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Mediation and the Estimation of Indirect Effects in Political Communication Research

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Much research in the communication and political science fields—political communication research being no exception—seeks to establish the extent to which certain communication-related variables (e.g., exposure to political debates, the tone of news coverage about politicians, or the diversity in opinions of those with whom one talks about politics) have effects on various political outcomes (e.g., evaluation of candidates running for office, political cynicism, or participation in the political process). To be sure, studies to establish or refute the existence of effects are important. But studies, and the investigators who conduct them, are usually much more impressive and helpful in advancing our understanding when they go further by establishing not only *whether or not* an effect exists or *how large* that effect is, but also *why* that effect exists. What is the process at work that produces an association between political discussion and participation, or between exposure to political debates and political cynicism, or between the tone of news coverage and evaluation of candidates running for public office? For example, is it that frequent political talk exposes one to more diverse viewpoints, which in turn makes one feel confused or less confident in one's beliefs, which in turn translates into a reduced likelihood of converting those beliefs into political action? Or might frequent talk increase one's sense that one can make a difference in the outcomes of the political process, spawning greater participation in that process?

Questions about mechanisms or process invoke the concept of *mediation*, the topic of this chapter. We say that the effect of some independent variable *X* on outcome variable *Y* is *mediated by M* if *M* is causally situated between *X* and *Y* such that changes in *X* cause changes in *M*, which in turn cause changes in *Y*. The most basic mediation model one can test takes the form in Figure 23.1 panel A. In this model, *X*'s effect on *Y* occurs both *directly* (the link directly from *X* to *Y*) as well as *indirectly*, through *X*'s effect on mediator variable *M*, which then affects *Y*. Of course, other more complicated mediation models are possible and no doubt are at work in political communication processes. Figure 23.1 Panel B, for instance, depicts a model in which *X*'s effect is transmitted indirectly through multiple mediators. An extension of this model allows the mediators to affect each other, as in Figure 23.1 Panel C. Many such models can be constructed by linking variables in a sequence of causal associations together.

Quantifying and testing indirect effects is the focus of this chapter. After first briefly reviewing some recent examples of published research in political communication focused on testing questions about mediation, we describe how indirect effects are typically quantified and how inferences about their magnitudes are made. Although there are many methods that can be used to estimate the paths in these models, we focus on the use of ordinary least squares (OLS) regression and structural equation modeling (SEM), as these are the methods most likely to be familiar to the reader. Similarly, there are many methods that have been discussed in the literature for making statistical inferences about indirect effects, but we focus largely on those few that are implemented in existing computing packages, so as to reduce the computational burden on those interested in applying the material described in this chapter in their own research.

Throughout this chapter, we will use the terms "mediation" and "indirect effect" loosely and somewhat interchangeably, although there is some debate in the literature over whether or not it is legitimate to do so (Mathieu & Taylor, 2006). Implied in the definition of mediation provided above is that *X* and *Y* are associated. That is, an effect that doesn't exist (what we will call the "total effect" below) can't be said to be mediated. For this reason, it is not uncommon for investigators to ignore

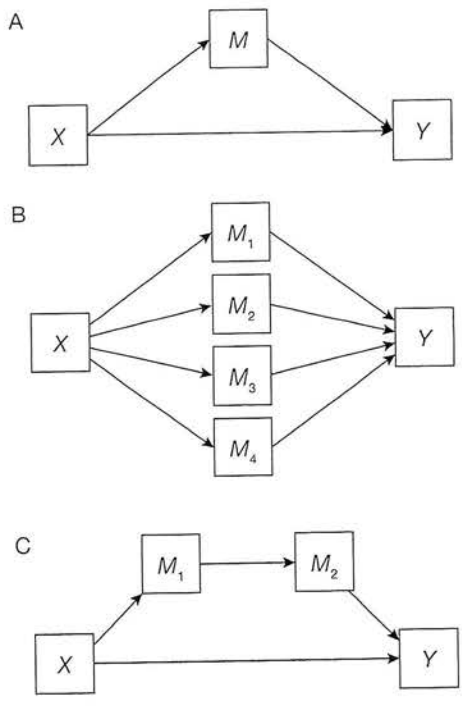

FIGURE 23.1 A few examples of mediation models.

the possibility of an indirect effect of *X* on *Y* through one or more intervening variables in the absence of an association between *X* and *Y*. This is a practice we do not endorse, for it is possible for *X* to indirectly affect *Y* in the absence of compelling evidence of an association between *X* and *Y*, especially in models that involve more than one intervening variable. Regardless, there is general agreement among experts in this area that a nonzero indirect effect in the direction consistent with the proposed mediation process is a necessary condition for a claim of mediation. For this reason, understanding how to quantify and test indirect effects is important for researchers interested in testing mediation hypotheses. Mathieu and Taylor (2006, pp. 1037–1039) dwell at some length on the distinction between an indirect effect and mediation, and their discussion is worth reading.

Before beginning, we want to emphasize that questions about mediation and indirect effects are ultimately questions about causality, and in the absence of a research design that affords a causal conclusion, confidence in one's causal inferences must necessarily be tempered. Ideally, the proposed causal agent (labeled *X* in Figure 23.1) is experimentally manipulated or, if not, can at least be known to be causally prior to its presumed effect (on *M* and *Y*). In many studies in the published literature that include tests of mediation, including the examples in this chapter, the correct temporal sequencing of cause and effect cannot be established except through theoretical or logical argument. Although it is true that there is no substitute for a good theory, causal theories cannot be definitively supported empirically unless the conditions of causality have been established by good research design. The methods we describe in this chapter are based on mathematical operations involving indices of association. But as there are many explanations for association between variables in a mediation model (such as spuriousness; cf. MacKinnon, Krull, and Lockwood, 2000), in the absence of design that allows for confident cause–effect inference, the methods described here can be used to assess only whether the evidence is consistent with a mediation process. This should come as no surprise and is no different from any statistical method, as inferences are products of the mind more than products of math. Use your analysis to inform your thinking rather than as a substitute for thinking. A good overview of the criteria for establishing causality and the sticky philosophical debates to which the concept of "cause" give rise can be found in Davis (1985) or Holland (1986).

MEDIATION IN RECENT POLITICAL COMMUNICATION RESEARCH

Political communication scholars have devoted considerable empirical attention to understanding how mass and interpersonal communication exert their effects on various political outcomes. Keum and colleagues (2004), for instance, explored how the use of news (televised and print) and exposure to various forms of televised entertainment (dramas, sitcoms, and talk shows) are linked to civic participation, such as volunteering and participating in community meetings. They tested a model in which the use of news and entertainment television affects participation through their influence on concerns and behavior pertaining to achieving social status and environmental degradation. They found that both news and entertainment media have both direct and indirect effects on participation, with increased exposure actually prompting both social status-oriented consumerism and concerns about the environment, which in turn both prompt greater participation. Relatedly, Holbert (2005a) examined the effects of television news viewing on attitudes about government spending on social programs, testing the hypothesis that the use of televised news exerts its influence on such attitudes by changing perceptions of the role of government in society. Indeed it does, although the strength of this effect differs as a function of party identification. According to Holbrook and Hill (2005), exposure to certain kinds of television shows can affect evaluations of political leaders by changing the salience or chronic accessibility of social issues pertaining to the themes portrayed in those shows. Using experimental and cross-sectional survey data, they find that exposure to crime dramas makes crime a more salient problem facing the nation, which in turn influences how the president is evaluated. In the realm of interpersonal communication, Eveland (2004) examines the mechanisms through which political discussion can increase political knowledge. His results support a mediated relationship in which interpersonal discussion about politics increases political knowledge through the cognitive elaboration of political information such discussion produces, which in turn facilitates greater knowledge acquisition.

These studies show that communication's effects on political outcomes (such as knowledge and attitudes) frequently are mediated. But communication-related variables have also been conceptualized and empirically studied as mediators of the effects of other variables. For example, Gidengil and Everitt (2003) show that the effect of the sex of a political leader on the public's evaluations of the leader occurs in part through the tone of the news coverage about politicians. Specifically, they found that journalists more frequently use negative or aggressive verbs in their coverage of female compared to male politicians, and this tonal difference contributes to differences in individuals' evaluations of those politicians, with females being perceived more negatively as a result of these differences in tone of coverage. Holbert, Shah, and Kwak (2003) report that exposure to three types of prime-time television programming (traditional dramas, progressive dramas, and situational comedies) mediates the effects of demographic variables on opinions about women's rights issues. That is, some of the differences in opinions that we see between people who differ in education, age, or sex is attributable to differences in their selection of entertainment television, which tends to portray women in certain ways. Other examples of communication as mediator include Pinkleton and Austin's (2001) study of political disaffection and Eveland's (2001) cognitive mediation model of learning from the news.

Although there are many studies in the field of political communication which place communication squarely in the role of independent or mediating variable, relatively few studies focus on communication as the ultimate outcome of interest. One notable exception is Sellers and Schaffner's (2007) analysis of media coverage of the U.S. Senate. They found that the way politicians structured events (such as press conferences) impacted the interest journalists found in the event, which then influenced the amount of coverage the event received. Therefore, news coverage of a U.S. senator was influenced by the extent to which a senator could structure an event that captured the attention and interest of journalists. Another example is Stroud's (2007) study of the determinants of political discussion. She found that an individual's attitude toward President G. W. Bush predicted whether or not they viewed the film *Fahrenheit 9/11* (communication as mediator), which in turn purportedly affected frequency of political discussion (communication as outcome).

Stroud's *Fahrenheit 9/11* study is an example of one in which communication variables play multiple roles, as both mediator (i.e., selective exposure to certain media content) and outcome (frequency of political talk). So, too, is Eveland's (2001) cognitive mediation model of learning, frequently applied to political contexts, in which motivations to use certain forms of news media (an independent variable) affect attention to news (a mediator), which in turn affects how much one learns. We would also expect there to be a feedback loop or "cycle," whereby communication (e.g., exposure to campaign advertisements) has certain effects on noncommunication variables (e.g., political cynicism) which, in turn, affect communication-related variables (e.g., interest in campaign news and selective avoidance of political advertisements). Alternatively, a communication-related outcome could function as a mediator of a communication outcome to follow, as in Holbert's (2005b) intermedia mediation model of political engagement and learning. Although such cyclical processes no doubt are at work in the world (cf. Slater, 2007), the extra empirical demands they require result in relatively few such studies in the literature. Regardless, as the above discussion makes clear, the study of mediation, mechanism, and process in one form or another is alive and well in the field of political communication.

