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Contemporary Approaches to Assessing Mediation in Communication Research

Kristopher J. Preacher

Andrew F. Hayes

Communication scientists routinely ask questions about causal relationships. Whether it is examining the persuasive impact of public service announcements on attitudes and behavior, determining the impact of viewing political debates on political knowledge or voter turnout, or assessing whether success in achieving one's Internet browsing goals prompts greater interest in e-commerce, communication scholars frequently conduct research to answer questions about cause. Data analysis usually focuses on examining if the putative causal variable, whether manipulated or measured, is related to the outcome using a linear model such as analysis of variance or linear regression. In many arenas of research, such analyses, when accompanied by good research design, are sufficient to answer the question as to whether variation in *X* causes variation in *Y*. But deeper

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understanding accrues when researchers investigate the *process* by which a given effect is produced. Although it might be interesting and even important to discover, for instance, that learning about political news is affected by whether exposure occurs through print or online formats, we usually want to understand more about how these effects or relationships arise—the *mechanisms* that produce and explain the associations.

When we ask questions about mechanism rather than simply whether or not an effect exists, we are asking about *mediation*—the process through which X exerts its effect on Y through one or more *mediator* variables. If a variable M is causally situated between X and Y and accounts for their association (at least in part), we say that *M mediates* the relationship, a term first used in this context by Rozeboom (1956). It is also said that X has an *indirect effect* on Y through M.[1] The mediator, M, sometimes called an *intervening variable* or a *mechanism* (Hoyle & Robinson, 2004), can be said to explain how a given effect occurs.

Before the cognitive revolution of the 1950s, there was little interest in mediation effects among scientists steeped in the behaviorist tradition. A notable exception was a paper by MacCorquodale and Meehl (1948) distinguishing between *intervening variables* (determinate functions of variables used for convenience) and *hypothetical constructs* (what we today call mediators). The first formal term for what is now called mediation was *interpretation* (Hyman, 1955). Hyman notes, "When the analyst interprets a relationship, he determines the process through which the assumed cause is related to what we take to be its effect" (p. 276). Hyman referred to mediators as *test factors* or *intervening variables* and laid out statistical criteria for establishing mediation that are identical to those popularized decades later by Judd and Kenny (1981) and Baron and Kenny (1986). Much methodological research has been conducted since these landmark contributions. In particular, David MacKinnon and colleagues (e.g., MacKinnon & Dwyer, 1993; MacKinnon, Warsi, & Dwyer, 1995; MacKinnon, Lockwood, Hoffman, West, & Sheets, 2002) have done much in recent years to promote the rigorous assessment of mediation using a variety of sophisticated methods.

McLeod and Reeves (1980) highlighted the importance of studying the mechanisms of media effects, and media researchers have increasingly focused their attention on examining and explaining the mechanisms that produce media effects rather than simply asking whether and to what extent those effects exist (also see McLeod, Kosicki, & Pan, 1996). Indeed, there are numerous examples in the media effects literature of studies testing the extent to which media's effect is mediated, or media itself functions as a mediator, in the relationship between two other variables. For example, Eveland (2001) and Eveland, Shah, and Kwak (2003) found that attention to news media and elaborative processing mediate the effect of surveillance gratification seeking on public affairs knowledge. Scheufele (2002) tested whether the influence of newspaper news use and interpersonal discussion on political participation is mediated by political knowledge. Holbert, Shah, and Kwak (2003) identified viewing of traditional drama, progressive drama, and situation comedy as three mediators of the effect of ideology on opinions concerning women's rights. And Chang (2001) identified advertisement-evoked emotion as a mediator of the effect of advertisement valence on political candidate evaluation and attitude toward the advertisement.

But media effects researchers are certainly not the only scholars in communication interested in studying mechanisms and empirically testing hypotheses about mediation. Lee and Nass (2004) found evidence that the effect of synthetic voices on persuasion is mediated by listeners' sense of social presence. Millar (2002) identified feelings of guilt as one of the mechanisms through which the door-in-the-face technique leads to increased compliance to requests. Knobloch and Carpenter-Thune (2004) examined the extent to which relational uncertainty functions as a mediator of the relationship between feelings of intimacy and the avoidance of certain conversational topics in interpersonal discussion. Hart and Miller (2005) reported evidence that the effect of experiencing certain organizational socialization tactics on feelings of role ambiguity in newly hired managers was mediated by the degree to which the manager received performance appraisals. Other examples can be found in abundance in the organizational, interpersonal, new technology, health, and political communication literature.

An informal content analysis of the major communication journals further reveals that considerable journal space is devoted to testing hypotheses about the extent to which the relationships between constructs of interest to the field are mediated, as well as whether communication variables themselves serve as mediators of interesting and important relationships. We examined the 2002 through 2005 volumes of *Communication Research, Human Communication Research, Journal of Communication, Journalism and Mass Communication Quarterly,* and *Media Psychology,* counting the number of empirical articles (defined as articles that reported the conceptualization, analysis, and results of a research study of some kind) as well as those that reported some kind of formal or informal test of mediation. The results of this perusal through the pages of the communication journals revealed that roughly 1 in 8 included an empirical test of mediation. Indeed, it is difficult to open a volume of these journals and not find such a test somewhere in its pages.

Yet despite the clear importance of understanding mechanisms that drive communication effects and the role of communication as a mediator in social, cognitive, and behavioral phenomena, mediation is a topic that is largely absent from most introductory methods and statistics texts (Pituch, 2004). Even statistical methods books that explicitly target communication scholars (e.g., Hayes, 2005) dedicate only a few pages to the topic. In part to address this lack of coverage, we dedicate the bulk of this chapter to discussing strategies by which mediation hypotheses may be formally tested statistically. We also discuss useful extensions of these strategies, such as how to address hypotheses that involve both mediation and moderation effects.

We suggest ways to proceed when theory implies that multiple mediators may intervene between two variables, and we explain how to address mediation hypotheses in modeling paradigms other than least squares multiple regression, such as structural equation modeling (SEM) and multilevel modeling. We describe how a priori consideration of study design, causality, and statistical power contribute to hypothesis tests with sound scientific bases, and we suggest ways to quantify effect size to facilitate communication of research findings. Finally, we address some practical software concerns for scientists who wish to undertake the rigorous investigation of hypotheses involving indirect effects. Throughout, we concentrate on communicating a conceptual and practical understanding of mediation without getting too deeply involved in the underlying mathematics. For the interested reader, we provide many references to sources with more thorough information. Finally, most of what we discuss may be accomplished using ordinary multiple regression, although SEM may also be used.

Statistical Approaches to Assessing Mediation

In this section, we survey several methods that have been used to quantify and test the statistical significance of mediation effects. The *simple mediation* model is depicted graphically in Figure 2.1. We refer to this as a simple mediation model because it involves only a single proposed mediating variable. Later in the chapter we describe more complicated mediation models such as models with more than one mediator.

The top panel of Figure 2.1 represents a causal process in which X, the independent variable, affects Y, the dependent variable. Path c quantifies this effect, called the *total effect* of X on Y. Although it is common in communication research to index relationships using standardized paths (i.e., derived using standardized variables), most methods for assessing mediation rely on unstandardized paths, and we encourage researchers to follow this tradition. The bottom panel illustrates the components of the total effect. Path a represents the causal effect of the independent variable on the proposed mediator, M. Path b represents the causal effect of the mediator on the dependent variable, controlling for the independent variable, whereas path c' represents the causal effect of the independent variable on the dependent variable controlling for the mediator. In the language of causal analysis, c' is the *direct effect* of X on Y and is distinguishable from the total effect, c, in that the direct effect partials out from the total effect that part of the causal effect that is shared with M. That is, it represents the part of the effect of X on Y in the model that is unique to X. Path b can also be considered a direct effect, in this case the direct effect of the mediator on the outcome variable. The *indirect effect* of X on Y is represented as the two paths linking X to Y through M, which in Figure 2.1 are the

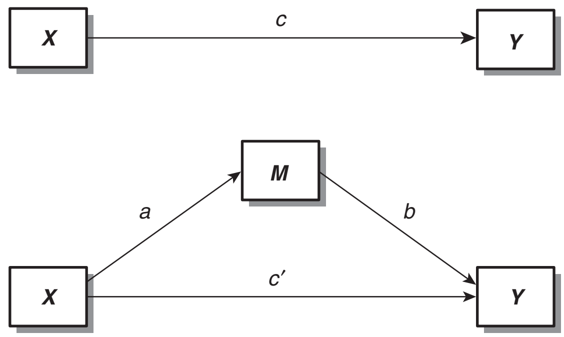

Figure 2.1 A Simple Mediation Model

a and *b* paths. As will be discussed, in causal analysis it is common to quantify the indirect effect of *X* on *Y* through *M* as the product of the *a* and *b* paths. In simple mediation models of this sort, it can be shown that the total effect of *X* on *Y* is equal to the sum of the direct and indirect effects. That is, $c = c' + ab$. Simple algebraic manipulation shows that the indirect effect is the difference between the total and the direct effects of *X* on *Y*: $ab = c - c'$.

CAUSAL STEPS STRATEGY

By far the most popular approach to testing a hypothesis of mediation is the *causal steps* strategy (or *serial approach;* Hoyle & Robinson, 2004), in which the researcher must satisfy a series of criteria before a pattern of effects can be termed mediation. Popularized by Judd and Kenny (1981) and Baron and Kenny (1986), the causal steps approach is most directly attributable to Hyman (1955, p. 280), although his text is cited very rarely. His criteria for establishing that an effect is mediated are as follows:

1. The presumed mediator (*M*) should be related to the assumed causal variable (*X*).
2. The mediator should be related to the assumed effect, *Y*.
3. When the sample is stratified according to the presumed mediator, the (partial) relationship between *X* and *Y* should be smaller than the relationship prior to stratification.

