect of X on Y that is independent of the pathway through M .

In more complex models, as in Figure 1C or D, the same rules apply. In Figure 1C, the total effect is equal to the direct effect of X on Y plus the sum of the indirect effect through M and the indirect effect through W . That is, $c = c' + a_1b_1 + a_2b_2$. In a model with two or more intervening variables, the indirect effect through a given intervening variable is called a *specific indirect effect* (e.g., the specific indirect effect of X on Y through M), and the sum of the specific indirect effects is called the *total indirect effect* of X . In Figure 1D, the total effect of X on Y can similarly be partitioned into direct and indirect components. Here, $c = c' + a_1b_1 + a_2b_2 + a_1a_3b_2$, with the latter three terms being specific indirect effects and their sum being the total indirect effect (see Brown, 1997).

The Causal Steps Approach to Testing Intervening Variable Effects

Models such as these are frequently estimated by communication researchers. Although there are many methods available for testing hypotheses about intervening variable effects, the most widely-used method is the *causal steps approach* popularized by Baron and Kenny (1986). This approach requires the researcher to estimate each of the paths in the model and then ascertain whether a variable functions as a mediator by seeing if certain statistical criteria are met. For example, if both a and b paths in a model such as Figure 1B are statistically significant and c' is closer to zero than c , then M is deemed a mediator of the relationship between X and Y . Some assess whether one's data meet these criteria only if there is evidence of a total effect of X (i.e., if c is statistically significant), one of the requirements of mediation outlined by Baron and Kenny. If the significance of c is not used as a prerequisite to further examination of the paths, then this causal steps approach is sometimes called a test of *joint significance*.

Unbeknownst to many, the causal steps approach has been criticized heavily on multiple grounds. Most notably, simulation studies have shown that among the methods for testing intervening variable effects, the causal steps approach is among the lowest in power (Fritz & MacKinnon, 2007; MacKinnon, Lockwood, Hoffman, West, & Sheets, 2002). That is, if X 's effect on Y is carried in part indirectly through intervening variable M , the causal steps approach is least likely of the many methods available to actually detect that effect. Another criticism of this approach is that it is not based on a quantification of the very thing it is attempting to test—the intervening effect. Rather, the existence of an indirect effect is inferred logically by the outcome of a set of hypothesis tests. If a and b are both different from zero by a statistical significance criterion, then so too must be the indirect effect according to the logic of this approach. But it is traditional in social science to base claims on tests of quantities pertinent to those claims. We infer the existence of partial association, differences between experimental conditions, and so forth by quantifying these effects and then testing hypotheses about or constructing interval estimates for their size. Given that an indirect effect is quantified as the product of its constituent paths, should we not base inferences about indirect effects on tests of the product? Additionally, it is possible for an indirect effect to be detectably different from zero

even though one of its constituent paths is not. Hypothesis tests are fallible. Each carries with it a possibility of a decision error. The more nulls that must be rejected in order to claim an indirect effect, the more likely the analyst will go away empty handed. It makes more sense to minimize the number of tests one must conduct to support a claim.

Given these problems with the causal steps approach, why is it so widely used? The most plausible explanation is that it is simple and widely understood. Most anyone can be taught this approach, its implementation described in only a few manuscript lines, and readers and reviewers will be able to follow along without difficulty. Yet these are not convincing reasons for the use of a method that is not optimal when there are better alternatives.

Modernizing Our Thinking About and Testing of Indirect Influences

Modern Approaches to Inference about Intervening Variable Effects

New analytical opportunities arise if we quantify indirect effects rather than infer their existence from a set of tests on their constituent paths. One inferential technique is the product of coefficients approach, most well known as the Sobel test (Sobel, 1982, 1986). This test requires an estimate of the standard error of ab (see Preacher & Hayes, 2004, for three estimators of the standard error). The ratio of ab to its standard error is used as a test statistic for testing the null hypothesis that the “true” indirect effect is zero, with the p -value derived from the standard normal distribution.

Although the Sobel test enjoys some use, frequently it is used as a supplement to the Baron and Kenny approach rather than instead of it. An investigator may first ask whether the analysis meets the Baron and Kenny criteria for establishing mediation and, if so, the analyst then conducts the Sobel test to attest to the validity of the conclusions reached without it. There is little point to this exercise. The outcome of a set of hypothesis tests about a and b is irrelevant and provides no additional information beyond the Sobel test about the size or significance of the indirect effect. Thus, one should not precondition the use of the Sobel test on significant paths linking X to M or M to Y .