PARSING THE TOTAL EFFECT INTO DIRECT AND INDIRECT COMPONENTS

One of the fundamental rules of path analysis is that a variable's effect, such as the effect of X on Y, can be mathematically partitioned into various components by tracing the paths between the two variables in a path diagram (as in Figure 23.1). In this section we will focus on those components of a path diagram directly pertinent to mediation analysis—the *total effect* of X on Y, the *indirect effect(s)* of X on Y through one or more mediators, and the *direct* effect of X on Y. We will illustrate the computation of these effects using data provided to us by William Eveland from an investigation of the cognitive mediation model of learning from the news (Eveland, 2001). This model proposes that people learn more through the media when they are motivated to attend to and deeply process the information in a way that facilitates learning. The data we use are from a sample of residents of Madison, Wisconsin, and nearby communities in 1985. Responses to various questions were aggregated to form indices quantifying the constructs central to the cognitive mediation model: *surveillance gratifications* (motivation to acquire information from the media about politics), *attention to news* (the extent to which respondents are exposed to and, when so exposed, focus their attention on news about national government and politics), *elaborative processing* (the extent to which respondents report thinking about things they've heard about in the news), and knowledge of recent events in politics and national and international affairs. For details on measurement, see Eveland (2001).

Simple Mediation Model

Figure 23.2 illustrates the *simple mediation model*, in which X transmits its effect on Y through a single mediator variable M. The tracing rules of path analysis tell us that the effect of X on Y in

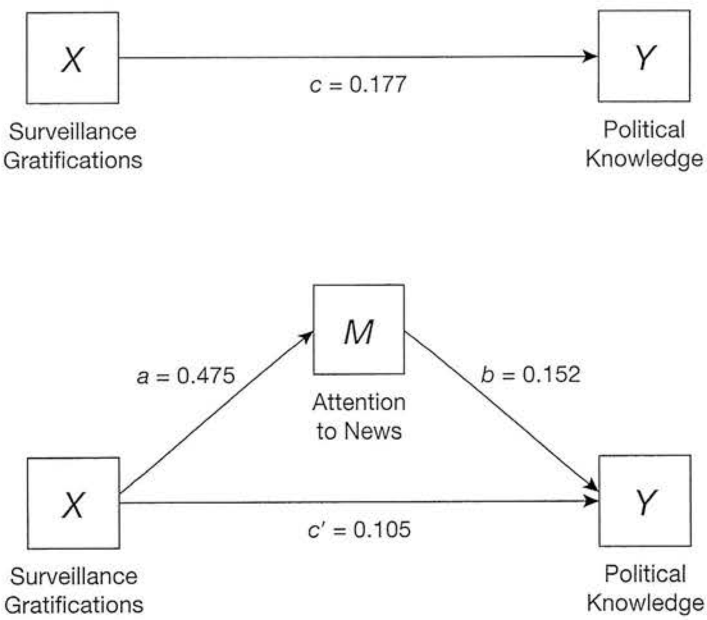

FIGURE 23.2 A simple mediation model.

this model can be partitioned into components by tracing the paths along which one can travel in the diagram to get from *X* to *Y* while never moving in a direction opposite to the direction of presumed causal flow. The direction of causal flow is denoted in a path diagram by the direction of the arrow.

The various effects in a mediation model that we discuss here can be estimated in a number of different ways. The simplest approach is through a set of OLS regression models. A more general approach that could be applied to simple models such as this as well as more complicated models would involve the use of an SEM program such as EQS, AMOS, LISREL, or Mplus, among others. An illustration of the use of SEM will be provided later. Unless we say otherwise, all effects in this chapter are estimated using OLS regression. Although any statistical software that can conduct regression could be used, we used a set of macros described by Preacher and Hayes (2004, 2008b) written for SPSS and SAS, which provide all the necessary information while simultaneously providing various inferential statistics for testing hypotheses about the indirect effects that we discuss later in this chapter.

The total effect of *X* on *Y* in any causal diagram is quantified quite simply as the regression coefficient in a model predicting *Y* from *X*, denoted in Figure 23.2 as c. This total effect in a simple mediation model can be partitioned into two components. The direct effect of *X* on *Y* is the c' path in Figure 23.2, quantified as the unstandardized regression weight for *X* in a model predicting *Y* from *both X* and *M*. It quantifies how two cases which differ by one measurement unit on *X* but which are equal on *M* (i.e., adjusting or controlling for *M*) are expected to differ on *Y*. The second component is the indirect effect of *X* on *Y* through *M*, which consists of the product of the a and b paths in Figure 23.2. The a path is the regression weight in a model estimating *M* from *X*, and the b path is the partial regression weight for *M* in a model estimating *Y* from both *X* and *M*. In a model with only observed (rather than latent) variables, the direct and indirect effects of *X* sum to produce the total effect of *X*. That is, $c = c' + ab$. Simple algebraic manipulation shows that the indirect effect is the difference between the total and direct effect, $ab = c - c'$. So the indirect effect quantifies the change in the effect of *X* on *Y* after controlling for *M*'s effect on *Y*.[1]

Using the Eveland (2001) data, we estimated the direct, indirect, and total effects of surveillance gratifications (*X*) on political knowledge (*Y*), with attention to news (*M*) as the proposed mediator. The first step is to estimate the total effect of surveillance gratifications on knowledge, derived by regressing political knowledge on surveillance gratifications. In these data, $c = 0.177$ (see Appendix 23.1 for the output from the SPSS version of the macro that we used to estimate the effects we discuss here, and Table 23.1 for a summary of these effects). So two people who differ by one unit in their surveillance gratifications are estimated to differ by 0.177 units in their political knowledge. Although these data come from a correlational design, a very liberal causal interpretation would be that if we could move people upward one unit on the gratifications scale, we would expect that their political knowledge would increase by 0.177 units on the knowledge scale.

According to the model in Figure 23.2, some of the change in political knowledge that we would expect to occur by increasing a person's surveillance gratification would occur by changing his or her attention to news, which in turn would affect how much that person learns. This is the indirect effect of gratification on knowledge through attention. But some of the effect of surveillance on knowledge is direct, occurring either without the aid of attention, or through some other mechanism not included in this simple mediation model. Just how much of this effect of *X* on *Y* is direct and how much is indirect through attention to news?

To answer this question, we must estimate the direct and indirect effects. The indirect effect of surveillance gratifications on political knowledge is estimated as the product of the effect of surveillance gratifications on attention (a) and the effect of attention on political knowledge (b).

TABLE 23.1
Path Coefficients and Indirect Effects for Three Mediation Models (Standard Errors in Parentheses)

	Path Coefficients			*Indirect Effects*			
	to Knowlege (K)	*to Attention (A)*	*to Elaboration (E)*	*Estimate*	*Sobel Z*	*Symmetric 95%CI*	*Bootstrap 95% CI*†
Model 1 (Figure 23.2)							
from Surveillance (S)	.105 (.035)	.475 (.058)					
from Attention (A)	.152 (.027)						
S→A→K				.072 (.016)	4.651	.042, .103	.042, .107†
Model 2 (Figure 23.3)							
from Surveillance (S)	.086 (.034)	.475 (.058)	.280 (.057)				
from Attention (A)	.126 (.027)						
from Elaboration (E)	.108 (.027)						
Total				.090 (.017)	5.311	.057, .123	.062, .129
Specific: S→A→K				.060 (.015)	4.064	.031, .089	.033, .094
Specific: S→E→K				.030 (.010)	3.080	.010, .050	.013, .056
Model 3 (Figure 23.4)							
from Surveillance	.087 (.034)	.475 (.058)	.167(.059)				
from Attention	.126 (.027)		.238(.046)				
from Elaboration	.108 (.027)						
Total				.090 (.017)	5.318	.057, .123	.058, .127
Specific: S→A→K				.060 (.015)	4.069	.031, .089	.030, .096
Specific: S→E→K				.018 (.008)	2.290	.002, .034	.005, .039
Specific: S→A→E→K				.012 (.004)	2.942	.004, .020	.005, .024

Total Effect = 0.177 (0.033) † Percentile CIs for Model 1 (from Preacher & Hayes, 2004) and BC CIs for models 2 (from Preacher & Hayes, 2008b) and 3 (Mplus)

The former is the regression weight estimating attention from just surveillance gratifications. Here, $a = 0.475$. The latter is the regression weight for attention in a model estimating political knowledge from both attention and surveillance gratifications. In these data, $b = 0.152$. When multiplied together, the indirect effect of surveillance gratifications on knowledge through attention is $ab = (0.475)(0.152) = 0.072$. The direct effect of surveillance gratifications comes out of the same model used to derive b, as it is the regression coefficient for surveillance gratifications controlling for attention to news. We found that $c' = 0.105$. Notice that, as promised, the total effect equals the sum of the direct and indirect effects: $0.177 = 0.105 + 0.072$.

Combining all this information, we can say that of the 0.177 unit difference in knowledge attributable to a unit difference in surveillance gratifications (the total effect), 0.072 of it is the result of the effect of gratification on attention, which in turn influences knowledge. The remaining 0.105 is direct, spurious, or attributable to other indirect effects not explicitly modeled.

An interesting question is how much of a variable's effect is due to a mediation process and how much of it is due to some other process. There is no universally agreed-upon approach to answering this question. One approach is to calculate the ratio of the indirect effect to the total effect, i.e., ab/c. In this example, this quantity is $0.072/0.177 = 0.407$. This *proportion of total effect that is mediated* measure can be interpreted to mean that 40.7% of the total effect of surveillance gratifications on political knowledge is due to its indirect effect through attention to news. An alternative proposal is to calculate the *ratio of the indirect effect to the direct effect*, i.e., ab/c'. Here, this ratio is $0.072/0.105 = 0.686$, meaning that the indirect effect through attention is about 68.6% of the size of the direct effect. Although these ratios make sense, they can be problematic in some situations. The proportion of total effect that is a mediated measure is not constrained to lie between 0 and 1 because, paradoxical as it may seem, it actually is possible for the indirect effect to be larger than the total effect or for the indirect effect and direct effect to have different signs, producing a negative proportion. The ratio of the indirect to direct effect also can be negative, or may involve division by 0 or by a very small number, which in turn produces impossible or ambiguous estimates. See Preacher and Hayes (2008a) and Hayes (2009) for a discussion of effect size measures for mediation effects.