Hyman's second criterion is an example of what he termed a *marginal effect,* a term he applied to both the $X \rightarrow M$ and $M \rightarrow Y$ relationships. *M* is said to *interpret* the original relationship if these criteria are met. Hyman also emphasized the necessity that *X* temporally precede *M,* an important consideration for a purportedly causal model. Implicit in the third criterion is the requirement that *X* is related to *Y*—that there exists a relationship between the presumed cause and the presumed effect to be explained.

Readers familiar with the Judd and Kenny (1981) and Baron and Kenny (1986) criteria for establishing mediation will find Hyman's criteria familiar. According to Judd and Kenny (1981, p. 605), the criteria for concluding that mediation exists are the following:

1. The treatment, *X,* affects the outcome variable, *Y.*
2. Each variable in the causal chain affects the variable that follows it in the chain, when all variables prior to it, including the treatment, are controlled.
3. The treatment exerts no effect upon the outcome when the mediating variable is controlled.

Kenny, Kashy, and Bolger (1998) note that only the last two of these steps are actually required, since a reduction in the strength of *c* implies that *c* was nontrivial before the mediator was introduced into the model. Baron and Kenny's criteria are similar to Hyman's but are stated in terms more explicitly related to statistical significance (p. 1176):

1. Variations in levels of the independent variable significantly account for variations in the presumed mediator.
2. Variations in the mediator significantly account for variations in the dependent variable.
3. A previously significant relationship between the independent and dependent variables is no longer significant after controlling for the mediator.

To illustrate the causal steps strategy as well as other procedures we describe later, we will use data from Kalyanaraman and Sundar (2006), who examined the mechanisms linking Web portal customization to user attitudes. In this study, 60 participants were randomly assigned into one of several experimental conditions in which they were directed to a *MyYahoo* Web portal that had been customized to varying degrees (low, moderate, or high) across participants based on a pretest questionnaire each participant completed prior to coming to the laboratory. During the study, participants were asked to surf the Web using the portal for eight minutes, after which they completed a questionnaire. The independent variable in this example is *perceived customization,* which reflects how customized the participants felt the portal was to their own interests and how "unique" it was to them (higher = greater perceived customization). The dependent variable is *attitude toward the portal* (higher = more positive attitude). Three additional variables were proposed as potential mediators of any relationship found between perceived customization and attitude: (1) perceived interactivity, (2) perceived novelty, and (3) perceived community. Perceived interactivity gauges the extent to which a user feels the experience with a Web page is an interactive exchange of information and feedback that the user controls. Perceived novelty quantifies the extent to which the user feels that the Web page is providing a unique service compared to other sites and somehow stands out as different. Perceived community measures a person's sense of "belongingness" and the extent to which the Web portal was perceived to welcome and want the user. These variables are scaled positively, such that a higher score reflects more of the construct. Greater detail on the measurement and study procedures can be found in Kalyanaraman and Sundar (2006).[2]

Drawing on this example, it may be of interest to determine whether perceived interactivity (M) mediates the effect of perceived customization (X) on attitudes toward the Web portal (Y). Baron and Kenny's (1986) criteria oblige us to estimate the paths in Figure 2.1, most easily using ordinary least squares (OLS) regression, although other estimation methods could be used. More specifically, we estimate the coefficients of the following models:

$$\hat{Y} = i_1 + cX$$
$$\hat{M} = i_2 + aX$$
$$\hat{Y} = i_3 + c'X + bM$$

where the i's are intercept terms and the carets over Y and M represent that these are estimated values. Using the Kalyanaraman and Sundar (2006) data, individual OLS regressions (or using the SPSS macro provided by Preacher and Hayes, 2004) reveal that the total effect is positive and different from zero, $c = 0.5119$, $SE = 0.0588$, $t(58) = 8.7183$, $p < .001$. Thus, greater perceived customization is associated with more positive attitudes toward the portal. Second, perceived customization does predict perceived interactivity, the putative mediator, $a = 0.4013$, $SE = 0.0778$, $t(58) = 5.1592$, $p < .001$. The greater the perceived customization, the more interactive the user perceives the Web portal to be. Third, perceived interactivity is significantly and positively related to attitude when controlling for perceived customization, $b = 0.3011$, $SE = 0.0917$, $t(57) = 3.2826$, $p < .002$. This suggests that the relationship between the mediator and the outcome is not spurious (due to both being caused by perceived customization) or epiphenomenal (which occurs when a predictor is correlated with an outcome only because the predictor is correlated with another variable that is causally related to the outcome). Finally, the direct effect of perceived customization on attitude is smaller than the total effect, $c' = 0.3911$, $SE = 0.0656$, $t(57) = 5.9575$, $p < .001$. According to Baron and Kenny's criteria, this pattern is consistent with a claim that *partial mediation* is occurring, as the direct effect of customization on attitude is statistically different from zero even after controlling for the mediator (we elaborate on the distinction between partial and complete mediation later). That is, part of the mechanism producing the effect of customization on attitudes is the tendency for more customized Web portals to be perceived as more interactive, which in turn leads to more positive attitudes.

The causal steps approach is by far the most commonly used method for assessing mediation. The criteria have recently been extended for use in within-subject designs as well (Judd, Kenny, & McClelland, 2001). But despite its simplicity and intuitive appeal, the causal steps strategy suffers from serious limitations relative to other methods we discuss soon. First, it is possible to observe seemingly paradoxical effects using this approach. For example, a significant c and nonsignificant c' may differ by a trivial amount in absolute terms, yet the causal steps criteria would indicate that mediation is occurring (a possible Type I error; Holmbeck, 2002). Particularly in large samples, it is possible to observe significant yet widely differing c and c' estimates, which might lead to the conclusion that mediation is of trivial magnitude (a possible Type II error).[3] Furthermore, the causal steps strategy has been found to exhibit below-expected Type I error rates as well as to suffer from very low power (MacKinnon et al., 2002; Pituch, Whittaker, & Stapleton, 2005), perhaps because significance requirements are placed on several regression coefficients. According to this approach, mediation cannot be claimed unless all relevant paths are statistically significant. Other approaches described below do not impose statistical significance requirements on all paths. Importantly, the causal steps strategy obliges the researcher to infer the presence and extent of mediation from a pattern of hypothesis tests, none of which directly addresses the hypothesis of interest—whether the causal path linking X to Y through M is nonzero and in the direction expected. Finally, because it does not directly estimate the size of the indirect effect, there is consequently no way to obtain a confidence interval for the population indirect effect (Pituch et al., 2005). Although the causal steps strategy is important to understand because of its widespread use, we believe its disadvantages relative to alternatives described later are large enough to warrant a recommendation that it not be used.

PARTIAL CORRELATION STRATEGIES

Olkin and Finn (1995, p. 160) describe a method of assessing mediation using correlations. Their point estimate of the mediation effect is $r_{YX} - r_{YX.M}$, where r_{YX} is the simple correlation between X and Y and $r_{YX.M}$ is the partial correlation between X and Y controlling for M. Mediation is assessed by calculating the ratio of $r_{YX} - r_{YX.M}$ to its standard error and deriving the p-value using the standard normal distribution or by constructing a confidence interval for the population value $\rho_{YX} - \rho_{YX.M}$. A few cautions are relevant to this method. First, Olkin and Finn's (1995) standard error contains a mistake; a corrected standard error is provided by Lockwood and MacKinnon (2000) and MacKinnon et al. (2002). Second, the test is cumbersome to conduct by hand, and we know of no software that implements it. Furthermore, Kenny (personal communication) and MacKinnon et al. (2002) note that even in the absence of mediation, the partial correlation method can lead to a spurious conclusion of mediation—a Type I error. We do not recommend that the partial correlation strategy be used.

DIFFERENCES IN COEFFICIENTS STRATEGIES

Noting that a nontrivial drop in the $X \rightarrow Y$ effect after partialing out the mediator constitutes positive evidence for mediation, several authors have examined the distributional properties of $c - c'$. MacKinnon et al. (2002) examine three such methods, proposed by Clogg, Petkova, and Shihadeh (1992), Freedman and Schatzkin (1992), and McGuigan and Langholtz (1988). All three methods suffer from statistical limitations, as detailed by MacKinnon et al. (2002). In addition, note that $c - c'$ is not a particularly useful way to quantify an indirect effect in models with multiple mediators or those involving both moderation and mediation, to be discussed later. We do not recommend that differences in coefficients strategies be used.

NESTED MODEL STRATEGY

Judd and Kenny (1981) and Holmbeck (1997) suggest a method of testing mediation hypotheses that takes advantage of a key feature of structural equation modeling—the ability to constrain model parameters to fixed values. This strategy makes use of the chi-square model fit index (χ^2) given as standard output by SEM software. Keeping sample size constant, the better a model fits data, the smaller the value of χ^2 will be. Tests of nested models[4] may be conducted by comparing values of χ^2 derived from the constrained and full models. When using path analysis, a completely saturated model will yield a χ^2 of zero. Thus, any constraints placed on such a model will necessarily increase χ^2 to a degree reflecting the unreasonableness of the constraint given the data. A test of the difference in fit between two nested models may be conducted by computing the difference in χ^2 values and comparing the result to a χ^2 distribution with degrees of freedom (*df*) equal to the difference in *df* between the two models (*df* = 1 in the case of simple mediation). If the value of χ^2 exceeds the critical value established by *df* and the desired α level for the test, then the models are said to have significantly different fit.

If the hypothesis is one of simple mediation, then using the nested model strategy would entail estimating two models—one estimating the direct effect of X on Y (c') and one in which c' has been constrained to zero. If complete mediation is occurring, then path c' should not differ appreciably from zero. If the addition of this constraint is accompanied by a significant decrease in model fit (i.e., an increase in χ^2), then complete mediation is ruled out. If the researcher is interested in determining whether any mediation at all is occurring, then one could estimate a model without M to obtain c, then constrain the c' coefficient in the full model (containing M) to equal c. If mediation is occurring, then path c should be significant and c' should be smaller than c upon the addition of the mediator to the model. Such a situation would imply that the value estimated for the original path coefficient would be unreasonably large in the presence of the mediator. If the addition of this constraint is accompanied by a significant decrease in model fit, then support has been found for mediation.