The Sobel test has a major flaw. It requires the assumption that the sampling distribution of the indirect effect is normal. But the sampling distribution of ab tends to be asymmetric, with nonzero skewness and kurtosis (Bollen & Stine, 1990; Stone & Sobel, 1990). We should not be using tests that assume normality of the sampling distribution when competing tests are available that do not make this assumption and that are known to be more powerful than the Sobel test. Of the alternatives, two seem to be winning the battle: bootstrapping, and the empirical M -test. Simulation research shows that these methods tend to have highest power and the best Type I error control. Although the empirical M -test (also known as the *distribution of products* approach) is advocated by Holbert and Stephenson (2003) as “the best option available to media effects scholars” (p. 566), it suffers the major weakness that

it is somewhat cumbersome to conduct without the assistance of tables (although MacKinnon, Fritz, Williams, & Lockwood, 2007, offer an algorithm to reduce some of the computational burden) and it requires additional assumptions that bootstrapping does not. In contrast, bootstrapping is already implemented in some SEM software (most extensively in Mplus; EQS and AMOS to a lesser extent) and routines are available that allow users of other popular programs such as SPSS, SAS, and R to bootstrap indirect effects (see, e.g., MacKinnon, 2008; Preacher & Hayes, 2004, 2008a; Shrout & Bolger, 2002). So I focus here on bootstrapping as the better of the two options.² Discussions of bootstrapping indirect effects have been in the literature since the 1990s (e.g., Bollen & Stine, 1990; Lockwood & MacKinnon, 1997), but the method has started to catch on only recently. There are actually many different bootstrap-based methods that are available for testing hypotheses about intervening variable effects (see MacKinnon, Lockwood, & Williams, 2004). I focus only on the basic concepts they share below.

Bootstrapping generates an empirical representation of the sampling distribution of the indirect effect by treating the obtained sample of size n as a representation of the population in miniature, one that is repeatedly *resampled* during analysis as a means of mimicking the original sampling process. The resampling of the sample is conducted *with replacement*, so that a new sample of size n is built by sampling cases from the original sample but allowing any case once drawn to be thrown back to be redrawn as the resample of size n is constructed. Once a resample is constructed, a and b are estimated this resampled data set and the product of the path coefficients recorded. This process is repeated for a total of k times, where k is some large number (typically at least 1000, although I recommend at least 5000). Upon completion, the analyst will have k estimates of the indirect effect, the distribution of which functions as an empirical approximation of the sampling distribution of the indirect effect when taking a sample of size n from the original population. An inference is made about the size of the indirect effect in the population sampled by using the k estimates to generate a $ci\%$ confidence interval. This is accomplished by sorting the k values of ab from smallest to largest. In this ordered set, the lower bound of a $ci\%$ confidence interval is defined as the value of ab in the $k(.5 - ci/200)$ th ordinal position of the ordered list (e.g., the 25th position if $k = 1000$ for a 95% confidence interval), and the upper bound is the value in the $1 + k(.5 + ci/200)$ th ordinal position (e.g., the 976th position if $k = 1000$ for a 95% confidence interval). This procedure yields a *percentile-based* bootstrap confidence interval. The endpoints can be adjusted to yield a *bias corrected* or a *bias-corrected and accelerated* confidence interval. Regardless of which is used, if zero is not between the lower and upper bound, then the analyst can claim that the indirect effect is not zero with $ci\%$ confidence. This is conceptually the same as rejecting the null hypothesis that the true indirect effect is zero at the $100 - ci\%$ level of significance.

Simulation research shows that bootstrapping is one of the more valid and powerful methods for testing intervening variable effects (MacKinnon et al., 2004; Williams & MacKinnon, 2008) and, for this reason alone, it should be the method of choice. One of the beauties of bootstrapping is that the inference is based on an

estimate of the indirect effect itself, but unlike the Sobel test, it makes no assumptions about the shape of the sampling distribution of the indirect effect, thereby getting around this problem that plagues the Sobel test. Additionally, notice that no standard error is needed to make the inference, rendering the controversy about how to best estimate the standard error of the indirect effect moot. Finally, it is a very general approach, in that it can be used for making inferences about indirect effects in *any* intervening variable model, regardless of how complex and how numerous the paths between X and Y .

Bootstrapping is being used with increasing frequency, although like the Sobel test, it is sometimes reported as a supplement to the causal steps approach rather than instead of it. I see no reason to report the results of both methods, although little harm is done if inferences are based on the bootstrap results.

Can Effects that Don't Exist be "Mediated"?