Single-Step Multiple Mediator Model

Of course, X may and often does exert its effect indirectly on some outcome Y through multiple mediators. Figure 23.3 is an example of one type of such a model, the *single-step multiple mediator model*. Although there are several mediators in this model, no mediator causally affects another mediator, so it requires stepping through only a single mediator to get to Y (and hence the name, "single-step" model). Path c' is the direct effect of X on Y, as in the simple mediation model, defined as the regression weight for X in a model estimating Y from X and all k of the proposed mediator variables ($k = 2$ in Figure 23.3). But now there are multiple paths from X to Y through the set of mediators, each referred to as a *specific indirect effect*. The specific indirect effect of X on Y through M_i is the product of the a and b paths linking X, M_i, and Y. For example, the specific indirect effect of X on Y through M_1 is a_1b_1, where a_1 is the weight for X in a model estimating M_1 from X, and b_1 is the weight for M_1 in a model estimating Y from X and all k M variables. In a single-step multiple mediator model, the *total indirect effect* of X on Y is the sum of the k specific indirect effects:

$$\text{total indirect effect of } X \text{ on } Y = \sum_{i=1}^{k} a_i b_i \tag{1}$$

As in the simple mediation model, the total effect, c, is the sum of the direct and indirect effects:

$$c = c' + \sum_{i=1}^{k} a_i b_i \qquad (2)$$

Simple algebra shows that the difference between the total and direct effect of X on Y, $c - c'$, is equal to the total indirect effect.

To illustrate, we extend the earlier example examining the direct and indirect effects of surveillance gratifications on political knowledge by adding an additional proposed mediator from the cognitive mediation model—elaboration—while retaining attention to news as a mediator variable. Here we use an SPSS macro described by Preacher and Hayes (2008b) to estimate the effects (see Appendix 23.2 for the output), although a series of OLS regressions could be conducted using any statistical program capable of doing regression, as could an SEM program such as Mplus, AMOS, EQS, or LISREL. But not all programs will produce the inferential tests we prefer, described in the next section.

The total effect is unchanged by the inclusion of additional mediators in the model. It remains $c = 0.177$. But now there are two indirect effects through which surveillance gratifications is proposed to exert its effect on political knowledge—the specific indirect effect through attention, and the specific indirect effect through elaboration. Arbitrarily labeling attention to news M_1 and elaboration M_2, to estimate these specific indirect effects we estimate a_1 and a_2, the paths from surveillance gratifications to attention and elaboration, respectively. The first path, a_1, is the same as the estimate from the simple mediation model, as this is still derived from a regression of attention on surveillance gratifications: $a_1 = 0.475$. The path from surveillance to elaboration is estimated by regressing elaboration on only surveillance gratifications: $a_2 = 0.280$. The b_1 and b_2 paths are derived from a model simultaneously predicting political knowledge from attention,

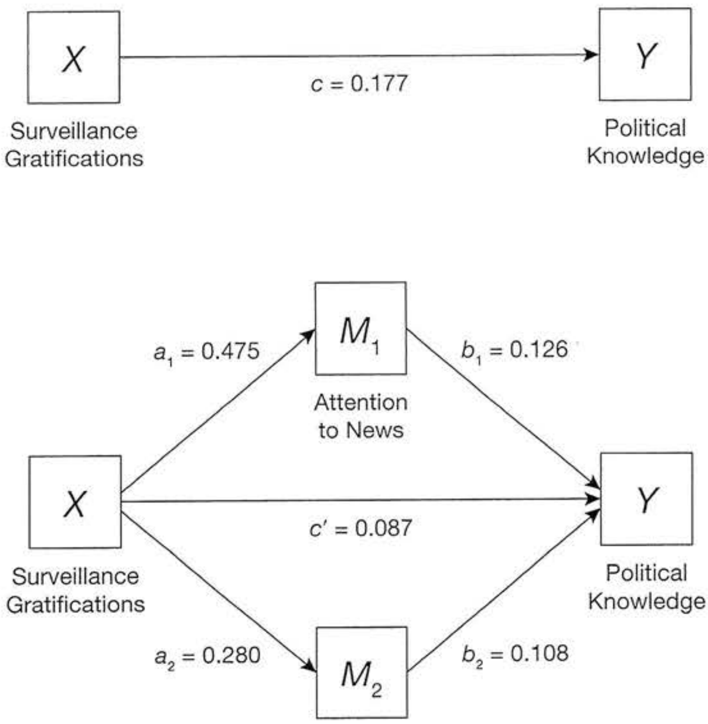

FIGURE 23.3 A single-step multiple mediator model with two proposed mediators. Not pictured is the covariance between the mediator residuals, rending this a saturated model.

elaboration, and surveillance gratifications. In these data, $b_1 = 0.126$ and $b_2 = 0.108$. Multiplying the corresponding a and b paths together yields the specific indirect effects of interest. For attention, $a_1b_1 = (0.475)(0.126) = 0.060$, and for elaboration, $a_2b_2 = (0.280)(0.108) = 0.030$. Summing the specific indirect effects yields the total indirect effect: $0.060 + 0.030 = 0.090$. Finally, the direct effect of elaboration comes from the same model used to estimate b_1 and b_2. It is the regression weight for surveillance gratifications in the model of knowledge, controlling for attention and elaboration: $c' = 0.087$. From equation (2), observe that the total effect is indeed equal to the sum of the direct effect and the two specific indirect effects (or, equivalently, the direct effect plus the total indirect effect): $0.177 = 0.087 + 0.060 + 0.030$.

In many instances, the single-step multiple mediator model is a much more realistic model of a process than is the simple mediation model, as effects often function through multiple mediators. When this is so and the investigator fails to include all the mediators that might be at work producing an effect, the simple mediation model can produce estimates of indirect effects that are statistically biased, in the same way that leaving an important variable out of a regression model related to one or more of the predictors and the outcome can bias the regression coefficient for those predictors. Indeed, it could be argued that direct effects in principle don't exist—that any direct effect that exists in a simple mediator model probably is itself mediated in some way, and the inclusion of another mediator in the model would likely shrink that direct effect toward zero. Thus, investigators should be open to investigating simultaneous indirect effects through multiple mediators. Furthermore, by doing so, it is possible to test for differences in the size of indirect effects through different mediators, as we discuss later.

Multiple-Step Multiple Mediator Model

This procedure for the partitioning of total effects into direct and indirect components also applies when mediators are allowed to causally affect other mediators. Figure 23.4 illustrates such a *multiple-step multiple mediator* model. Observe that, unlike the single-step multiple mediator model in Figure 23.3, this model has a path between two mediators, from M_1 to M_2. Using the tracing rule, four effects of X on Y can be identified—three specific indirect effects and one direct effect. The direct effect of X on Y, path c', is the weight for X in a model estimating Y from X, M_1, and M_2. The first specific indirect effect progresses only through M_1 and is defined as the product of the a_1 and b_1 paths, where the a_1 path is the weight for X in a model predicting M_1 from X and the b_1 path is the weight for M_1 in a model estimating Y from X, M_1, and M_2. The second specific indirect effect progresses through M_2 only. This effect is defined as the product of a_2 and b_2, where a_2 is the weight for X in a model predicting M_2 from X and M_1, and b_2 is the weight for M_2 in a model predicting Y from X, M_1, and M_2. The third specific indirect effect progresses first through M_1 and then through M_2 before ending at Y and is quantified as the product of a_1, a_3, and b_2, where a_3 is the regression weight for M_1 in a model predicting M_2 from M_1 and X, and a_1 and b_2 are defined as previously. These three specific indirect effects, when added together, define the total indirect effect of X on Y:

$$\text{Total indirect effect of } X \text{ on } Y = a_1b_1 + a_2b_2 + a_1a_3b_2 \tag{3}$$

and the total effect, c, is, as always, the sum of the direct and indirect effects:

$$c = c' + a_1b_1 + a_2b_2 + a_1a_3b_2 \tag{4}$$

Again, simple algebra shows that the total indirect effect is simply the difference between the total and direct effects: $c - c'$.

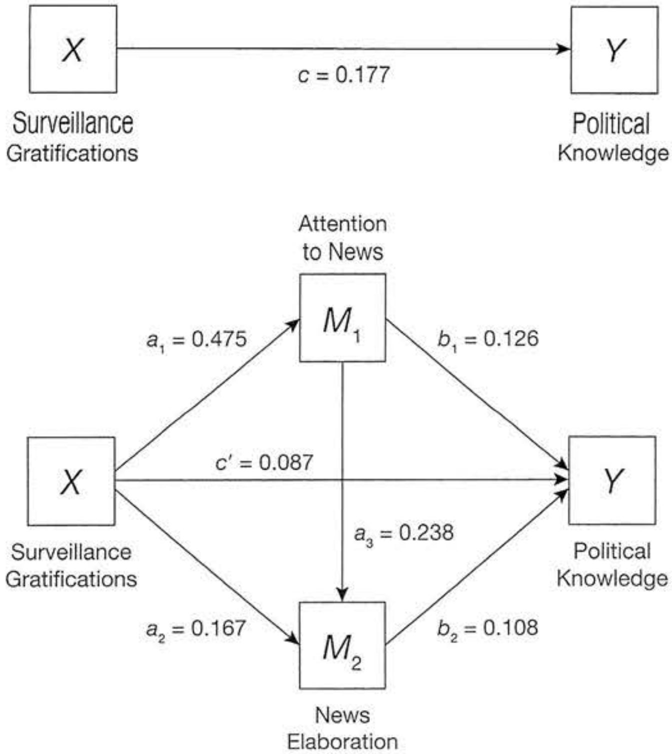

FIGURE 23.4 A multiple-step multiple mediator model with two proposed mediators.