A shortcoming of the nested model strategy is that it involves testing the hypothesis of *complete* mediation, or that the entire effect of X on Y is carried through M.[5] Whereas strong tests of hypotheses are generally to be encouraged, we reiterate Baron and Kenny's (1986) observation that complete mediation is probably rare in practice. Second, use of the difference in χ^2 has been criticized in the methodological literature on the basis that it is heavily dependent on sample size. Given a moderately large sample, the nested model strategy will virtually always result in rejection of the hypothesis of complete mediation.

PRODUCT OF COEFFICIENTS STRATEGIES

Most methodologists agree that the product of the coefficients a and b is a logical way to quantify an indirect effect.[6] The logic behind the product of coefficients strategy is simple. If M mediates the effect of X on Y, then X should affect M and M should affect Y while controlling for X. If either a or b is zero, then their product will be zero. If both a and b are nonzero, as is the case if M mediates the $X \rightarrow Y$ relationship, then the product will be nonzero. The product of a and b will be further from zero as the strength of the indirect effect increases. Furthermore, typically $ab = (c - c')$, which is one way mediation is operationalized using the causal steps strategy. Therefore, it seems sensible to use ab as a basis for statistical inference and confidence interval construction. Three broad strategies—the product of coefficients, distribution of the product, and bootstrapping—quantify the indirect effect in this way, and differ mainly in how they construct and use the sampling distribution of *ab*. We discuss each in turn.

Given that the indirect effect is quantified by the point estimate *ab*, the goal is to determine whether *ab* is significantly different from some specified value (usually zero) or to estimate its precision and report a confidence interval for the population value of *ab*. Assuming normality of the sampling distribution of *ab*, all that is required to construct a confidence interval for the population *ab* is the sample point estimate *ab* and its standard error (i.e., the mean and standard deviation of the sampling distribution of *ab*). A number of approaches can be used to derive the standard error under the assumption of normality. The multivariate delta method, for example, proceeds by first forming a Taylor series expansion of the function of interest (here *ab*) and applying the definition of a variance to that expansion. Preacher, Rucker, and Hayes (2007) provide detailed derivations of the variances[7] of several indirect effects, one of which is for the simple mediation model, using a matrix expression for the second-order delta method. Aroian (1947), Goodman (1960), Folmer (1981), MacKinnon et al. (1995), and Sobel (1982, 1986, 1988) provide alternative derivations that converge on the same result. In the case of simple mediation involving only one mediator, the second-order expression for the estimated standard error of *ab* is

$$SE_{ab} = \sqrt{b^2 s_a^2 + a^2 s_b^2 + s_a^2 s_b^2}$$

where s_a^2 and s_b^2 are the asymptotic variances of a and b. These quantities are readily available from standard regression or SEM software. The third term in the expression above, $s_a^2 s_b^2$, is sometimes omitted from the standard error expression not only because it does not appear in the first-order solution for the variance but also because it tends to be trivially small in practice (MacKinnon et al., 1995). Goodman (1960) provides an unbiased variance estimator that subtracts rather than adds $s_a^2 s_b^2$ in the equation above, but we do not recommend using Goodman's approach as it can sometimes result in a negative value for the standard error (MacKinnon et al., 2002).

Regardless of the standard error estimator used, hypothesis tests are conducted by dividing the point estimate *ab* by the standard error and comparing the resulting ratio to a standard normal distribution. This is known as the *Sobel test* for an indirect effect (Sobel, 1982). Alternatively, the standard error can be used in conjunction with critical values for the standard normal distribution (e.g., ±1.96) to form a confidence interval for the population indirect effect. The product of coefficients approach has been found to perform well in simulation studies with sample sizes greater than 50 or so, but the standard error tends to be overestimated in small samples (MacKinnon et al., 1995). The advantages of the product of coefficients approach are that it is easy and intuitive, and it is implemented in numerous software applications (see the Computer Software section below). One disadvantage is that it requires a large sample in order to be confident that the sampling distribution of *ab* divided by its standard error is normal, which one must assume to derive a *p*-value or compute a confidence interval using this approach. In addition, the Type I error rate and power tend to be lower than optimal (MacKinnon, Lockwood, & Williams, 2004; Pituch et al., 2005). Of greatest concern is the normality assumption. This is usually a safe assumption in extremely large samples, but in practice, *ab* tends to be positively skewed and highly leptokurtic (Bollen & Stine, 1990; MacKinnon et al., 2002; MacKinnon et al., 2004), compromising the validity of statistical inference. However, the performance of the product of coefficients approach improves with increased sample size.

Bobko and Rieck (1980) describe a related procedure that involves computing the significance of the product of regression coefficients a and b in Figure 2.1 when X, M, and Y have been standardized. Under these circumstances, $a = r_{MX}$ and b is the standardized partial regression weight for M when Y is regressed on M and X. Type I error rates are comparable to those for the Sobel test (MacKinnon et al., 2002). In theory, Bobko and Rieck's (1980) approach should provide identical results to the first-order version of the Sobel test because the scales of the variables involved should not influence whether or not the empirical evidence is consistent with an indirect effect. In support of this, MacKinnon et al. (2002) found that this method resembled the first-order Sobel test in terms of power and Type I error rates.

Again drawing on our running example, we used the Sobel test to assess the presence of an indirect effect of perceived customization on attitude toward the portal through perceived interactivity. From the regression analyses performed earlier, $a = 0.4013$ and $b = 0.3011$, so $ab = (0.4013)(0.3011) = 0.1208$. Using the second-order estimator of the standard error of ab,

$$\begin{aligned} SE_{ab} &= \sqrt{(0.3011)^2(0.0778)^2 + (0.4013)^2(0.0917)^2 + (0.0778)^2(0.0917)^2} \\ &= 0.0442 \end{aligned}$$

Dividing the estimated indirect effect by this standard error yields $Z = 0.1208/0.0442 = 2.7330$, which exceeds the critical value of ± 1.96 for a hypothesis test at $\alpha = .05$, assuming that the sampling distribution of ab is normal. These results are consistent with the claim that perceived interactivity mediates the effect of perceived customization on attitudes. In addition to conducting a significance test, we could use this standard error to create confidence limits for the indirect effect in the usual way. For example, the lower and upper bounds of a 95% confidence interval are given by $ab \pm 1.96 SE_{ab}$. The 95% confidence interval computed on the basis of sample data is $0.1208 \pm 1.96(0.0442)$ or 0.0342 to 0.2074.

DISTRIBUTION OF THE PRODUCT STRATEGIES

The product of coefficients strategy is simple to apply but suffers from the limitation that it requires the assumption of normality of the sampling distribution of *ab*. In practice, this sampling distribution is rarely normal, although it may approach normality in large samples. The distribution of the product family of strategies (see MacKinnon, Lockwood, & Hoffman, 1998; MacKinnon et al., 2002; MacKinnon et al., 2004) circumvents the need to assume normality by making use of the known distribution of the product of two normally distributed variables (Aroian, 1947; Craig, 1936; MacKinnon et al., 2004; Springer, 1979). With this approach, the assumption of normality of the sampling distributions of *a* and *b* is still invoked, but this assumption is much more realistic than the assumption of normality of the distribution of their product, *ab*. The form of the distribution of the product is highly complex, but values of the function are tabulated in Springer and Thompson (1966) under the null condition that $a = b = 0$. Although there are tables that do not assume both *a* and *b* are zero (Meeker, Cornwell, & Aroian, 1981), for hypothesis testing their use still requires assumptions about the true value of either *a* or *b*, information that is not usually available. However, these tables can be used for generating confidence intervals, and confidence intervals can be used as an indirect means of testing a hypothesis by assessing whether the null hypothesized value of the indirect effect is inside of the confidence interval. Recently, MacKinnon, Fritz, Williams, and Lockwood (in press) have made SPSS, SAS, and R macros available that can be used for generating confidence intervals using the distribution of products method. These macros make implementation of this method easier than it has been in the past.

BOOTSTRAPPING

The Sobel test relies on distributional assumptions that are typically violated in practice. Through the use of the normal distribution for deriving *p*-values and confidence intervals, the Sobel test assumes that the sampling distribution of the indirect effect is normal. But there is strong reason to be suspicious of this assumption, especially in small to moderately sized samples. It is documented theoretically and through simulation that the sampling distribution of the indirect effect converges to normal as the sample size increases, but not quickly enough to justify the routine use of the Sobel test. More and more, statisticians are advocating a move away from statistical procedures that rely on assumptions, particularly when they are unrealistic, to computationally intensive methods such as *bootstrapping*, as these methods typically make fewer unwarranted assumptions and, as a result, can produce more accurate inference (see, e.g., Efron & Tibshirani, 1998; Good, 2001; Lunneborg, 2000; Mooney & Duval, 1993; Rodgers, 1999). Statistical methodologists who study mediation have taken this call seriously and are advocating bootstrapping as one of the better methods for estimating and testing hypotheses about mediation (e.g., Bollen & Stine, 1990; Lockwood & MacKinnon, 1998; MacKinnon et al., 2004; Preacher & Hayes, 2004, 2007; Shrout & Bolger, 2002).