If a mediator is a variable, M , that is causally between X and Y and that *accounts at least in part for the association between X and Y* , then by definition X and Y must be associated in order for M to be a mediator of that effect. According to this logic, if there is no evidence that X affects Y , then how can X 's effect on Y be mediated and so what is the point of estimating indirect and direct effects? But it is possible for M to be causally between X and Y even if X and Y aren't associated. In this case, some prefer to avoid the term *mediator* when describing M and instead refer simply to X 's indirect effect on Y through M (see Mathieu & Taylor, 2006, for a discussion of the distinction between indirect effects and mediation).

The distinction between mediation and indirect effect is not always made by users of the Baron and Kenny method, who may prematurely end the hunt for evidence of indirect effects if there is no evidence that X and Y are associated. If the size of c constrained the size of a and b and therefore their product, this logic would make sense. Unfortunately, no such constraints exist, and it is easy to show that the claim that X can't affect Y indirectly in the absence of a detectable total effect is false. Consider, for example, the covariance matrix in Table 1 based on a sample of 100. Suppose X is an experimental manipulation of exposure to political campaign news, M a measure of trust in government, and Y a measure of the likelihood of voting in the next election. A simple regression of Y on X yields a nonsignificant total effect of X , $c = 0.147$, $p > .20$. It appears from this analysis that there is no relationship

Table 1 Variances (on the Diagonal) and Correlations (on the Off-diagonal) between Four Variables in a Sample of Size 100

	Y	X	M	W
Y	1.381			
X	0.063	0.252		
M	0.171*	-0.388***	1.053	
W	0.429***	0.185 ⁺	-0.106	1.167

⁺ $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$.

between exposure to campaign news and likelihood of voting. And yet in a simple mediation model, the indirect effect of X on Y through M is not zero by a 95% bias-corrected bootstrap confidence interval based on 5000 bootstrap samples (-0.463 to -0.016 , with a point estimate of -0.209), as are the paths from X to M ($a = -0.794$, $p < .001$) and M to Y controlling for X ($b = 0.264$, $p = .035$).³ These results are consistent with the claim that exposure to campaign news reduces trust, which in turn lowers the likelihood of voting. In this example, an investigator who proposed X exerts its effect on Y indirectly through M would never have reached the stage of testing the indirect effect if he or she insisted the total effect be detectably different from zero.

That X can exert an indirect effect on Y through M in the absence of an association between X and Y becomes explicable once you consider that a total effect is the sum of many different paths of influence, direct and indirect, not all of which may be a part of the formal model. For example, it could be that two or more indirect paths carry the effect from X through Y , and those paths operate in opposite directions (cf., MacKinnon, Krull, & Lockwood, 2000). Much as a main effect in 2×2 ANOVA might be nonsignificant if the simple effects are opposite in sign (i.e., a crossover interaction), two or more indirect effects with opposite signs can cancel each other out, producing a total effect and perhaps even a total *indirect* effect that is not detectably different from zero, in spite of the existence of specific indirect effects that are not zero.

Consider now a multiple mediator model of the same data in Table 1, in which two variables M and W are both proposed as mediators of the effect of X on Y (as in Figure 1C). Suppose W is a measure of perceived importance of the outcome of the election. As noted earlier, the total effect of the manipulation (X) on likelihood of voting (Y) is nonsignificant. However, 95% bias corrected bootstrap confidence intervals (based on 5000 bootstrap samples) for specific indirect effects through trust in government ($X \rightarrow M \rightarrow Y$) and perceived importance of the outcome of the election ($X \rightarrow W \rightarrow Y$) both do not include zero. The specific indirect effect of X on Y through M is negative (-0.473 to 0.063 , with a point estimate of -0.226), whereas the specific indirect effect through W is positive (0.006 to 0.436 , with a point estimate of 0.191). The direct effect, as in the prior analysis, is not significant and in fact is almost the same as the total effect ($c' = 0.182$, $p > .20$), and all a and b paths are statistically significant (albeit the $X \rightarrow W$ path only “marginally” so, $p = .056$).

In this example, exposure to campaign news appears to exert an effect on likelihood of voting through two mechanisms that work in opposite directions, by increasing perceived importance of the outcome, which then increases likelihood, while simultaneously lowering trust in government, which reduces likelihood. These two effects, operating together, in effect cancel each other out in the estimation of the total effect and so appear as a nonsignificant total effect of the manipulation. The moral of this example is that I concur with others (e.g., MacKinnon et al., 2000; Shrout & Bolger, 2002) who recommend that researchers not require a significant total effect before proceeding with tests of indirect effects. If you find a significant indirect effect in the absence of a detectable total effect, call it what you want—

mediation or otherwise. The terminology does not affect the empirical outcomes. A failure to test for indirect effects in the absence of a total effect can lead to you miss some potentially interesting, important, or useful mechanisms by which X exerts some kind of effect on Y .