Using the Eveland (2001) data, we estimated the direct and indirect effects for the full cognitive mediation model using both Mplus and SPSS. Mplus is also capable of doing all the inferential tests we soon discuss. Although SEM programs can be useful because they allow the investigator to analyze structural relations between latent variables and also provide measures of model fit, in this example the model is saturated so fit is necessarily perfect, and we use only observed variables, so these advantages of SEM do not exist here. Note that we could have estimated the coefficients in this model using any OLS regression program and then either used a different program for the inferential tests or conducted some of those tests by hand. Having said that, some of the inferential statistics we advocate later cannot be hand-calculated at all or, if they can, only with considerable effort with high potential for mistakes. For models of this variety, Mplus is our platform of choice, but others could be used. The estimates for this model can be found in Table 23.1 and Figure 23.4, and relevant Mplus and SPSS output can be found in Appendices 23.3 and 23.4, respectively.

Because the total effect of surveillance gratifications on political knowledge is not a function of the proposed mediators, the total effect remains $c = 0.177$. To derive the direct and indirect effects, we estimate all the path coefficients in the model simultaneously. The path from surveillance gratifications is estimated from a model predicting attention from just surveillance gratifications. As in all prior models, $a_1 = 0.475$. The path from surveillance gratifications to elaboration, a_2, is estimated by predicting elaboration from both surveillance gratifications and attention. Here, $a_2 = 0.167$. The model of elaboration also provides a_3, the path from attention to elaboration: $a_3 = 0.238$. All that remains for calculating indirect effects are the paths from the proposed mediators to the outcome. These are derived by predicting knowledge from both attention and elaboration while controlling for surveillance gratifications. Our model yields $b_1 = 0.126$ and $b_2 = 0.108$, respectively. The coefficient for surveillance gratifications in this model is c', the direct effect. In these data, $c' = 0.087$. Combining all this information yields the total and specific indirect effects of surveillance gratifications. The specific indirect effect through attention only is $a_1b_1 = (0.475)(0.126) = 0.060$. The specific indirect effect through elaboration only is $a_2b_2 = (0.167)(0.108) = 0.018$. The specific indirect effect through both attention and elaboration is $a_1a_3b_2 = (0.475)(0.238)(0.108) = 0.012$. Adding up the specific indirect effects yields the total indirect effect of surveillance gratifications on political knowledge: $0.060 + 0.018 + 0.012 = 0.090$. Notice that as discussed earlier, the total effect is equal to the sum of the direct effect plus all the indirect effects: $0.177 = 0.087 + 0.060 + 0.018 + 0.012$.

STATISTICAL INFERENCE ABOUT MEDIATION AND INDIRECT EFFECTS

In the prior section we explained the parsing of a total effect into direct and indirect components. To this point, our discussion of these effects has been a purely descriptive one—quantifying how changes in X are related to changes in Y directly and indirectly through one or more mediators. Of course, these estimates are subject to sampling error, given that the participants who participated in the study were a random sample from the community in which data collection occurred. Of interest is whether the effects observed give us reason to believe that they represent a property of the population sampled rather than merely the vagaries of sampling error, i.e., "chance." That is, is there evidence in the analysis that some kind of mediation process is at work in the population? We now shift to an inferential focus by discussing three popular ways of making inferences about mediation and indirect effects. These are by no means the only methods that are available, but we focus on these three because they are fairly easy to conduct and are implemented in several computing packages. For discussions of available inferential methods, see MacKinnon (2008) and Preacher and Hayes (2008a).

Causal Steps Approach

The causal steps procedure is the most widely implemented procedure for testing a mediation hypothesis (Baron & Kenny, 1986; Hyman, 1955). The popularity of this approach is no doubt due in part to how easy it is to understand and implement. In essence, the causal steps procedure requires the investigator to conduct a set of hypothesis tests for each link in a path diagram. A failure to reject one or more of the null hypotheses leads one to claim an absence of evidence of mediation.

Applied to a simple mediation model as in Figure 23.2, the causal steps approach first asks whether there is evidence of an effect to be mediated. That is, is the total effect of X on Y (i.e., path c in Figure 23.2) statistically significant? If not, the investigator cannot claim mediation, as an effect that does not exist cannot be mediated, and further testing stops. Presuming that there is evidence of a relationship between X and Y, the investigator then tests for evidence that X is related to M (path a in Figure 23.2). Again, in the absence of a statistically significant relationship in a model predicting M from X, testing for mediation stops and the investigator claims no mediation effect. However, if this condition is met, the investigator then asks whether M is significantly related to Y after controlling for X (path b in Figure 23.2). If not, the investigator claims no mediation. If b is significant, then the investigator examines the relative size of c and the partial effect of X on Y controlling for M (path c' in Figure 23.2). If all effects are in the direction consistent with the proposed mediation process, c' will typically be closer to zero than c is. If c' is not statistically significant, the investigator claims that M completely mediates the effect of X on Y. But if c' remains significant but closer to zero than c, then this supports a claim of partial mediation—that some but not all of the effect of X on Y is carried through M.

The causal steps procedure can, in principle, be applied to more complicated mediation models involving multiple mediators. The investigator can test each specific effect in a path diagram with multiple mediators by conducting a hypothesis test for each step in the path. If all links are significant and the pattern of the total and direct effects of *X* on *Y* is consistent with a reduction in the effect of *X* after accounting for the proposed mediator or mediators, this suggests mediation is at work through that mediator or mediators. However, problems arise when indirect effects work in opposite directions. In such a situation, *c* (the total effect of *X* on *Y*) may not even be statistically significant. For this reason, we do not believe that a significant total effect should be a prerequisite to testing for indirect effects using any of the methods we discuss in this chapter. This is an important point worth stressing again. Investigators should not condition the hunt for indirect effects on a significant total effect, for it is possible for an indirect effect to exist in the absence of compelling evidence of a total effect (see Hayes, 2009, for a more detailed discussion and a concrete example).

In spite of its popularity—one might say even *dominance* in the research literature—we cannot advocate the routine use of the causal steps approach. There are two primary reasons for our position on this. First, the causal steps approach does not rely on an estimate of the indirect effect, which is ultimately what carries information about *X*'s effect on *Y* through one or more intervening variables. Thus, this method encourages researchers to not think about effect size, and it does not allow for the construction of a confidence interval for the indirect effect to acknowledge the uncertainty in the estimation process. Second, it relies on a series of hypothesis tests which requires the investigator to think in categorical terms about each step of the model. Either an effect exists or not, and if any of the criteria are not met, the investigator is left with nothing. There would be nothing inherently wrong with this if the causal steps procedure led the investigator to the correct conclusion given the data available. But, unfortunately, the causal steps procedure is among the *lowest* in power among tests of the effect of intervening variables in causal models (MacKinnon, Lockwood, Hoffman, West, & Sheets, 2002; MacKinnon, Lockwood, & Williams, 2004).

Product of Coefficients Approach

Unlike the causal steps approach, the product of coefficients approach acknowledges that the investigator has an estimate of an indirect effect—the product of estimates of the paths linking *X* to *Y* through a specific mediator variable *M*—and inferences about the indirect effects and mediation are based on that estimate. Like any statistic, the product of these regression coefficients (*ab*) has a sampling distribution. And, like any sampling distribution, the sampling distribution of *ab* has a standard deviation. The standard deviation of the sampling distribution of *ab*—also known as the *standard error* of *ab*—can be estimated and used for hypothesis testing and the construction of confidence intervals for the indirect effect. Typically, the investigator would be interested in testing the null hypothesis that the population indirect effect is equal to zero. Rejection of the null hypothesis implies that there is an indirect effect of *X* on *Y* through a given mediator *M*. This procedure is popularly known as the *Sobel test* (after Sobel, 1982). Alternatively, the investigator could construct a confidence interval for the population value of *ab* and make inferences about its size (including whether zero is a plausible value for the indirect effect).

There are numerous formulae circulating for how to estimate the standard error of *ab* that applies to single-step simple and multiple mediator models. The formula implemented in the Preacher and Hayes (2004) macro used in the simple mediation model above is

$$s_{ab} = \sqrt{b^2 s_a^2 + a^2 s_b^2 + s_a^2 s_b^2} \tag{5}$$

where a^2 and b^2 are the squares of the estimates for the paths from X to M and M to Y, respectively, and s_a^2 and s_b^2 are the squared standard errors of those path coefficients. An alternative formula subtracts $s_a^2 s_b^2$ under the radical in (5) rather than adding it, and another omits the $s_a^2 s_b^2$ term altogether (see, e.g., Goodman, 1960). In practice, the difference between these approaches is small and it usually matters little which formula is used. How to best estimate the standard error is a moot question when using bootstrapping, the approach we ultimately favor and discuss later.

Once the standard error is estimated, the ratio of the indirect effect to its standard error (typically denoted as Z) is used as a test statistic for testing the null hypothesis that the population indirect effect is zero. The p-value for this ratio is derived in reference to the standard normal distribution, as this approach assumes that the sampling distribution of ab is normal. Alternatively, a 95% confidence interval (CI) for the indirect effect can be derived in the usual way, as

$$95\%\ \text{CI} = ab \pm 1.96 s_{ab} \tag{6}$$

If 0 is not in the confidence interval, one can claim that there is evidence of an indirect effect linking X and Y through that mediator with 95% confidence. Confidence intervals for different levels of confidence can be used by substituting the appropriate critical value in equation (6) for the desired confidence (e.g., 2.57 for 99% confidence).

We illustrate this procedure for the two specific indirect effects in the single-step multiple mediator model above. Recall in that model that attention to news (M_1) and elaboration (M_2) were proposed mediators of the relationship between surveillance gratifications (X) and political knowledge (Y). To four significant digits (see Appendix 23.2), the two estimates were $a_1 b_1$ = 0.0599 (for attention to news) and $a_2 b_2$ = 0.0302 (for elaboration). Using the standard errors for the paths shown in the output in Appendix 23.2,

$$s_{a_1 b_1} = \sqrt{(0.1261)^2\,(0.0578)^2 + (0.4750)^2\,(0.0271)^2 + (0.0578)^2\,(0.0271)^2} = 0.0149 \tag{7}$$

$$Z_{a_1 b_1} = \frac{0.0599}{0.0149} = 4.0201,\ p < .001 \tag{8}$$

$$95\%\ \text{CI} = 0.0599 \pm 1.96(0.0149) = 0.0301 \text{ to } 0.0891 \tag{9}$$

and

$$s_{a_2 b_2} = \sqrt{(0.1081)^2\,(0.0571)^2 + (0.2796)^2\,(0.0274)^2 + (0.0571)^2\,(0.0274)^2} = 0.0099 \tag{10}$$

$$Z_{a_2 b_2} = \frac{0.0302}{0.0099} = 3.0505,\ p < .002 \tag{11}$$

$$95\%\ \text{CI} = 0.0302 \pm 1.96(0.0099) = 0.0108 \text{ to } 0.0496 \tag{12}$$

For both specific indirect effects, we can reject the null hypothesis of no indirect effect through that mediator. These results are very close to those displayed in Appendix 23.2, differing as a result of rounding error produced by hand computation. In practice, there would be no need to implement these procedures by hand, as the macros we use here and SEM programs such as Mplus will do the computations for you.