To bootstrap an indirect effect, an empirical approximation of the sampling distribution of the product of the a and b paths is generated by taking a new sample of size n *with replacement* from the available sample and estimating a and b as usual. That is, each time a case is drawn from the original sample, that case is put back into the pool, potentially to be chosen again as the sample of size n is constructed. These estimates of a and b are used to calculate ab^*, the indirect effect in a single resample of size n from the original data. This process is repeated over and over for a total of k times, preferably at least 1,000. The distribution of the k values of ab^* serves as an empirical, nonparametric approximation of the sampling distribution of ab. The mean of the k estimates of ab^* can be used as a point estimate of the indirect effect, and the standard deviation functions as the standard error of the sampling distribution of ab. A g% confidence interval for ab is derived by sorting the k values of ab^* from low to high. The lower and upper bounds of the confidence interval are defined, respectively, as the $0.5(1 - g/100)k^{\text{th}}$ and $1 + 0.5(1 + g/100)k^{\text{th}}$ values of ab^* in the sorted distribution. For instance, when $k = 1{,}000$, the lower and upper bounds of a 95% confidence interval are the 25th and 976th values of ab^* in the sorted distribution of estimates. This procedure yields what is called a *percentile-based* confidence interval. More accurate confidence intervals can be derived through the process of *bias correction* or *bias correction and acceleration* (see Efron, 1987; Efron & Tibshirani, 1998; Lunneborg, 2000; Preacher & Hayes, 2007; MacKinnon et al., 2004; and Stine, 1989, for details on these corrections). The null hypothesis of no indirect effect is tested by determining whether zero is inside of the confidence interval. If not, the researcher can claim that the indirect effect is different from zero. Although still a relatively new approach to testing mediation hypotheses, research to date has shown that bootstrapping the indirect effect is superior to the causal steps, product of coefficients, and distribution of product methods, both in terms of power and Type I error rates (MacKinnon et al., 2004).

The primary advantage of bootstrapping is that no assumptions are made about the sampling distributions of a, b, or their product, because bootstrapping approximates the sampling distribution of ab empirically, with no recourse to mathematical derivations. The result is that bootstrapping provides confidence intervals that cannot be obtained with the product of coefficients method (Lockwood & MacKinnon, 1998). For instance, the Sobel test and product of coefficients method assume that the sampling distribution of ab is symmetrical. But this is not usually true. Bootstrapping can produce confidence intervals that are asymmetric, in that the lower bound of the confidence interval may be more or less distant from the point estimate than the upper bound. Furthermore, bootstrapping enables researchers to use smaller samples than would be necessary to satisfy the distributional assumptions of other methods (although samples should not be *too* small).

The disadvantages of bootstrapping are few and minor. First, the accuracy of confidence limits obtained through bootstrapping depends on the number of resamples, and resampling takes time. However, with today's fast desktop computers this is no longer a realistic limitation. Second, if the same sample is subjected to bootstrapping multiple times, the same confidence limits will never be obtained (MacKinnon et al., 2004). Third, bootstrapping is useful only to the extent that the distributions of the variables in the original sample closely approximate the population distribution. Large samples are likely to be more representative than small samples. Fourth, raw data must be available in order to use bootstrapping; if only correlations or covariances are available, some other method must be used to assess mediation. Finally, only a handful of software applications currently implement bootstrapping, but this situation is changing rapidly. Nevertheless, given its superior performance relative to alternatives combined with its few and weak assumptions, we believe bootstrapping is the preferred method and thus strongly advocate its routine use.

To illustrate, we bootstrapped the indirect effect of perceived customization on attitude toward the portal through perceived interactivity using the SPSS macro described by Preacher and Hayes (2004, 2007). With 5,000 bootstrap samples, the point estimate was 0.1222, with a 95% confidence interval from 0.0409 to 0.2618. As zero is not in the confidence interval, these results are consistent with the claim that perceived customization's effect on attitude is at least partially indirect through perceived interactivity. Unlike the Sobel test and other methods described earlier, we need not assume anything about the shape of the sampling distribution of *ab* to have confidence in this conclusion. Notice as well the asymmetry in the confidence interval, with the upper bound being much farther from the point estimate than the lower bound. This reflects the true asymmetry of the sampling distribution of the indirect effect.

Extensions to More Complex Mediator Models

In the previous section, we described many of the existing methods for estimating and testing indirect effects in simple mediation models. In this section, we extend some of these methods to models with more than a single mediator and to models in which the paths to or from the mediator are allowed to vary systematically as a function of one or more other variables.

MULTIPLE MEDIATOR MODELS

Variables often exert their effects through multiple mediators, and it behooves researchers to consider explanations beyond simple mediation, preferably before the data collection so that such explanations can be tested statistically. Shah, Cho, Eveland, and Kwak (2005), for instance, examined a model in which both interactive civic messaging and interpersonal discussion functioned as mediators of the effect of news exposure on civic participation. Beaudoin and Thorson (2004) examined the mediating role of reliance on the news media and elaboration of its contents in explaining the effect of learning motivations on political knowledge. Additional examples of multiple mediator models in the communication literature include Kiousis, McDevitt, and Wu (2005), Slater and Rasinski (2005), and Holbert et al. (2003).

A graphical depiction of a model involving multiple mediators can be found in Figure 2.2. We call this a *single-step multiple mediator* model, in that although it contains several mediators, no mediators affect each other. That is, it takes only a single step to get from X to Y, through one and only one of the mediators. Other models involving multiple mediators are possible (e.g., $X \rightarrow M_1 \rightarrow M_2 \rightarrow Y$; Cheung, 2007; Taylor, MacKinnon, & Tein, in press), but we restrict our discussion to single-step models such as in Figure 2.2. The top panel of the figure represents the total effect of X on Y, represented with the unstandardized path coefficient c, and the bottom panel represents both the *direct effect* of X on Y (path c') and the *direct effects* of M on Y (the b paths). The *specific indirect effect* of X on Y via mediator j is defined as the product of the two unstandardized paths linking X to Y via that mediator. For example, the specific indirect effect of X on Y through M_1 is quantified as a_1b_1. The *total indirect effect* is the sum of the specific indirect effects, $\Sigma_i(a_ib_i)$, $i = 1$ to j, where j is the number of proposed mediators. The total effect of X on Y is the sum of its direct effect and the j specific indirect effects; that is, $c = c' + \Sigma_i(a_ib_i)$, $i = 1$ to j.

The dual challenge for the researcher in investigating models such as these is to assess the presence and strength of the total indirect effect through the set of mediators and to assess the presence and strength of the specific indirect effects through individual mediators. Although the researcher might be tempted to employ a set of simple mediation analyses, one for each proposed mediator, this approach is problematic for a number of reasons. One cannot simply add up the indirect effects calculated in several simple mediation analyses to derive the total indirect effect, as the mediators in a multiple mediator model typically will be intercorrelated. As a result, the specific indirect effects estimated using several simple mediation analyses will be biased and will not sum to the total indirect effect through the multiple mediators. Furthermore, hypothesis tests and confidence intervals calculated for specific indirect effects without controlling for the other mediators in the model will be

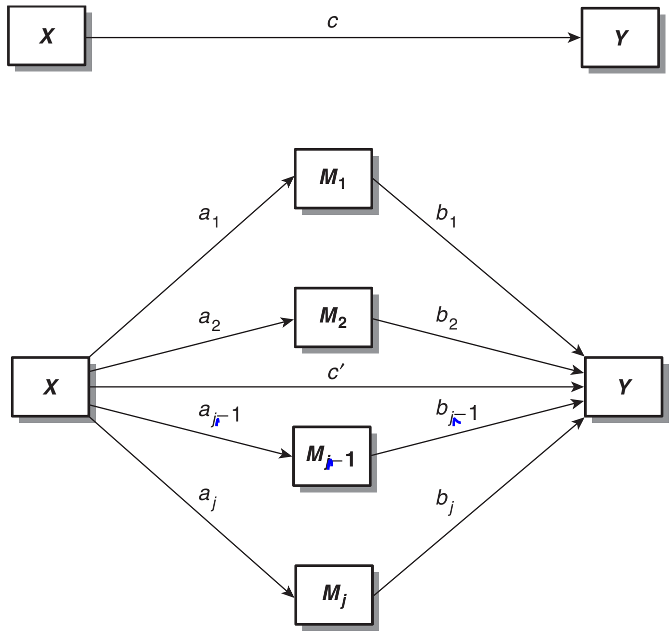

Figure 2.2 A Single-Step Multiple Mediator Model

invalid. Although the total indirect effect can be estimated easily as $c - c'$ using two regression analyses, this simple subtraction is purely descriptive. A multiple mediation analysis is more appropriate under such circumstances. For excellent overviews of methods to examine and interpret total and specific indirect effects, consult Alwin and Hauser (1975), Bollen (1987), Brown (1997), Sobel (1982, 1986, 1988), and sources cited therein. MacKinnon (2000) suggests an extension of the product of coefficients approach to assessing the significance of the total indirect effect and pairwise contrasts between specific indirect effects. Holbert and Stephenson (2003) describe the advantages of a distribution of the products method in a multiple mediator model. Preacher and Hayes (2007) elaborate on MacKinnon's methods, providing software to facilitate the use of the product of coefficients approach and to bootstrap confidence intervals for both total and specific indirect effects, as well as pairwise contrasts of specific indirect effects. Large-scale simulation studies have also been undertaken to examine and compare approaches to assessing multiple mediation (Azen, 2003; Briggs, 2006; Williams, 2004) and generally show bootstrapping to be the preferred method. We refer the reader to these sources for in-depth discussions and mathematical treatments.

As an example of a multiple mediation analysis, consider the hypothesis that the effect of perceived customization (X) on attitude (Y) is mediated by perceived interactivity, perceived novelty, and perceived community. Referring to Figure 2.2 and our running example, let X refer to perceived customization; let M_1, M_2, and M_3 refer to perceived interactivity, perceived novelty, and perceived community, respectively; and let Y refer to attitude toward the portal. We estimated the paths in this model using an SPSS macro described in Preacher and Hayes (2007) and available online at http://www.comm.ohio-state.edu/ahayes/. The output from this macro can be found in the Appendix.