Comparing Specific Indirect Effects

Researchers frequently test more complex models that include multiple linkages between independent variables, proposed mediators, and outcomes. Some examples in communication include Afifi, Afifi, Morse, and Hamrick (2008), Ledbetter (2009), Smith, Downs, and Witte (2007), and Southwell and Torres (2006). The inclusion of multiple pathways to an outcome means that different theories can be pitted against each other in a single model (i.e., Theory A might propose variable M_1 functions as a mediator of X 's effect on Y , whereas Theory B proposes M_2 as the mediator) while eliminating the problem of estimation bias that occurs when multiple mediators that are intercorrelated are tested individually in simple mediation models using the Baron and Kenny approach (see Preacher & Hayes, 2008a). Although communication researchers acknowledge that variables typically produce effects through multiple pathways, the reliance on the piecemeal causal steps approach means investigators rarely take the extra step of statistically examining and testing for differences in the relative sizes of specific indirect effects.

The quantification of indirect effects allows the investigator to answer such questions as whether the specific indirect effect of X on Y through proposed Mediator 1 differs in size from specific indirect effect through proposed Mediator 2. Because specific indirect effects from X to Y are free of the scale of measurement of the intervening variables (see Preacher & Hayes, 2008a), they are comparable without standardization or any other form of transformation. MacKinnon (2000) presents a method for conducting such contrasts by deriving the standard error for differences between specific indirect effects. Preacher and Hayes (2008a) discuss pairwise contrasts in single-step multiple mediator models with any number of mediators and provide SPSS and SAS routines for bootstrap-based inference. In SEM, contrasts can be conducted by imposing equality constraints on products of paths and then examining whether those constraints produce a better or worse fitting model (see Preacher & Hayes, 2008a, for LISREL and Mplus code).

Combining Mediation and Moderation

Mediation refers to a sequence of causal relations by which X exerts its effect on Y by influencing intervening variables. Moderation, a term sometimes confused with mediation, describes a situation in which X 's effect on Y varies as a function of some third variable M , the moderator variable. A moderated effect is typically modeled statistically as an interaction between X and the moderator variable, frequently quantified as the product of X and M . Moderation can help us to understand how a process operates if the moderator places constraints on how or when that process can function.

Mediation and moderation can be combined analytically into either a *moderated mediation* or *mediated moderation* model (see Muller, Judd, & Yzerbyt, 2005). For instance, an investigator might propose that X exerts its effect on Y indirectly through some variable M , but that this indirect effect might be larger among men than women, or increase linearly as a function of age, educational attainment, or some other continuum. Readers familiar with multiple group structural equation modeling might approach the modeling of such a process analytically by estimating a mediation model in two or more groups and then compare models in which equality constraints are imposed or relaxed across groups on one or more of the effects defining the indirect and direct effects. If a model with equality constraints on the paths across groups fits worse than one that allows the paths to vary between groups, this suggests that the indirect or direct effects differ across groups.

Recent treatments of the analysis of moderated mediation models focus on the estimation of interactions between the moderator and the pathways that define the indirect effect (Edwards & Lambert, 2007; Muller et al., 2005; Preacher, Rucker, & Hayes, 2007). Preacher et al.'s approach (2007) emphasizes the estimation of *conditional indirect effects*—the value of indirect effects conditioned on values of the moderator or moderators—moderators that can be either continuous (unlike in SEM using the multiple group approach) or categorical. They provide formulas for estimating and testing such effects in five different types of models as well as SPSS macros that can probe moderated mediation models using a variety of approaches, including the bootstrapping of conditional indirect effects. Recent examples of the application of this approach include Jensen (2008) and Palomares (2008).

A *mediated moderation* model conceptualizes an interaction between X and a moderator variable W on Y as carrying its influence through an intervening variable M . For example, Scheufele (2002) shows in his differential gains model of media effects on political participation that the interactive effect of mass media use and interpersonal discussion on political participation carries its effect through political knowledge, which in turn affects participation. Such a model is mathematically equivalent to a moderated mediation model in which the path from X to M is moderated by a third variable W , whereas the path from M to Y is unmoderated (Model 2 in Preacher et al., 2007). The distinction between mediated moderation and this form of moderated mediation is purely one of interpretive focus. In mediated moderation, the focus is on the estimation of the indirect effect of the product of X and W on Y through M , whereas in moderated mediation, the interpretation is directed at estimates of the conditional indirect effect of X on Y through M at values of W .