Standard Errors in a Multiple-Step Multiple Mediator Model. The formula in equation (5) is used to compute the standard error of a single-step indirect effect in models containing one or more mediators. However, what about the standard error for the multiple-step $X \rightarrow M_1 \rightarrow M_2 \rightarrow Y$ indirect effect ($a_1a_3b_2$)? This term, after all, is the product of *three* regression weights, so the formula becomes somewhat more complicated. Taylor, MacKinnon, and Tein (2008; see also Fletcher, 2006) derive the standard error of $a_1a_3b_2$ as:

$$s_{a_1a_3b_2} = \sqrt{a_1^2a_3^2s_{b_2}^2 + a_1^2b_2^2s_{a_3}^2 + a_3^2b_2^2s_{a_1}^2 + a_1^2s_{a_3}^2s_{b_2}^2 + a_3^2s_{a_1}^2s_{b_2}^2 + b_2^2s_{a_1}^2s_{a_3}^2 + s_{a_1}^2s_{a_3}^2s_{b_2}^2} \quad (13)$$

As before, once the standard error is estimated, the ratio of the indirect effect to $s_{a_1a_3b_2}$ is used as a test statistic for testing the null hypothesis that this specific indirect effect is zero in the population. In our running example, the standard error equates to 0.0042, as the reader may verify in Appendix 23.3. Because the specific indirect effect $a_1a_3b_2 = .0122$,

$$Z_{a_1a_2b_3} = \frac{0.0122}{0.0042} = 2.903, p < .004 \quad (14)$$

which differs from Table 23.1 and Appendix 23.3 only as a result of rounding error produced by hand computation. We conclude that the specific indirect effect through M_1 and M_2 is statistically significant.

Standard Errors for the Total Indirect Effect. Thus far we have discussed inferences for specific indirect effects. But in models with multiple mediators, there is also a total indirect effect, which we noted earlier is equal to the sum of the specific indirect effects. Using a similar logic as described above, it is possible to construct confidence intervals or test hypotheses about the total indirect effect by estimating its standard error. These computations are somewhat complex and generally done by a computer, so we don't discuss them here. See Fletcher (2006), MacKinnon (2000), and Preacher and Hayes (2008b) for guidance. Most SEM programs, as well as our SPSS and SAS macro for multiple mediator models (for single-step multiple mediator models; Preacher & Hayes, 2008b) provide standard errors for the total indirect effect. For example, in Appendix 23.2, the estimated standard error for the total indirect effect is 0.0170. The ratio of the total indirect effect to its standard error is $Z = 0.0901/0.0170 = 5.3106, p < .001$ and a 95% confidence interval for the total indirect effect is $0.0901 \pm 1.96(0.0170) = 0.0568$ to 0.1234.

Bootstrapping

The product of coefficients approach purchases power over the causal steps approach by focusing specifically on the estimate of the indirect effect rather than the use of multiple hypothesis tests for making inferences about a sequence of relations. Nevertheless, the product of coefficients approach suffers from one major limitation which leads us to prefer an alternative approach. The product of coefficients approach relies on the assumption of a normally distributed sampling distribution of the indirect effect. This assumption was used to justify the standard normal distribution as the reference distribution for generating a *p*-value, as well as for the construction of a symmetric interval estimate for the population indirect effect. By *symmetric*, we mean that the distance between the point estimate (the value of *ab* in the sample) and upper bound of an interval estimate of the indirect effect is the same as the distance between the point estimate and the lower bound. A less serious limitation of this method is that it also relies on a standard error formula based on mathematical theory that is itself based on certain assumptions. Furthermore, there are several standard error formulas for the indirect effect, and it is not clear whether one or the other is best in a given situation.

The problem with assumptions is that they are not always justified or met, and in this case, we know that the assumption of a normally distributed sampling distribution for the indirect effect simply is not justified, at least not in small samples (Bollen & Stine, 1990). Instead, it is skewed and kurtotic. Although this distribution approaches normality with increasingly larger samples, it is difficult to know in a particular application whether this assumption is reasonable, making it hard to justify the use of the normal distribution as the reference distribution for generating a *p*-value for the Sobel test. Furthermore, it means that a confidence interval, if it accurately reflected the asymmetric of the sampling distribution of the indirect effect, would not and should not be symmetric. That is, the distances between the upper and lower bounds of a confidence interval and the point estimate should differ.

Although researchers have been satisfied to rely on assumptions about the shape of distributions throughout the history of statistics and its application to real data problems, in modern times it is not necessary to rely on obsolete assumptions known to be incorrect. An alternative method for making inferences about indirect effects exists that does not rely on the normality assumption: *bootstrapping*. Bootstrapping is a member of the category of statistical methods sometimes called *resampling methods*, in that it relies on the repeated sampling of the data set to empirically estimate properties of the sampling distribution of a statistic. The resampling of the data set allows the investigator to relax many of the assumptions that ordinary inferential statistics require. A few decades ago, bootstrapping was nice in theory but somewhat difficult to implement in practice because of the computing power it requires. But desktop computing power has increased substantially since the invention of bootstrapping and, as a result, this approach to statistical inference has gained in popularity. New applications of the method are appearing in the empirical journals regularly. There are numerous books and journal articles that describe bootstrapping (e.g., Boos, 2003; Efron & Tibshirani, 1993; Stine, 1989), and we encourage the reader to explore this fascinating method in greater detail. In the interest of space, here we focus on the application of bootstrapping to the problem at hand—making inferences about the size of indirect effects in the kinds of mediation models discussed in this chapter.

As with all statistical methods, bootstrapping starts with a sample of size n from some population of interest. With bootstrapping, this sample is treated as a pseudopopulation that represents the population from which the sample was derived. More specifically, bootstrapping assumes that the data set with n cases that the investigator has available represents, in a sense, the population in miniature—that the distributions of the variables measured (i.e., the relative frequency of various observed measurements) are fairly similar to the distributions that would have been observed had the entire population been available for measurement. With this construal of the sample as a pseudopopulation, bootstrapping requires that the investigator take a simple random sample of size n from the original sample *with replacement*, meaning that when a case is randomly drawn to be included in the new sample, that case is put back to potentially be redrawn in that same sample. In this "resample" of size n from the original sample (which, remember, is also of size n) the coefficients of the model are estimated and recorded. So, for instance, for a simple mediation model, the investigator estimates the a and b paths in this resample of the original sample. This process is then repeated by taking a new resample of size n, again from the original sample, and a and b are estimated again. Of course, the two values of a in these two resamples will differ from each other, as will the two values of b, just as a and b would differ from sample to sample when sampling from the full population. This process is repeated a total of k times, with k at least 1,000, although the larger the k the better. After k resamples, k estimates of

a and b and, therefore, the product of a and b are available. This distribution of k values of ab, each estimated in a resample of size n from the original sample, represents an empirical approximation of the sampling distribution of the indirect effect of X on Y through M when taking a sample of size n from the full population. It is this empirically derived representation of the sampling distribution that is used for making inferences about the indirect effect.

Observe that this method can be used to make inferences about all of the effects in a mediation model—direct, specific indirect, total indirect. Furthermore, it can be used for mediation models of any complexity. For instance, in a single-step multiple mediation model, one can simultaneously generate a bootstrap approximation of the sampling distribution for each of the specific indirect effects by calculating (for a two-mediator model) a_1b_1 and a_2b_2 in each of the k resamples. Or in a multiple-step multiple mediator model as in Figure 23.4, the sampling distribution of the indirect effect of X on Y through M_1 and M_2 can be estimated k times by estimating $a_1a_3b_2$ (along with a_1b_1 and a_2b_2) in each resample. Indeed, bootstrapping can be used to estimate the sampling distribution of nearly any statistic one might want to compute.

Before describing how these k estimates of an indirect effect can be used to our advantage, let us pause and contemplate the elegance of this method. Traditionally, statistical methods rely on various statistical theories about the behavior of a statistic, such as the Central Limit Theorem, when sampling from a population. By relying on theory, all that is needed is a single sample in order to make inferences about the population from which the sample was derived. But applications that rely on statistical theories often make assumptions, such as assumptions about the shape of the sampling distribution of the statistic. Bootstrapping eliminates the need for such assumptions about the shape of the sampling distribution because it relies on an empirical approximation of the sampling distribution by mimicking the sampling process. When bootstrapping, each case in the original sample is construed as an exemplar or representative of all units in the population like it. This is why any case that is resampled is put back into the pool to be potentially redrawn when building each resample. In other words, if "John" happened to be included in the original sample, John is used to represent all the other people just like John in the population who could have been included in the sample but happened not to be. Thus, John could show up several times (or never) in a single resample, just like many people just like John could have ended up in the original sample but perhaps were not. By mimicking the sampling process in this way, bootstrapping empirically approximates the sampling distribution of the indirect effect when taking a random sample of size n from a larger population.

So what do we do with these k estimates of an indirect effect once they are calculated? There are many applications. For example, one could calculate the standard deviation of the k estimates and use this as the standard error in the Sobel test rather than the standard error based on a theoretical formula (of which there are several, as described above). This would yield a "bootstrap Sobel test" of sorts. Indeed, the SPSS outputs in Appendices 23.1 and 23.2 provide the standard deviation of the bootstrap sampling distribution of the indirect effect (under "S.E." in the "Bootstrap Results" section), as do some SEM programs that bootstrap. But if the p-value is generated using the standard normal distribution, this does not eliminate the very problem that leads us to prefer bootstrapping in the first place, in that it still relies on the assumption of a normally distributed sampling distribution of the indirect effect. So we do not recommend a bootstrap-based Sobel test.