Of course, the total effect of X on Y, path c, does not change as a function of the nature and number of mediators, so it remains 0.5119 and statistically different from zero. We find that the total indirect effect is $a_1b_1 + a_2b_2 + a_3b_3 = 0.3104$. Using the multivariate delta method as discussed in Preacher and Hayes (2007), the estimated standard error is 0.0931. Dividing the total indirect effect by the standard error yields $Z = 0.3104/0.0931 = 3.3330$, which leads to a rejection of the null hypothesis of no total indirect effect at any reasonable α level. The specific indirect effects are $a_1b_1 = (0.4013)(0.2535) = 0.1018$ (through perceived interactivity), $a_2b_2 = (0.6446)(0.1700) = 0.1096$ (through perceived novelty), and $a_3b_3 = (0.5829)(0.1699) = 0.0990$ (through perceived community). Dividing these by their estimated standard errors yields:

$$z_{a_1b_1} = \frac{0.1018}{0.0408} = 2.4948,\ p = .0126$$

$$z_{a_2b_2} = \frac{0.1096}{0.0645} = 1.6989,\ p = .0983$$

$$z_{a_3b_3} = \frac{0.0990}{0.0657} = 1.5063,\ p = .1320$$

From these analyses, we can conclude that perceived interactivity mediates the effect of perceived customization on attitudes. It seems that more customized Web portals are perceived as more interactive, and this interactivity leads to a more positive attitude. However, perceived novelty and perceived community do not mediate the effect of customization on attitudes.

This approach of dividing a specific indirect effect by its standard error to test the hypothesis requires the same assumption as this test in a simple mediation context—that the sampling distribution of the indirect effect is normal. There is just as much reason to be skeptical of this assumption in the multiple mediator context as in the simple mediation context. Bootstrapping is a useful means of relaxing this assumption, and the logic of its implementation in the multiple mediator context is the same—each indirect effect is estimated multiple times by repeatedly sampling cases with replacement from the data and estimating the model in each resample. Bias-corrected and accelerated bootstrap confidence intervals for the total and specific indirect effects are provided in the Appendix (from the SPSS macro described in Preacher and Hayes, 2007). In the section in the Appendix labeled "BOOTSTRAP RESULTS FOR INDIRECT EFFECTS," notice that the bootstrap estimates yield a different conclusion. Because zero is not in the confidence interval for perceived community (variable name "commune") and perceived interactivity (variable name "inter"), we can argue a claim that perceived customization exerts its effect on attitudes in part through perceived community as well as perceived interactivity. These bootstrap results are more trustworthy than the Sobel test because they require fewer assumptions and simulation studies demonstrate their superiority. Notice as well that we can claim an indirect effect through community even though the path linking perceived community to attitudes is not statistically different from zero (from the section labeled "Direct Effects of Mediators on DV (b paths)")—an advantage of quantifying and testing the indirect effect explicitly rather than relying on the causal steps strategy.

Some caveats must be mentioned where multiple mediators are concerned. First, a specific indirect effect should be interpreted as the indirect effect of X on Y through a given mediator *controlling for all other included mediators.* If the mediators are mutually uncorrelated, then each specific indirect effect may be interpreted as if it were a simple indirect effect. The more general (and likely) case is that mediators will be correlated, in which case each specific indirect effect represents the unique ability of each intervening variable to mediate the $X \rightarrow Y$ effect, above and beyond the other mediators. As in any linear model with correlated predictors, high correlations between the mediators can produce instability in estimates of the b paths, meaning that although each might function as a mediator considered on its own, when combined, the specific indirect effects may wash each other out through multicollinearity. Second, when multiple intervening variables are included in a model, it is difficult to tell which ones act as mediators and which, if any, act as *suppressors* (MacKinnon, Krull, & Lockwood, 2000). Interpretation should be made with care. Finally, there are other ways in which multiple mediators may be included in a single model. For example, Hyman (1955) notes that whenever an intervening variable is included in a model, the researcher may be tempted to explore potential mediators of the $X \rightarrow M$ and $M \rightarrow Y$ links. Taylor, MacKinnon, and Tein (in press) address the situation in which multiple mediators operate serially rather than in parallel, for example, $X \rightarrow M_1 \rightarrow M_2 \rightarrow Y$, but little other work has been done on this topic. Theory should guide the decision of how and whether to include multiple mediators.

MODERATED MEDIATION AND MEDIATED MODERATION

Moderated mediation can be defined as occurring when the size of an indirect effect is contingent on the level or value of a moderator variable. A moderator variable can be defined as a variable that influences or is related to the size of the relationship between two other variables. So if the relationship between X and Y varies as a function of W, then it is said that W moderates the relationship between X and Y, or that W is a moderator of the relationship. Moderation is also known as *interaction.* A process can be described as moderated mediation if the size of the indirect effect of the putative cause on the outcome through the mediator varies as a function of the moderator variable(s).

Although communication researchers routinely employ regression and analysis of variance to test hypotheses about moderation, rarely are tests of whether indirect effects vary as a function of one or more moderator variables formally conducted, even though intuition suggests that such moderated mediation is probably a fairly common phenomenon in communication processes both empirically and theoretically. One example is the differential gains hypothesis. Scheufele (2002) provides evidence that newspaper hard news use and interpersonal discussion about politics interact in influencing political knowledge, and political knowledge in turn predicts political participation. Thus, the magnitude of the indirect effect of newspaper hard news use on participation through knowledge depends on how much a person discusses politics with others. Conversely, the magnitude of the indirect effect of interpersonal discussion on participation through knowledge depends on newspaper hard news use. Slater, Hayes, and Ford (2007) provide another example in which the effect of adolescent sensation seeking on perceptions of the risks of alcohol use are mediated by attention to news about alcohol-related accidents and crime, with the magnitude of the indirect effect being contingent on both prior bad experiences with alcohol and exposure to network news.

Early literature on the subject addressed moderated mediation using an extension of the causal steps strategy (Baron & Kenny, 1986; James & Brett, 1984). For example, James and Brett (1984) considered models involving regression equations requiring "the addition of a moderator for either the $\hat{m} = f(x)$ or $\hat{y} = f(m)$ relations, or both." Moderated mediation is also addressed by Edwards and Lambert (2007), Wegener and Fabrigar (2000), Morgan-Lopez and MacKinnon (2006), Rose, Holmbeck, Coakley, and Franks (2004), and Muller, Judd, and Yzerbyt (2005), but most employ inconsistent definitions of moderated mediation. Preacher et al. (2007) address the problem by considering *conditional indirect effects,* which they define as indirect effects conditional on the values of at least one moderator. Their general strategy can be used to address all previously offered definitions of moderated mediation. They consider several basic models in which it would be sensible to examine conditional indirect effects:

1. The independent variable (X) functions as a moderator of the b path.
2. Some fourth variable (W) affects the a path.
3. W affects the b path.
4. W affects a while yet another variable (Z) affects b.
5. W affects both a and b.

These five models are depicted graphically in Figure 2.3, allowing for the possibility that X may still have a direct effect on Y. For each of these basic model forms, Preacher et al. (2007) provide product of coefficients and bootstrapping strategies (and software) for investigating the significance of the $X \rightarrow M \rightarrow Y$ indirect effect at conditional values of the moderator(s). This strategy is a direct extension of strategies used to probe significant interaction effects (e.g., Aiken & West, 1991; Muller et al., 2005; Tein, Sandler, MacKinnon, & Wolchik, 2004), and a recent example of its use in the communication literature can be found in Slater et al. (in press). Alternatively, the researcher may obtain the values of the moderator for which the indirect effect is statistically significant, an extension of the Johnson-Neyman technique (Johnson & Neyman, 1936; Rogosa, 1980, 1981). This strategy does not require the researcher to select arbitrary conditional values of the moderator at which to investigate the significance of the indirect effect.

Moderated mediation is easily confused with *mediated moderation,* a related but different process. Whereas moderated mediation relates to the moderation of the size of an indirect effect, mediated moderation occurs when the interactive effect of two variables on the outcome variable is carried indirectly through a mediator. Revisiting the differential gains hypothesis, Scheufele (2002) did include a test of mediated moderation using the causal steps strategy. He showed that the interaction between newspaper hard news use and interpersonal discussion in predicting political participation, the outcome variable, was in part indirect through their interactive influence on political knowledge, the mediating variable.

As researchers elaborate on theories to include interaction effects, models incorporating both mediation and moderation are expected to increase in frequency. Much work remains to be done. Good conceptual and statistical overviews of both moderated mediation and mediated moderation can be found in Edwards and Lambert (2007), Muller et al. (2005), and Preacher et al. (2007).

MEDIATION IN MULTILEVEL MODELS

With the increasing popularity of multilevel models (see Chapter 8 in this volume, as well as the October 2006 issue of *Human Communication Research*), attention has turned to assessing mediation in contexts involving hierarchical data. When data are organized hierarchically, multilevel regression is a more appropriate strategy than OLS regression. In such data, cases (Level-1 units) are said to be nested within clusters (Level-2 units). Common examples of nested data include employees (Level 1) nested within organizations (Level 2) and repeated measures nested within individuals.