Effect Size

The quantification of effect size in intervening variable models remains a cutting-edge area of thinking and research. Estimates (point and interval) of indirect effects are scaled in terms of the unit of metric of X and Y . They are free of the metric of M . If X and Y have substantively meaningful metrics, then the indirect effect of X on Y has a substantively meaningful interpretation regardless of how M is quantified and can

be interpreted as an effect size measure without further mathematical manipulation. For example, if X is an experimental manipulation coded 0/1 for exposure (1) or not (0) to smoking cessation public service announcements and Y is a measure of number of cigarettes smoked in the last week, the indirect effect of X on Y can be interpreted as the difference in the number of cigarettes smoked attributable to the indirect pathway through the mediator. Most would agree that this is a meaningful measure of effect size.

When the scale of measurement is arbitrary, some advocate the interpretation of standardized rather than unstandardized effects. A standardized indirect effect is calculated as the product of the standardized estimates of a and b . But if X and Y are arbitrary scales without inherent quantitative meaning, standardization does not make the scaling of the indirect effect any more meaningful. Standardizing X and Y prior to analysis only changes the “one-unit difference in X ” interpretation to a standard deviation rather than the original metric of measurement and, typically, will produce an indirect effect scaled between -1 and 1 as opposed to $-\infty$ and ∞ .⁴ If X is dichotomous, standardized indirect effects are actually less meaningful than unstandardized ones, for standardization destroys the interpretation of the indirect effect as the mean difference between groups on the outcome attributable to the pathway through M . Attempts have been made to scale indirect effects in proportion terms relative to some kind of reference, but none of the methods proposed thus far are particularly satisfying (see, e.g., MacKinnon & Dwyer, 1993; Sobel, 1982). The ratio of the indirect effect to the total effect (i.e., ab/c) is often interpreted as the proportion of the total effect that is mediated. This measure has little to recommend it, for c can be smaller than ab (yielding a proportion greater than 1), c and ab can have different signs, yielding a negative proportion, it is undefined if $c = 0$, and it explodes to infinity as c approaches zero, making most any indirect effect seem massive. A related measure references the indirect effect relative to the direct effect (i.e., ab/c'). Although slightly better, this measure also should not be interpreted as a proportion, for it also has no upper or lower bounds and explodes as c' approaches zero. Recently, Fairchild, MacKinnon, Taborga, and Taylor (2009) introduced a measure of effect size they report can be interpreted as the proportion of the variance in Y explained by the indirect effect. But this measure also does not have the properties of a proportion, as it too can be negative.

Conclusion

Communication scholars have always welcomed advances in statistical techniques into their analytical arsenal. In this paper, I attempted to accelerate the adoption of modern methods of mediation analysis in the field by challenging habit and the conventional wisdom of common practice and offering “new” alternatives and perspectives. My treatment has been necessarily brief and certainly nonexhaustive. For instance, I spent little time on latent variable models here, I gave no treatment to longitudinal mediation models (see, e.g., Cheong, MacKinnon, & Khoo, 2003; Cole & Maxwell, 2003; Little, Preacher, Selig, & Card, 2007), and I ignored the burgeoning

literature on the estimating of intervening variable effects in multilevel models (e.g., Bauer, Preacher, & Gil, 2006; Kenny, Korchmaros, & Bolger, 2003; Krull & MacKinnon, 2001). Those who want to take additional steps will find a number of overview articles and book chapters (e.g., MacKinnon, Fairchild, & Fritz, 2007; Preacher & Hayes, 2008b), a recent book (MacKinnon, 2008), and a special issue of *Organizational Research Methods* (2008) valuable as aides in their trek into the twenty-first-century analysis of intervening variable models, and the reader will certainly find much to explore and think about in the references cited herein.

Notes

- [1] Although the partitioning of a total effect into direct and indirect components in the manner described here does not require the assumption that the errors in estimation are uncorrelated, such intercorrelation can bias parameter estimates and standard errors. Correlated errors can result from, among other things, the exclusion of variables from the model that are correlated with two or more included variables.
- [2] Otherwise, I agree wholeheartedly with the position Holbert and Stephenson (2003) take in their statement that communication researchers should place much more emphasis on the estimation and testing of indirect effects than they have in the past.
- [3] Bootstrapping requires the raw data rather than just a covariance matrix. Readers interested in the raw data used in this example can contact me at hayes.338@osu.edu and I will gladly send it.
- [4] Contrary to conventional wisdom, it is possible (although rare in practice) for a standardized coefficient to be larger than 1 in absolute value (Deegan, 1978). This means that even the standardized indirect effect has no real upper or lower bound.

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