Instead, we recommend the use of bootstrap confidence intervals for making inferences about indirect effects. A bootstrap confidence interval for the indirect effect is constructed by sorting the distribution of k indirect effect estimates from low to high and then finding the estimates in this sorted distribution that define the LL $= (k / 100)(50 - \delta / 2)$th and UL $= [(k / 100)(\delta / 2) + 1]$th position in this list, where δ is the desired confidence level for the interval. The estimate in the

LLth position is the lower limit of the confidence interval, and the estimate in the ULth position is the upper limit. For instance, for a $\delta = 95\%$ confidence interval based on 5000 bootstrap samples, LL = (5000 / 100)(50–95/2) = 125 and UL = (5000 / 100)(50 + 95/2) + 1 = 4876. So the lower and upper limits of the confidence interval are the 125th and 4876th estimates in the list, respectively. Although this sounds obtuse when described formally, this procedure does nothing other than delineate the inner δ% of the sampling distribution of the indirect effect. In other words, the indirect effect estimate in the LLth and ULth positions cut off the lower and upper $0.5(100–\delta)\%$ of the sampling distribution from the rest of the distribution. Typically, the lower and upper limits of the confidence interval will not be equidistant from the point estimate of the indirect obtained in the original sample because the empirical approximation of the sampling distribution of the indirect effect will usually be skewed. As a result, a bootstrap confidence interval is sometimes called an *asymmetric* interval estimate.

The approach we just described is known as the *percentile bootstrap*, and it is the method implemented in our SPSS and SAS macros for simple mediation models (Preacher & Hayes, 2004) and SPSS for multiple-step multiple mediator models of the form in Figure 23.4 (see Appendix 23.4). As can be seen in Appendix 23.1, based on 5000 bootstrap samples, the 95% confidence interval for the indirect effect of surveillance gratifications on knowledge through attention to news is 0.0422 to 0.1072 (note that a 99% CI is also provided in the output). As zero is not in this interval, we can say with 95% confidence that the "true" indirect effect is positive. Technically, we cannot say that we are rejecting the null hypothesis that the indirect effect is zero at a 0.05 level of significance because this is not really a null hypothesis test as a hypothesis test is formally defined. Nevertheless, our substantive conclusion is that the indirect effect is probably not zero. It is likely to be somewhere between about 0.042 and 0.107, with the obtained indirect effect of 0.072 being a sensible point estimate. Notice that, as promised, the confidence interval is asymmetric; the upper and lower bounds of the confidence interval are not the same distance from the point estimate of 0.072, reflecting the skew in the sampling distribution of the indirect effect.

Bootstrap confidence intervals can be made more accurate through the use of bias correction or through bias correction and acceleration, yielding a *bias corrected* (BC) or *bias corrected and accelerated* (BCa) confidence interval. These adjustments defy nonmathematical explanation, so we refer the reader elsewhere for the details (Efron, 1987; Efron & Tibshirani, 1993). Suffice it to say that they adjust the upper and lower limits of the interval estimate so as to produce intervals that are more likely to contain the true indirect effect without changing the level of confidence. Neither adjustment is clearly superior to the other, and in practice it makes little difference whether BC or BCa limits are used, although one or the other is generally preferred to percentile bootstrap intervals. Mplus has implemented both percentile and BC bootstrap confidence intervals but prints only 95% and 99% intervals. In Appendix 23.3, BC confidence intervals are printed for all three indirect effects and the total effect in the multiple-step multiple mediator model corresponding to the cognitive mediation model. As can be seen, for all these indirect effects, the 95% BC confidence intervals do not contain zero, suggesting evidence for a positive indirect effect for each and every indirect path from surveillance gratifications to political knowledge. Our SPSS and SAS macros for single-step multiple mediator models allow the user to specify which among all three methods to calculate, including all three at once if desired, and the user can specify any level of desired confidence. In Appendix 23.2, bias corrected and accelerated confidence intervals are displayed for the two specific indirect effects in that model as well as the total indirect effect. Again, all are positive.

We believe bootstrapping is the most sensible approach to assessing the size of indirect effects. It makes no assumptions about the shape of the sampling distribution. It does not require the investigator to think about each step in a causal sequence in dichotomous terms (i.e., significant or not). And given that it does not rely on the computation of a standard error, it is immune to problems that can arise when the assumptions associated with the use of a standard error are not met. When combined, these properties of bootstrapping are no doubt what accounts for the findings from various simulations that bootstrapping has greater statistical power than the product of coefficients strategy for detecting indirect effects when they are present (Briggs, 2006; MacKinnon et al., 2002, 2004; Williams & MacKinnon, 2008).

Having said this, at the same time we do not want to overstate our case. In many instances, the choice of which method to use will matter little in terms of substantive findings, as was the case in the analyses presented above. This would tend to be true in large samples or when effects are large enough to be detected with power near 1. But researchers often do not have the luxury of large samples or the kinds of sample sizes that make inferential tests a mere formality. In those situations, the choice of method can make a difference. When given the choice, bootstrapping is our preferred method.

Comparing Specific Indirect Effects

An indirect effect is free of the scale of measurement of the mediator or mediators in the model. That is, the indirect effect quantifies how much Y is expected to change on the Y measurement scale through M as X changes by one unit on the X measurement scale, irrespective of the scale of the mediator or mediators that the causal influence passes through (see Preacher & Hayes, 2008b, for an informal proof of this point). Therefore, it is possible to make meaningful comparisons, both descriptive and inferential, between the sizes of indirect effects in models with more than one mediator. For example, in the single-step multiple mediator model described earlier, is the indirect effect of surveillance gratifications on political knowledge larger through attention to news or elaboration of news content?

To answer such a question, all that is necessary is a quantification of the difference between specific indirect effects and either an estimate of the standard error of the sampling distribution of the difference or, alternatively, the ability to bootstrap that sampling distribution. Mplus, LISREL, and our SPSS and SAS macros for multiple mediator models provide standard errors for pairwise comparisons between specific indirect effects as well as the ability to bootstrap the sampling distribution of the difference. Notice in Appendix 23.2, for example, the lines of the output that read "C1." These lines provide the difference between the specific indirect effect of surveillance on knowledge through attention and through elaboration (i.e., $a_1b_1 - a_2b_2$), including the standard error for this difference as well as a Z and p-value for testing the null hypothesis of no difference. A bootstrap 95% CI is also provided in the bootstrap section of the output. Both of these approaches point to the same conclusion—that there is no evidence that these specific indirect effects differ from each other in size. Preacher and Hayes (2008b) provide Mplus and LISREL code for conducting comparisons between indirect effects in those SEM programs, and MacKinnon (2000) describes simple and complex contrasts between specific indirect effects.

MEDIATION IN MORE COMPLEX MODELS

The mediation models we have described to this point all have been variations on the same theme. Simply put, there is an effect of X on Y that needs explaining, and the models we have described represent different hypotheses about the means by which X exerts its influence on Y. The researcher may posit one mediator to explain the effect, or even two or more mediators acting in tandem in a variety of ways (see Figures 23.1, 23.3, and 23.4 for simple examples). As causal models go, these are relatively simple ones. But such simple causal models are rarely sufficient to describe and explain the complexity of social phenomena and causal processes that are the focus of much communication research.

The basic mediation model may be (and has been) extended in a number of ways. We have already described a few alternative methods of testing mediation hypotheses; there are many more that we did not discuss, and these issues are currently being debated, studied, and resolved in an extensive methodological literature (see, e.g., MacKinnon, 2008; MacKinnon et al., 2002, 2004). Mediation modeling has also been extended to accommodate nonnormal data (Finch, West, & MacKinnon, 1997), binary data (Huang, Sivaganesan, Succop, & Goodman, 2004; Li, Schneider, & Bennett, 2007; MacKinnon, Lockwood, Brown, Wang, & Hoffman, 2007), multilevel or hierarchical data (Bauer, Preacher, & Gil, 2006; Kenny, Korchmaros, & Bolger, 2003), and survival data (Tein & MacKinnon, 2003). New models and ways of thinking about mediation have been proposed (Cole & Maxwell, 2003; Collins, Graham, & Flaherty, 1998). The variety and complexity of models that may be proposed is enormous. We have scarcely scraped the surface, and much is yet to be discovered.

In the sections that follow, we describe extensions of the basic mediation model that may be of particular interest to political communication scientists. Specifically, we address the interface and combination of mediation and moderation effects, and mediation models for longitudinal data. Finally, we emphasize the value of including covariates and (whenever possible) using latent variable models to provide unbiased effects.

Moderated Mediation and Mediated Moderation

Moderation (or interaction) effects are common in political communication research. Moderation occurs when the effect of X on Y depends on a moderator variable W. For example, Scheufele (2002) hypothesized (and found) that the impact of people's exposure to hard news content (X) on political participation (Y) would be moderated by interpersonal discussion about politics (W). Moderation hypotheses like this are usually tested by including the product term XW (along with conditional terms X and W) as a predictor of Y in a regression or SEM context. If the coefficient for XW is statistically significant, then W is said to moderate the effect of X on Y, and further plotting and exploration are often warranted (Aiken & West, 1991; Bauer & Curran, 2005; Hayes & Matthes, 2009).

It is not difficult to imagine scenarios in which a moderation effect is itself hypothesized to be mediated by some intervening variable M. Likewise, it is simple to imagine scenarios in which a mediation effect is thought to be moderated by some variable W. Such effects are called, respectively, *mediated moderation* and *moderated mediation*, and statistical methods exist to assess these processes.

Mediated moderation hypotheses are becoming increasingly common in political communication research. For example, Scheufele (2002) hypothesized that the interactive effect of hard news exposure and political discussion on political participation would be mediated by political knowledge (M). Mediated moderation is particularly easy to assess. Say we have demonstrated an interaction effect of X and W predicting Y. That is, the effect of X may be of a different magnitude, or even a different sign, for different values of W. This effect must somehow find its way to Y, and it is not unreasonable to suppose that it does so via a mediator M. The researcher would specify a model in which X and W interact in predicting M, which in turn may predict Y. If the interaction slope of XW on Y is denoted a_3, and the effect of M on Y (controlling for X, W, and XW) is denoted b, then the mediated moderation effect is simply a_3b. The significance of this effect may be determined by any of the methods we described earlier, although we particularly recommend using bootstrap confidence intervals. The SPSS and SAS macros provided for multiple mediator models (Preacher & Hayes, 2008b) can be used to assess such effects. Only one mediator is involved, but the macros permit the inclusion of X and W as covariates, with XW serving as the primary predictor.