In multilevel models, the familiar regression coefficients relating Level-1 variables may themselves vary across Level-2 units. It might at first seem reasonable to apply the same methods used for assessing mediation in OLS regression in the case of multilevel regression. However, when the regression equations for mediation are framed as multilevel regressions,

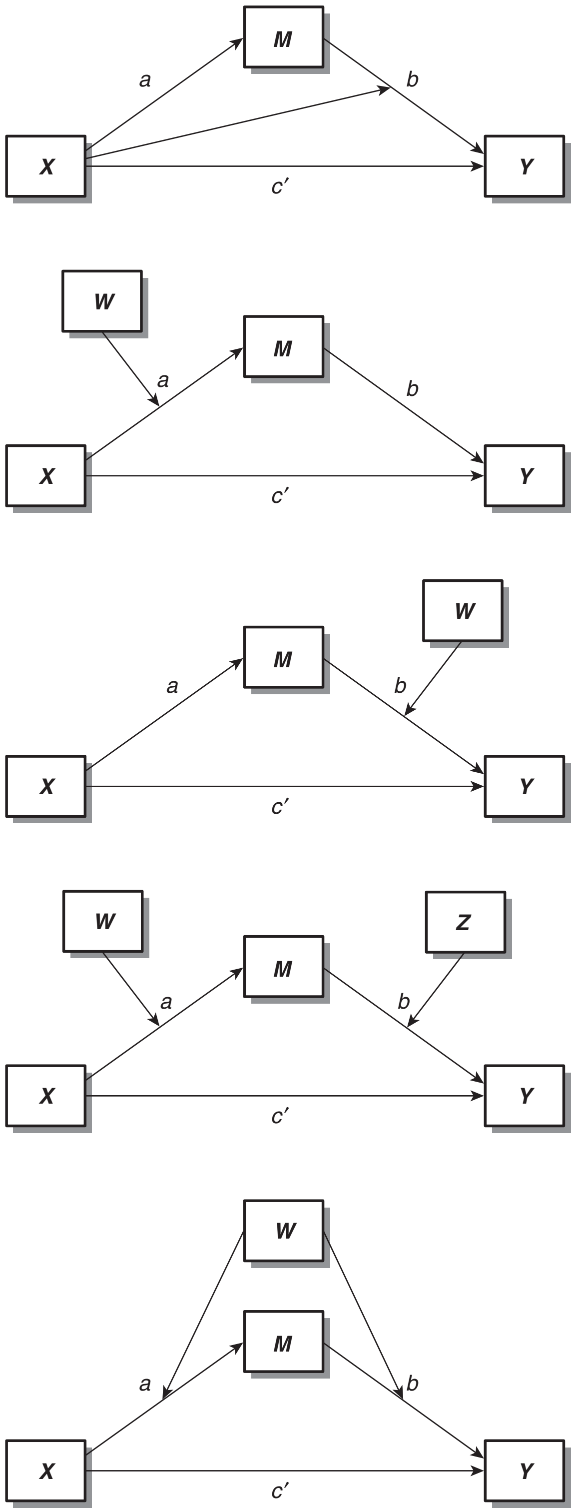

Figure 2.3 Some Moderated Mediation Models

some difficulties emerge. For example, *ab* no longer necessarily equals $c - c'$ in a given analysis, although on average, they tend to be the same and the discrepancy disappears as Level-1 and Level-2 sample sizes increase (Krull & MacKinnon, 1999). In addition, having random slope coefficients implies that mediation may be stronger for some Level-2 units than for others, so applying traditional methods in the multilevel case may misrepresent large portions of the sample.

Work investigating mediation processes in multilevel designs has only just begun and is an active area of research. Raudenbush and Sampson (1999) provide a method of examining mediation in multilevel models when *X* and *M* are Level-2 predictors and *Y* is a Level-1 outcome. Krull and MacKinnon (1999) examined the case in which *X* is a Level-2 predictor and both *M* and *Y* are Level-1 outcomes (a 2–1–1 model). They recommend that the first-order standard error of the indirect effect derived for use in single-level regression can still be used in the multilevel context. Krull and MacKinnon (2001) and Pituch, Stapleton, and Kang (2006) expanded on this work by investigating the use of single-level techniques in situations where *X*, both *X* and *M*, and neither *X* nor *M* are measured at Level 2 (2–1–1, 2–2–1, and 1–1–1 models, respectively), with random intercepts but no random slopes. Kenny, Korchmaros, and Bolger (2003) and Bauer, Preacher, and Gil (2006) investigated mediation in multilevel models where all variables are measured at Level 1 and all relevant slopes are random, whereas Pituch et al. (2005) investigated Level-1 mediation when slopes are fixed. An important point emerging from this literature is that it is desirable to assess not only the mean indirect effect characterizing a sample but also the variability in indirect effects across Level-2 units. Explaining variability in slopes across Level-2 units permits a new way to test moderated mediation hypotheses (Bauer et al., 2006; Kenny et al., 1998).

Controversies, Questions, and Miscellaneous Issues

This section includes brief discussions of concerns we commonly hear from researchers engaged in testing mediation hypotheses.

MEDIATION AND CAUSALITY

We have used the word “causal” with some regularity in this chapter. It cannot be stressed enough that mediation is a causal process, so any investigation of mediation should ensure that necessary preconditions for causality have been met. This is especially true given that hypotheses of mediation are usually tested with correlational data. Regardless of the strategy used to assess the strength and significance of mediation, no statistics can establish whether or not an effect is causal. Necessary preconditions for causal inference include temporal precedence (causes must occur before their presumed effects), concomitant variation (the variables covary in some expected pattern), and the elimination of spurious covariation (other potential causes of covariation have been eliminated).

Establishing the conditions necessary for making claims of causality is an issue of research design more than of statistical inference. For example, whereas measuring *M* before *Y* does not ensure that changes in *M* lead to changes in *Y*, it certainly makes the inference of causality more tenable (on the other hand, Cole and Maxwell [2003] point out that even when variables are *measured* in the proper order, that does not ensure that the constructs *occur* in the proper order). Similarly, causal inferences may be made with more confidence when *X* is experimentally manipulated than when *X* is merely observed. It is also frequently wise to include covariates to help eliminate likely sources of spurious correlation between *M* and *Y*, and to avoid situations in which shared method variance may spuriously inflate the regression weights used to assess mediation (Kenny et al., 1998). Often, however, mediation tests are based on correlational data, so frequently the best that can be claimed is that the data are consistent with (or do not contradict) the hypothesis of mediation. Hoyle and Robinson (2004) and Cole and Maxwell (2003) discuss means by which designs may be improved so that causality can be more confidently assumed. In addition, under some circumstances, the mediator may be experimentally manipulated in order to better establish the causal relationship between *M* and *Y* (Aron & Monin, 2005; Hoyle & Robinson, 2004; Spencer, Zanna, & Fong, 2005), but this strategy introduces complications. For example, the putative mediator must be amenable to both measurement and manipulation in order to use this *experimental-causal-chain* strategy (Spencer et al., 2005).

EFFECT SIZE

The methods presented so far address the statistical significance or precision of the estimate of the indirect effect. However, it is almost always the case that the researcher will also want to characterize an indirect effect in terms of both statistical and *practical* significance. A common way to express practical significance is in terms of *effect size* as a sort of objective gauge of the importance of an effect (Wilkinson et al., 1999). There are many measures of unstandardized and standardized effect size that can be employed in various analyses (e.g., R^2 or the squared semipartial correlation in regression, and η^2 or ω^2 in ANOVA; see, e.g., Hayes, 2005). Standardized effect-size measures have the advantage that they do not rely on the scales of the variables involved, and thus can be interpreted without knowledge of those scales. Unstandardized effect-size measures remain interpretable only in units of the variables' original scales, which may be an advantage in many circumstances.

A few methods exist for quantifying effect size in mediation analysis. MacKinnon and Dwyer (1993; see also Alwin & Hauser, 1975; MacKinnon, 1994) and Sobel (1982) propose the proportion of the total effect that is mediated, calculated as $ab/(ab + c')$ or ab/c, as a measure of the extent to which the $X \rightarrow Y$ effect is mediated.[8] MacKinnon and Dwyer (1993) and Sobel (1982) also propose the ratio of the indirect to the direct effect, or ab/c'. Sobel (1982) shows how asymptotic standard errors for these indices may be derived. Our stance is that these methods provide useful heuristics but suffer from some limitations. First, ab/c does not constitute a proper proportion, as it is not necessarily bounded by 0 and 1. Second, both measures can give misleading estimates of the magnitude and importance of an effect. For example, if the total effect c is very small, then even trivial indirect effects may appear to be very large or important. The reverse is also possible. Finally, point estimates of these measures have been found to be unstable unless the sample size is at least 500 (and in some cases more than 5,000; MacKinnon et al., 1995).

An alternative method of quantifying practical significance for indirect effects might be to simply interpret the point estimate in substantive terms. Products of slopes can be interpreted in much the same way as slopes themselves. Consider the equations for slopes a and b in a simple mediation model:

$$a = r_{XM} \frac{SD_M}{SD_X}$$

$$b = \frac{r_{MY} - r_{XY} r_{XM}}{1 - r_{XM}^2} \left(\frac{SD_Y}{SD_M} \right)$$

where SD is the standard deviation of the variable subscripted. When a and b are multiplied to form the indirect effect ab, the SD_M terms cancel, leaving:

$$ab = r_{XM} \frac{r_{MY} - r_{XY} r_{XM}}{1 - r_{XM}^2} \left(\frac{SD_Y}{SD_X} \right)$$

or more simply:

$$ab = \tilde{b}_{MX} \tilde{b}_{YM.X} \frac{SD_Y}{SD_X}$$

where $\tilde{b}_{MX}$ is the standardized regression weight estimating M from X and $\tilde{b}_{YM.X}$ is the standardized regression weight estimating Y from M controlling for X. Note that in this expression, the indirect effect is devoid of the metric of M. Thus, ab can be interpreted as the expected change in Y per unit change in X that occurs indirectly through M.

Because the practical significance of the indirect effect should not depend on the metrics of the variables involved, consider the special case in which X and Y (although not necessarily M) have been standardized, in which case the ratio of standard deviations drops out. We propose the following *index of mediation* as a rough measure of effect size:

$$ab' = \tilde{b}_{MX}\,\tilde{b}_{YM.X}$$

This index is identical to the standardized indirect effect proposed by Bobko and Rieck (1980). Note that ab' is standardized in the sense that ab' does not depend on the scales of the variables involved. Also note that whereas the proportion and ratio measures of effect size may indicate that an indirect effect is large even when the total effect is small, ab' would indicate a small effect, commensurate with intuition. A *very* rough rule of thumb for interpreting ab' is to compare it to the product of the correlation relating X to M and the semipartial correlation relating M to Y that would be considered meaningful. For example, if both $r_{MX} = 0.2$ and $r_{YM.X} = 0.3$ are considered "small" in a particular research area, then ab' values of about 0.06 might reflect a small effect. We urge caution, however, because there are many values that $\tilde{b}_{MX}$ and $\tilde{b}_{YM.X}$ might assume that would yield the same index of mediation. The ab' index would perhaps need to be modified in more elaborate models. Quantifying effect size for mediation effects would be a fruitful avenue for future research.