Mediation processes may also be moderated. For example, Holbert (2005b) tests a moderated mediation hypothesis showing that the indirect path from TV news viewing to attitudes about government involvement in social issues via perceptions of the government's role is different for Democrats, Republicans, and Independents. This analysis was accomplished using multiple-group SEM, but the moderator variable (political affiliation in this example) may be continuous or categorical. Moderated mediation is somewhat more difficult to assess than mediated moderation, but methods exist (Edwards & Lambert, 2007; Preacher, Rucker, & Hayes, 2007). The basic idea is to specify one's model as a path model and consider which path or paths are expected, on the basis of theory, to be moderated by one or more potential moderator variables. Product terms are then entered into the model in the appropriate places, which will differ from situation to situation. Once the model is estimated, the researcher can plot, and explore the significance of, *conditional indirect effects*—that is, indirect effects at specific values of the moderator(s) that are of key theoretical interest. Preacher et al. (2007) provide an SPSS macro that considerably eases the burden on researchers.

Mediation in Longitudinal Studies

Mediation models are causal models. One implication of a simple mediation model is that if case i's X value were changed in some fashion, then case i's M value would change as a result, and this change in M would in turn cause case i's Y value to change. In the examples used throughout this chapter, such a strong causal interpretation is hard to justify in spite of the statistical evidence because there has been no measurement of change and no manipulation of one or more of the proposed causal agents. Instead, claims of mediation in the examples described thus far have been based on a cross-sectional or between-persons argument—that people who differ in X also differ on M as a result of those differences in X, which in turn produce differences between those people in Y. Longitudinal designs, where X, M, and Y are measured repeatedly at set intervals, allow for the actual assessment of change over time, and this can give the researcher much greater confidence in a mediation claim when it can be substantiated that difference is actually related to the specified variables.

There is a growing literature on the testing of mediation when the variables in the proposed causal system are measured over time. One approach is the use of the standard cross-lagged panel design. For example, to examine the indirect effect of negative political advertisements (X) on political participation (Y) through its effects on political cynicism (M), an investigator could measure these three variables at three or more times during an election season, preferably with roughly equally spaced intervals that are appropriately timed given the causal process being studied. With such a design, indirect effects can be calculated as the product of the effect of ad exposure at time 1 on political cynicism at time 2 and the effect of political cynicism at time 2 on political participation at time 3 all while controlling for temporal stability in the variables over time as well as direct effects between ad exposure and participation that reach across time. In such a design, change is operationalized somewhat loosely as being either higher or lower on the outcome variable than would be expected given the prior measurement on that variable and other variables in the model. There are many assumptions that one must make when using such an approach to the analysis of mediation, and there are still alternative explanations that can account for indirect effects in such models. Careful thought must go into the planning of a longitudinal design such as this. For guidance, we recommend Cole and Maxwell (2003), Little, Preacher, Selig, and Card (2007), and Maxwell and Cole (2007).

An exciting alternative for assessing mediation in longitudinal studies is the use of *parallel process latent growth curve modeling*. Latent growth curve modeling is typically conducted using SEM. With at least three repeated measurements of each variable, latent growth curve modeling capitalizes on regularity in the fluctuation in measurement of each case over time to quantify that case's "latent change" per unit of time on that variable, or "latent growth" as it more typically called. The mean latent growth parameters (e.g., intercept and slope) estimated from the data can then be linked together in a structural model to assess mediation. In Figure 23.5 we present one way of setting up such a model, although there are many ways that the parameters can be linked together, depending on various substantive, theoretical, or statistical considerations. This model includes several indirect effects of X on Y through M. One indirect effect, a_0b_0, quantifies the influence of baseline X (π_{X0}) on baseline Y (π_{Y0}) through baseline M (π_{X0}). A second indirect effect, a_1b_1, quantifies the extent to which change in X (π_{XI}) predicts change in M (π_{M0}) which in turn predicts change in Y (π_{YI}). But observe as well that baseline X could influence the extent to which M changes, which in turn could influence change in Y. This indirect effect of X on Y through M is quantified as a_2b_1. Finally, this model also allows for the possibility that baseline X could

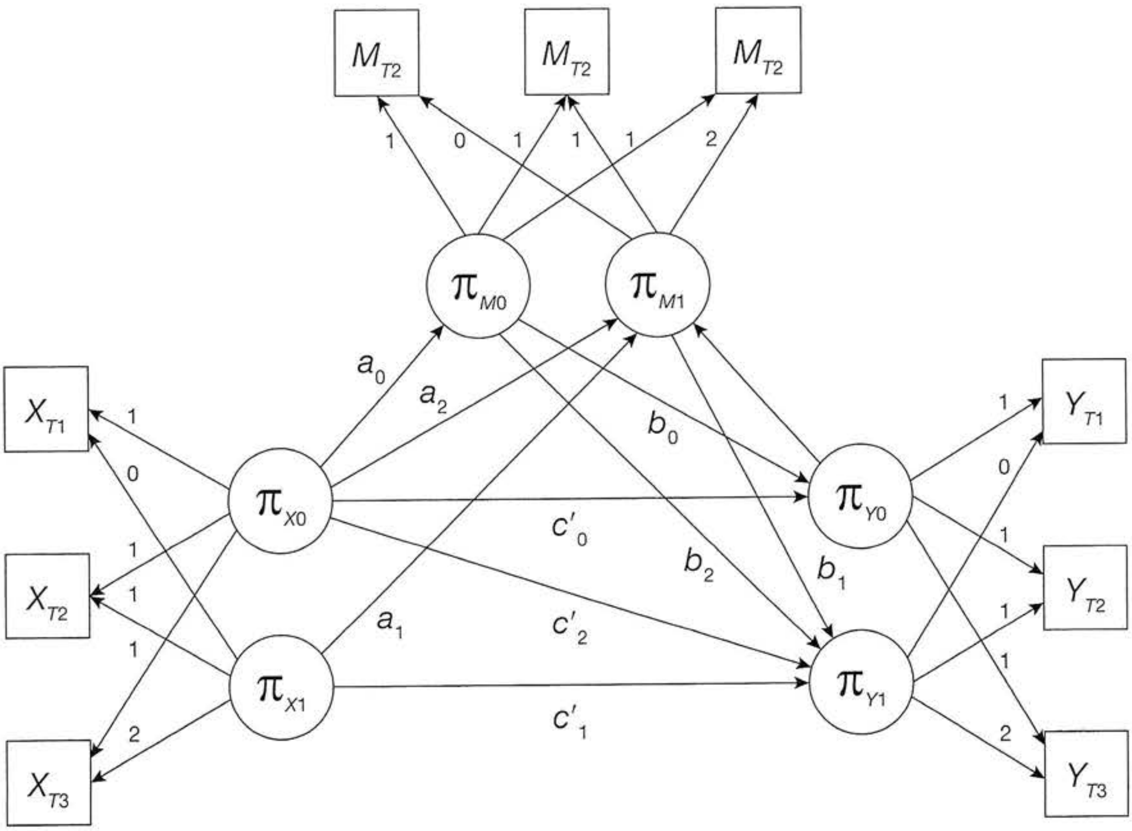

FIGURE 23.5 An example of a parallel process latent growth mediation model. Not pictured are correlations between intercept and slope residuals (within M and Y), the correlation between X intercept and slope, and intercorrelations between indicator residuals within time.

influence baseline M, which in turn influences change in Y, quantified as a_0b_2. The parallel process model for assessing mediation shows great promise when experimental manipulation of X is not possible, but measurement over time is. Simons-Morton, Chen, Abroms, and Haynie (2004) present an application in a health communication context, but to our knowledge, this method has never been used in a political communication study. Cheong, MacKinnon, and Khoo (2003) discuss a form of the parallel process growth curve model where X is experimentally manipulated at time 1 whereas M and Y are measured longitudinally over three occasions. In such a design, it is possible to assess whether the experimental manipulation changes the latent change trajectory for the proposed mediator and whether that change in turn is related to latent change in the outcome.

Models with Covariates or Latent Variables

Thus far we have neglected to acknowledge that many models that political communication researchers would be interested in testing would include numerous statistical controls. For instance, it is common to estimate the paths in a mediation model after first partialing out demographic variables such as sex, education, income, age, and so forth. The basic logic of the methods described here do not need modification to handle covariates. One version of our SPSS and SAS macros (Preacher & Hayes, 2008b) for multiple mediators, as well as the one for moderated mediation (Preacher et al., 2007) allows for the inclusion of covariates under the assumption that all covariates are partialed out of all mediators and the outcome. When an SEM program such as Mplus is used, the inclusion of covariates is accomplished quite simply by including directional paths from the covariates to whatever variable in the model the investigator desires, and all direct and indirect effect estimates, standard errors, and confidence intervals (bootstrapped or otherwise) will be correctly calculated after statistical adjustment for the control variables.

In all examples we have presented in this chapter, the variables linked in causal sequence in each model were treated as observed variables. Doing so greatly simplifies our discussion and also allows the use of the various macros we have produced for popular platforms such as SPSS and SAS. Of course, with few exceptions, it is usually not so much the observed measurements we care about, but rather the latent variables that give rise to the observed measurements. Fortunately, all the methods we have discussed can be used to test mediation hypotheses involving latent variables, although doing so requires the use of structural equation modeling rather than any of the macros we have described. An advantage of going latent is that it typically reduces one source of bias in the estimation of indirect effects that we have not acknowledged—measurement error. Mplus provides all the features the user will need to implement these methods, including bootstrapping of structural paths and estimation of indirect effects.

RECOMMENDING FURTHER READING

In this chapter we have discussed the application of basic principles in the estimation of mediation and indirect effects to political communication research. Out of necessity, we have glossed over many interesting controversies as well as important extensions. It is our hope that the discussion above has stimulated readers to further explore the vast literature on this topic and to try some of the methods we describe here and others that can be found elsewhere. For a good starting place, we recommend MacKinnon (2008), which promises to be the seminal treatment of the analysis of mediation. In it you will find examples of mediation analyses in many different fields, as well as valuable guidance on how to conduct mediation analyses using data from numerous research designs. Additional good and brief treatments include Frazier, Tix, and Barron (2004), MacKinnon, Fairchild, and Fritz (2007), Mathieu, DeShon, and Bergh (2008), Preacher and Hayes (2008a), and Shrout and Bolger (2002).

APPENDIX 23.1

The computer output below comes from the SPSS version of the simple mediation macro described in Preacher and Hayes (2004). In this example, X is surveillance gratifications, M is attention to news, and Y is political knowledge.

sobel y = know/x = grats/m = newsattn/boot = 5000.

```
Run MATRIX procedure:

DIRECT AND TOTAL EFFECTS
              Coeff        s.e.          t     Sig(two)
b(YX)         .1767       .0333     5.3003        .0000   ← c path
b(MX)         .4750       .0578     8.2137        .0000   ← a path
b(YM.X)       .1518       .0267     5.6852        .0000   ← b path
b(YX.M)       .1046       .0346     3.0229        .0027   ← c' path

INDIRECT EFFECT AND SIGNIFICANCE USING NORMAL DISTRIBUTION
        Value    s.e.   LL 95 CI   UL 95 CI        Z   Sig(two)
Sobel   .0721   .0155      .0417      .1025   4.6514      .0000   ← ab

BOOTSTRAP RESULTS FOR INDIRECT EFFECT
         Mean    s.e.   LL 95 CI   UL 95 CI  LL 99 CI   UL 99 CI
Effect  .0721   .0165      .0422      .1072     .0352      .1195   ← ab

SAMPLE SIZE
    437

NUMBER OF BOOTSTRAP RESAMPLES                95% bootstrap confidence
    5000                                     interval for indirect effect

    - - - - - END MATRIX - - - - -
```

APPENDIX 23.2

The computer output below comes from the SPSS version of the mediation macro described in Preacher and Hayes (2008b). We recommend this macro even for simple mediation models, as it has more options than the Preacher and Hayes (2004) macro.

```
INDIRECT y=know/x=grats/m = newsattn elab/contrast = 1
/normal = 1/bca = 1/boot = 5000.

Run MATRIX procedure:
Dependent, Independent, and Proposed Mediator Variables:
DV    =  know
IV    =  grats
MEDS  =  newsattn
         elab

Sample size
      437

IV to Mediators (a paths)
              Coeff        se          t            p
newsattn      .4750     .0578     8.2137        .0000   ← a1 path
elab          .2796     .0571     4.8977        .0000   ← a2 path

Direct Effects of Mediators on DV (b paths)
              Coeff        se          t            p
newsattn      .1261     .0271     4.6586        .0000   ← b1 path
elab          .1081     .0274     3.9452        .0001   ← b2 path

Total Effect of IV on DV (c path)
              Coeff        se          t            p
grats         .1767     .0333     5.3003        .0000   ← c path

Direct Effect of IV on DV (c' path)
              Coeff        se          t            p
grats         .0866     .0343     2.5215        .0120   ← c' path

Model Summary for DV Model
R-sq       Adj R-sq         F        df1          df2        p
.1561         .1503   26.6989     3.0000     433.0000    .0000

*****************************************************************
```

```
                NORMAL THEORY TESTS FOR INDIRECT EFFECTS

Indirect Effects of IV on DV through Proposed Mediators (ab paths)
            Effect        se          Z          p
TOTAL        .0901      .0170      5.3106      .0000
newsattn     .0599      .0147      4.0639      .0000
elab         .0302      .0098      3.0802      .0021
C1           .0296      .0184      1.6092      .1076
```

$\leftarrow a_1b_1 + a_2b_2$ (TOTAL)
$\leftarrow a_1b_1$ (newsattn)
$\leftarrow a_2b_2$ (elab)
$\leftarrow a_1b_1 - a_2b_2$ (C1)

```
***********************************************************

                BOOTSTRAP RESULTS FOR INDIRECT EFFECTS

Indirect Effects of IV on DV through Proposed Mediators (ab paths)
              Data       Boot        Bias         SE
TOTAL        .0901      .0902       .0001      .0177
newsattn     .0599      .0599       .0000      .0164
elab         .0302      .0304       .0001      .0109
C1           .0296      .0295      -.0001      .0215

           Bias Corrected and Accelerated Confidence Intervals

             Lower      Upper
TOTAL        .0565      .1256
newsattn     .0306      .0959
elab         .0123      .0560
C1          -.0117      .0725
```

95% BCA bootstrap confidence interval for indirect effect

CI for difference between specific indirect effects

```
***********************************************************

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            newsattn     elab

- - - - - END MATRIX - - - - -
```

APPENDIX 23.3

Mplus code and output for the multiple-step multiple mediator model discussed in the text. The exclamation points in the code denote comments and should be removed to implement bootstrapping and to obtain bias-corrected confidence intervals for the indirect effects.

```
TITLE:   Multiple Step Multiple Mediator Model
DATA:
         FILE IS C:\CMM1985.dat;
         FORMAT IS free;
VARIABLE:
         NAMES ARE elab grats newsattn know;
         USEVARIABLES elab grats newsattn know;
ANALYSIS:
! BOOTSTRAP = 5000;
MODEL:
     newsattn ON grats;
     elab on grats newsattn;
     know ON newsattn elab grats;
MODEL INDIRECT:
     know IND newsattn grats;
     know IND elab grats;
     know IND elab newsattn grats;
OUTPUT:
! CINTERVAL(BCBOOTSTRAP);
```

MODEL RESULTS

	Estimate	S.E.	Est./S.E.	Two-Tailed P-Value	
NEWSATTN ON					
GRATS	0.475	0.058	8.236	0.000	← a_1 path
ELAB ON					
GRATS	0.167	0.059	2.805	0.005	← a_2 path
NEWSATTN	0.238	0.046	5.190	0.000	← a_3 path
KNOW ON					
NEWSATTN	0.126	0.027	4.680	0.000	← b_1 path
ELAB	0.108	0.027	3.963	0.000	← b_2 path
GRATS	0.087	0.034	2.529	0.011	← c' path
Intercepts					
KNOW	−0.398	0.121	−3.291	0.001	
NEWSATTN	0.892	0.170	5.237	0.000	
ELAB	2.691	0.168	15.991	0.000	

```
Residual
  Variances
  KNOW              0.100    0.007     14.782       0.000
  NEWSATTN          0.336    0.023     14.782       0.000
  ELAB              0.308    0.021     14.782       0.000

TOTAL INDIRECT, SPECIFIC INDIRECT EFFECTS

                                         Two-Tailed
       Estimate    S.E.  Est./S.E.       P-Value

Effects from GRATS to KNOW

Sum of
indirect   0.090   0.017      5.318        0.000     ← a1b1 + a2b2 + a1a3b2

Specific indirect

KNOW
NEWSATTN
GRATS      0.060   0.015      4.069        0.000     ← a1b1

KNOW
ELAB
GRATS      0.018   0.008      2.290        0.022     ← a2b2

KNOW
ELAB
NEWSATTN
GRATS      0.012   0.004      2.942        0.003     ← a1a3b2
```

The sections of the output below are from a rerunning of the model by requesting bootstrapping and bias corrected confidence intervals. This is accomplished by removing the exclamation points "!" from the lines in the code above.

```
CONFIDENCE INTERVALS OF TOTAL, TOTAL INDIRECT, SPECIFIC INDIRECT, AND
DIRECT EFFECTS

          Lower.5%    Lower 2.5%    Estimate    Upper 2.5%    Upper .5%

Effects from GRATS to KNOW

Sum of
indirect     0.047         0.058       0.090         0.127        0.140
```

```
Specific indirect
KNOW
NEWSATTN
GRATS        0.023        0.030        0.060        0.096        0.108
KNOW
ELAB
GRATS        0.002        0.005        0.018        0.039        0.047

KNOW
ELAB
NEWSATTN
GRATS        0.003        0.005        0.012        0.024        0.028
```

95% BC bootstrap confidence interval for indirect effect

APPENDIX 23.4

SPSS output from the MEDTHREE macro for estimating paths in a multiple-step multiple mediator model as discussed in the text. The macro and instructions on its use can be downloaded from http://www.comm.ohio-state.edu/ahayes/macros.htm.

```
MEDTHREE y = know/x = grats/m1 = newsattn/m2 = reflect/boot = 5000.
_ _ _ _ _ _ _ _ _ _ _ _ _ _ _ _ _ _

Run MATRIX procedure:

VARIABLES IN MEDIATION MODEL
  Y    know
  X    grats
  M1   newsattn
  M2   elab

DESCRIPTIVES STATISTICS AND PEARSON CORRELATIONS
              Mean        SD      know     grats   newsattn      elab
know         .5435     .3451    1.0000     .2463      .3277     .2807
grats       2.9134     .4810     .2463    1.0000      .3664     .2286
newsattn    2.2769     .6235     .3277     .3664     1.0000     .3021
elab        3.7185     .5883     .2807     .2286      .3021    1.0000

SAMPLE SIZE: 437
```

Model Path Estimates

		Coeff	SE	t	p
a1	:	.4750	.0578	8.2137	.0000
a2	:	.1666	.0596	2.7944	.0054
a3	:	.2379	.0460	5.1734	.0000
b1	:	.1261	.0271	4.6586	.0000
b2	:	.1081	.0274	3.9452	.0001
c	:	.1767	.0333	5.3003	.0000
c¢	:	.0866	.0343	2.5215	.0120

Indirect Effects (with bootstrap 95%CI and standard errors)

		Effect	LL95%CI	UL95%CI	BootSE
Total	:	.0901	.0575	.1262	.0177
M1	:	.0599	.0299	.0928	.0163
M2	:	.0180	.0038	.0371	.0086
M1&M2	:	.0122	.0043	.0225	.0047

------------------------ NOTES ------------------------

Number of Bootstrap Samples: 5000

NOTE

1. In all examples, our discussion is based on estimates of the direct, indirect, and total effects derived using available data rather than on population values of these effects. We discuss inference from the estimates to population values in a later section of this chapter.

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