STATISTICAL POWER

As with most inferential statistical techniques, power (the probability of finding a given nonzero effect statistically significant) is of concern in mediation analysis. If an indirect effect exists, we would like to identify it. The causal steps strategy has been found consistently to suffer from low power relative to the alternatives discussed here. This criticism can also be leveled at the product of coefficients approach, partly as a consequence of violating the assumption of normality. However, the product of coefficients strategy tends to have higher power than the causal steps strategy (MacKinnon et al., 2002; Pituch et al., 2005). Distribution of the product strategies have been found to have superior power and Type I error rates when compared to virtually all other methods for assessing mediation (MacKinnon et al., 2002; Pituch et al., 2005), with the possible exception of bootstrapping, which also performs very well (MacKinnon et al., 2004). Most methods of assessing mediation, however, are characterized by Type I error rates that are below nominal levels when both a and b are zero in the population, but can be too small or too large when either a or b is nonzero (MacKinnon et al., 2004; Pituch et al., 2006).

Judd and Kenny (1981) note that a large *a* path is associated with mediation, yet a strong association between *X* and *M* also implies some degree of collinearity, which in turn may increase the standard error of *b*, compromising power for any test of mediation. Measurement error can also reduce power (Judd & Kenny, 1981). Hoyle and Kenny (1999) investigated the power of the product of coefficients strategy in simple mediation models as a function of the reliability of the mediator. They found that even modest unreliability can have drastic consequences for statistical power, especially in small samples (below 200 or so). They recommend that the sample size be at least 100 to achieve adequate power for detecting mediation with a highly reliable mediator, and that the sample size be at least 200 if the mediator has less than optimal reliability. Stone and Sobel (1990) found the product of coefficients strategy to work well with sample sizes as small as 200.

Regardless of the method used to assess mediation, steps often can be taken to increase statistical power. For example, representing *X*, *M*, and/or *Y* as latent variables with multiple indicators may improve power and reduce parameter bias (Kenny et al., 1998), as can judicious inclusion of covariates.

DISTAL VERSUS PROXIMAL MEDIATORS

Mediators that are causally "nearer" to the independent variable than to the dependent variable are called *proximal mediators*, whereas those that are measured very close to the dependent variable are termed *distal mediators*. Proximal and distal may, but will not necessarily, correspond closely to time of measurement. Common examples of the former are manipulation checks, which, if the manipulation is good, are essentially determinate functions of *X*. Mediators that are "too" proximal or distal may inflate *a* or *b* beyond realistic levels, compromising generalizability (Kenny et al., 1998; Kraemer, Wilson, Fairburn, & Agras, 2002). More generally, the time that elapses between measurement of *X*, *M*, and *Y* may have powerful effects on tests of mediation hypotheses. For example, if *X* is expected to cause immediate effects on *M*, then it may be important to measure *X* and *M* in close succession, whereas if *M* is expected to exert its effect on *Y* over time, a longer lag would be appropriate. Identifying the appropriate lag may itself be a considerable research undertaking. Investigators should keep in mind that the generalizability of conclusions drawn about mediation may be quite limited unless careful attention is paid to the time intervals separating measurements (see Cole & Maxwell, 2003; Shrout & Bolger, 2002).

SHOULD THE TOTAL EFFECT BE SIGNIFICANT BEFORE ASKING ABOUT MEDIATION?

Many researchers (e.g., Frazier, Tix, & Barron, 2004; Hyman, 1955; Judd & Kenny, 1981) state that the $X \rightarrow Y$ effect should be significant prior to testing an indirect effect; that is, there first ought to exist an effect for the mediator to explain or the question of whether or not M is a mediator becomes moot. This recommendation is implicit in the Baron and Kenny (1986) criteria. However, others (Kenny et al., 1998) argue that this requirement is not necessary. It is possible for an indirect effect to be statistically significant in the absence of a significant $X \rightarrow Y$ relationship (Sobel, 1986), leading many to consider mediation as a special case of an indirect effect that accompanies a significant $X \rightarrow Y$ relationship. The debate is largely semantic. We urge researchers to consider the predictions of theory and to frame hypotheses accordingly.

There are other situations in which it makes sense to investigate indirect effects in the presence of a nonsignificant $X \rightarrow Y$ effect. In models involving multiple mediators, for example, the indirect effects of two variables may have opposite signs and "cancel out," leading to situations in which there is a negligible direct effect both before and after adding mediators, yet an interesting pattern of large and significant indirect effects. In this situation, one of the intervening variables may act as a *suppressor* and the other as a mediator (Collins, Graham, & Flaherty, 1998; Frazier et al., 2004; MacKinnon, 2000; MacKinnon et al., 2000; Sheets & Braver, 1999; Shrout & Bolger, 2002).

SHOULD WE USE ORDINARY LEAST SQUARES REGRESSION OR STRUCTURAL EQUATION MODELING?

Throughout this chapter, we have assumed that the regression coefficients a and b have been estimated via ordinary least squares (OLS) regression analyses. But there are other ways to obtain these coefficients. In particular, multiple regression may be seen as a special case of path analysis, which in turn is a special application of structural equation modeling (SEM) with no latent variables. OLS regression is usually adequate for conducting a mediation analysis. However, there are some advantages to using SEM. For example, the $X \rightarrow M \rightarrow Y$ simple mediation model may comprise a small part of a larger network of relationships hypothesized to exist among variables. Mediation hypotheses can be assessed in the context of these larger models. Second, in SEM, models can contain a mix of measured and latent variables. Using latent variables with multiple measured indicators can improve the power and validity of a model by dealing effectively with measurement error. Third, as we mentioned earlier, parameter constraints may be added in SEM, permitting the comparison of nested models. Fourth, SEM software often permits the user to choose from among several estimation methods, including OLS, maximum likelihood (ML), generalized least squares (GLS), asymptotically distribution-free (ADF) methods, and others. Different assumptions must be satisfied for the various estimation methods, but mediation may be assessed using any of them. Furthermore, several SEM software applications conduct a product of coefficients test for total indirect effects upon request even for very complex indirect effects. On the other hand, it should be noted that SEM is a large-sample technique. Hoyle and Kenny (1999) found that using SEM to investigate simple mediation in cases where the mediator is specified as a latent variable can be problematic if the sample size is less than 100 or so. Finally, Cheung (2007) describes an elegant and very general method for using SEM to test a variety of mediation effects. We urge the reader to consult an introductory SEM text to learn more (Bollen, 1989; Loehlin, 1998; Maruyama, 1998).

PARTIAL VERSUS COMPLETE MEDIATION

If c' is smaller than c but c' is different from zero, it is sometimes said that the mediator *partially mediates* the effect of X on Y (Judd & Kenny, 1981), or that the evidence is consistent with *partial mediation.* If c' is statistically indistinguishable from zero, *complete* or *full mediation* is said to have occurred (Baron & Kenny, 1986; James & Brett, 1984; Kenny et al., 1998). We regard these coarse designations of effect size as having limited utility, depending as they do on the size of the total effect and on sample size. Using popular criteria, *complete* mediation should occur most often when the total effect is negligible (but statistically significant) and when the sample size is small. Much larger and potentially more important effects may be characterized as partial in larger samples even if they would ordinarily be considered large effects in an absolute sense. Furthermore, the conclusion of complete mediation may quell future research into other possible mediators (Pituch et al., 2005). The recognition that all indirect effects are partial may serve as a cue that other mediators can always be considered or may lead to the hypothesis that mediation is stronger for one group than for another (Shrout & Bolger, 2002). Because the terms *partial* and *complete* denote practical significance but are most often defined in terms of statistical significance, we urge researchers to abandon these terms altogether. Researchers are encouraged to focus instead on clearly distinguishing statistical and practical significance and to consider reporting effect size assessed by means discussed earlier.

THE ROLE OF THEORY

Mediation models are confirmatory models rather than exploratory ones. In other words, it is not appropriate to try all possible assignments of X, M, and Y to roles as independent, dependent, and mediator variables and see what turns out to be significant. The framework assumes that the causal ordering of the variables is known or at least strongly rooted in theory. Given that ordering, the methods described here are useful for helping the researcher decide whether and to what extent the data are *consistent* with mediation (mediation, and indeed any other scientific hypothesis, can never be definitively proved). When the appropriate ordering of variables is not known, theory should be used to determine the proper order (Hoyle & Robinson, 2004).

COMPUTER SOFTWARE

We are somewhat reluctant to address the issue of software for two reasons. First, software changes quickly, and many of our comments on existing software applications may no longer be relevant in the near future. Second, methods used to assess indirect effects are logically independent of the software designed to implement them, and some can be understood and applied without specialized software. Nevertheless, software applications greatly ease the burden of computation and lower the probability of committing errors of calculation. Following is a discussion of software applications that are, at the time of this writing, capable of addressing indirect effects.

Stone (1985) provided a Fortran program (CINDESE) for computing standard errors of indirect effects. MacKinnon and Wang (1989a, 1989b) and J. Scott Long (in Sobel, 1988) provide SAS/IML code for conducting tests of indirect effects using output from SEM software. Similar code is now incorporated in LISREL (Jöreskog & Sörbom, 1996), EQS (Bentler, 1997), AMOS (Arbuckle, 1999), and Mplus (Muthén & Muthén, 2004). Tests of indirect effects in these applications are limited to tests of total indirect effects, the exception being Mplus (which can also conduct tests of specific indirect effects), but all of them can handle models with multiple mediators in complicated configurations and allow for control variables. Preacher and Leonardelli (2001) authored a JavaScript Web page that provides tests of indirect effects in single-mediator models using first-order, second-order, and bias-corrected variances. Preacher and Hayes (2004) provide SPSS and SAS code for conducting the causal steps approach as well as the Sobel test. Similarly, Dudley, Benuzillo, and Carrico (2004) describe SPSS and SAS macros (Dudley & Benuzillo, 2002) that perform tests of the indirect effect in single-mediator models. Their macros also provide two measures of effect size, the percentage of the total effect that is mediated and the ratio of the indirect effect to the direct effect. MacKinnon, Fritz, Williams, and Lockwood (in press) provide SPSS, SAS, and R macros for generating confidence intervals using the distribution of products method.

Resampling approaches to assessing indirect effects are somewhat newer. Lockwood and MacKinnon (1998) provide an SAS macro (BOOTME) to bootstrap confidence intervals in single-mediator models. Their code also provides a confidence interval for the indirect effect using the first-order variance and has been recently updated (MacKinnon et al., 2004). Shrout and Bolger (2002) provide EQS and SPSS syntax for bootstrapping confidence intervals of indirect effects, as well as instructions for using AMOS (Arbuckle, 1999) for the same purpose. Preacher and Hayes (2004) provide SPSS and SAS macros that bootstrap confidence intervals and provide normal-theory results for the product of coefficients method. SAS code provided by Morgan-Lopez (2003) conducts a test of the indirect effect using the first-order variance and provides asymmetric confidence intervals using the bootstrap and bias-corrected bootstrap methods (and, assuming one has the Meeker et al. [1981], tables in the proper format, constructs intervals from the distribution of the product). Specialized SAS and SPSS macros also exist to bootstrap confidence intervals for total and specific indirect effects in multiple mediator models with and without statistical controls (Preacher & Hayes, 2007) and for conditional indirect effects in moderated mediation models (Preacher et al., 2007). SAS code for investigating mediation in multilevel models is provided by Bauer et al. (2006) and Kenny et al. (2003). Cheung (2007) provides LISREL, Mplus, and Mx code for testing a variety of mediation hypotheses in SEM using normal-theory methods, bootstrapping, and asymmetric likelihood-based confidence intervals.

Concluding Remarks

In this chapter, we have considered and evaluated strategies that can be used to address mediation hypotheses in communication research. We considered causal steps, correlational, difference in coefficients, nested model, product of coefficients, distribution of the product, and bootstrapping strategies for estimating and testing indirect effects. We have also discussed several topics relevant to indirect effects, including reporting effect size and considering statistical power. We covered several useful extensions of the basic mediation model that are receiving increasing attention in the methodological literature, including mediated moderation, moderated mediation, mediation in multilevel data structures, and models with multiple mediators. We briefly touched on issues concerning causality and several other points of contention and confusion. Finally, we discussed software implementation. Throughout, we occasionally illustrated key points with a running example.

No mediation model is ever correct, for the simple reason that no model is ever correct, period. Models serve as approximations to processes and should not be expected to precisely mirror the underlying processes giving rise to observed data (MacCallum, 2003). Models are merely parsimonious metaphors to reality created for the purpose of testing and comparing ideas, so it is arguably meaningless to ask whether a model is "correct." We do not intend to imply that investigating mediation is a pointless undertaking, merely that the researcher should keep in mind that models are simply tools to clarify our understanding of phenomena and that some models are better tools than others. Mediation models may be incorrect for a variety of reasons, including reasons of causal misspecification, confounds, and omitted variables that threaten any research enterprise (Judd & Kenny, 1981). In addition, mediation models carry with them a host of often untested assumptions. The inference procedures may involve assumptions of normality, heteroscedasticity, and independence of regression residuals. We assume that our samples are representative of the population toward which inference and generalization are desired. We usually assume that a linear model conveys all the information useful for making conclusions about mediation. Strictly speaking, none of these assumptions is ever exactly met in practice, but steps can be taken to minimize the impact of violating these assumptions in specific applications. By the same token, no mediation model is ever complete, in the sense that yet more mediators may always be introduced to explain any direct effect in a mediation model. Again, we say this not as a deterrent to investigating mediation but rather to suggest that introducing more proximal mediators may help the researcher better understand the process under scrutiny (see Hyman, 1955, pp. 325–327).

RECOMMENDED READING

We have attempted to provide an overview of many (but definitely not all) of the issues involved in assessing mediation effects in communication research. We did not go into depth on these topics, and we avoided the underlying mathematics. However, we provided a number of relevant citations under each heading; the interested reader is urged to consult them for more detail. For good overviews of mediation analysis we recommend Baron and Kenny (1986), Frazier et al. (2004), Judd and Kenny (1981), MacKinnon et al. (2002), MacKinnon, Fairchild, and Fritz (2007), Mallinckrodt, Abraham, Wei, and Russell (2006), and Shrout and Bolger (2002). For issues of design, we recommend Cole and Maxwell (2003) and Hoyle and Robinson (2004). An old and extensive literature exists on quantifying indirect effects, using the multivariate delta method, and computing the variances of products of random variables. For readers who wish to delve more into the quantitative aspects surrounding mediation analysis, we recommend consulting MacKinnon et al. (1995), Sobel (1982, 1986), Bollen (1987), Preacher and Hayes (2006), and Preacher et al. (2007), as well as sources cited therein.

Notes

1. We use the terms *mediation* and *indirect effect* interchangeably here. Holmbeck (1997, p. 603) points out, however, that the two should be disentangled. Cole and Maxwell (2003, p. 558) usefully define an *indirect effect* as “the

degree to which a change in an exogenous variable produces a change in an endogenous variable by means of an intervening variable."

2. In their article, Kalyanaraman and Sundar (2006) proposed two additional mediators—*perceived involvement* and *perceived relevance*. We excluded these mediators in this example because, in our judgment, these constructs overlap with the manipulation in such a way that it could be argued that their experimental manipulation of customization also manipulated involvement and relevance.

3. In rare cases, it is even possible to observe c (nonsignificant) > c' (significant) or c (significant) < c' (nonsignificant).

4. Model B is said to be nested in Model A if its free parameters are a subset of those in Model A. Any model in which parameters are constrained to zero will fit worse than a model with fewer or no zero-constraints on the same set of parameters.

5. Later, we argue that the distinction between complete and partial mediation is not a useful distinction to make.

6. To our knowledge, Hyman (1955, p. 284) was the first to suggest quantifying mediation by multiplying a and b paths: ". . . the original relationship is seen to be the result of the marginal terms—the product of the relationships between the test factor and each of the original variables. Symbolically, $[xy] = 0 + 0 + [xt][ty]$," where the 0s represent partial or conditional relationships (assumed constant in the simple mediation context) and xt and ty represent the a and b paths, respectively.

7. The standard error will be the square root of this asymptotic variance estimate.

8. MacKinnon and Dwyer (1993) present this measure as a percentage rather than as a proportion.

Appendix

Output from an SPSS macro for conducting a multiple mediation analysis (Preacher & Hayes, 2007). The macro, also available for SAS, can be downloaded from http://www.comm.ohio-state.edu/ahayes/.

```
indirect y = attitude/x = custom/m = inter novel commune/
  contrast = 1/normal = 1/boot = 5000.

Run MATRIX procedure:

Dependent, Independent, and Proposed Mediator Variables:
DV    = attitude
IV    = custom
MEDS  = inter
        novel
        commune

Sample size
         60
```

```
IV to Mediators (a paths)
                      Coeff      se        t       p
  inter               .4013   .0778   5.1592   .0000
  novel               .6446   .0704   9.1555   .0000
  commune             .5829   .0604   9.6462   .0000

Direct Effects of Mediators on DV (b paths)
                      Coeff      se        t       p
  inter               .2535   .0924   2.7447   .0082
  novel               .1700   .1019   1.6688   .1008
  commune             .1699   .1154   1.4721   .1467

Total Effect of IV on DV (c path)
                      Coeff      se        t       p
  custom              .5119   .0588   8.7138   .0000

Direct Effect of IV on DV (c' path)
                      Coeff      se        t       p
  custom              .2016   .1061   1.8993   .0628

Fit Statistics for DV Model
    R-sq    Adj R-sq       F      df1      df2      p
   .6666       .6424  27.4948   4.0000  55.0000  .0000

****************************************************

      NORMAL THEORY TESTS FOR INDIRECT EFFECTS

Indirect Effects of IV on DV through Proposed Mediators
(ab paths)
                Effect      se        Z       p
  TOTAL          .3104   .0931   3.3330   .0009
  inter          .1018   .0408   2.4948   .0126
  novel          .1096   .0645   1.6989   .0893
  commune        .0990   .0657   1.5063   .1320
  C1            -.0078   .0830   -.0944   .9248
  C2             .0027   .0793    .0346   .9724
  C3             .0106   .0933    .1134   .9097
****************************************************

       BOOTSTRAP RESULTS FOR INDIRECT EFFECTS

Indirect Effects of IV on DV through Proposed Mediators
(ab paths)

                  Data     Boot     Bias      SE
  TOTAL          .3104    .3099   -.0005   .0996
  inter          .1018    .1012   -.0005   .0474
  novel          .1096    .1123    .0027   .0594
  commune        .0990    .0963   -.0027   .0571
  C1            -.0078   -.0111   -.0033   .0736
  C2             .0027    .0049    .0021   .0707
  C3             .0106    .0160    .0054   .0822
```

```
Bias  Corrected  and  Accelerated  Confidence  Intervals
                 Lower   Upper
  TOTAL           .1164   .5069
  inter           .0286   .2197
  novel          -.0050   .2303
  commune         .0015   .2311
  C1             -.1487   .1444
  C2             -.1348   .1479
  C3             -.1693   .1626
****************************************************

Level of Confidence for Confidence Intervals:

  95

Number of Bootstrap Resamples:
5000
****************************************************

 INDIRECT EFFECT CONTRAST DEFINITIONS: Ind_Eff1 MINUS
                       Ind_Eff2

  Contrast    IndEff_1    IndEff_2
  C1             inter       novel
  C2             inter     commune
  C3             novel     commune
—— END MATRIX ——-
```

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