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Maxwell and Cole (2007) showed that cross-sectional approaches to mediation typically generate substantially biased estimates of longitudinal parameters in the special case of complete mediation. However, their results did not apply to the more typical case of partial mediation. We extend their previous work by showing that substantial bias can also occur with partial mediation. In particular, cross-sectional analyses can imply the existence of a substantial indirect effect even when the true longitudinal indirect effect is zero. Thus, a variable that is found to be a strong mediator in a cross-sectional analysis may not be a mediator at all in a longitudinal analysis. In addition, we show that very different combinations of longitudinal parameter values can lead to essentially identical cross-sectional correlations, raising serious questions about the interpretability of cross-sectional mediation data. More generally, researchers are encouraged to consider a wide variety of possible mediation models beyond simple cross-sectional models, including but not restricted to autoregressive models of change.

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Mediation is a fundamental concept in many areas of psychology and other sciences because of its importance in addressing questions about causal mechanisms. In experimental as well as observational studies, investigators typically want to understand the intervening processes whereby some variable X causes some other variable Y. Over the past several decades, most empirical tests of mediation have been based on cross-sectional data and have involved methods initially developed by Kenny (1979) and described in more detail by Baron and Kenny (1986). More recently, these methods have been expanded by numerous authors (e.g., Kenny, Kashy, & Bolger, 1998; MacKinnon, 2008; MacKinnon, Fairchild, & Fritz, 2007; MacKinnon, Lockwood, Hoffman, West, & Sheets, 2002; Preacher & Hayes, 2004; Shadish, Cook, & Campbell, 2002; Shrout & Bolger, 2002).

Most original presentations of methods for studying mediation did not explicitly consider the role of time despite the fact that mediational processes necessarily develop over time. Interestingly, a clear counterexample was one of the earliest papers devoted specifically to mediation, namely that of Judd and Kenny (1981). They described the potential importance of longitudinal designs for studying mediation, and emphasized the bias that could occur from failing to control for prior assessments of the mediator and the outcome variable. However, their cautions were largely ignored not only by substantive researchers but also by methodologists. As a result, most substantive studies of mediation are based on cross-sectional data. For example, Maxwell and Cole's (2007) survey of the five American Psychological Association (APA) journals publishing the most articles studying mediation revealed that over half of the studies tested mediation with methods that did not allow time for an independent variable to have an effect on a dependent variable. After nearly 20 years of largely neglecting the role of time for studying mediation, methodological articles began to appear in the late 1990s, with authors arguing that because mediation invariably occurs over time, empirical investigations of mediation should take time into account (e.g., Cole & Maxwell, 2003; Collins, Graham, & Flaherty, 1998; Kenny, Korchmaros, & Bolger, 2003; Kraemer, Stice, Kazdin, & Kupfer, 2001; MacCallum & Austin, 2000; Maxwell & Cole, 2007; Tein, Sandler, MacKinnon, & Wolchik, 2004). Much of this work is an outgrowth of an earlier body of research by Gollob and Reichardt (1985, 1987, 1991), who emphasized the importance of time in the formation and interpretation of structural equation models (SEM). Another major impetus for this perspective comes from the MacArthur approach to mediation, which emphasizes the necessity of incorporating time into the study of mediation. For example, Kraemer, Kiernan, Essex, and Kupfer (2008) stated "Perhaps the most important implication of the MacArthur approach is the necessity of using longitudinal studies with at least two and usually three time points to establish moderators and mediators" (p. S106).

Despite recent methodological work arguing for longitudinal designs to study mediation, most substantive investigations of mediation continue to be based on cross-sectional designs. One reason for this apparent inconsistency is that only recently has work begun to examine the problems associated with using cross-sectional designs to study mediation. In particular, Maxwell and Cole (2007) showed mathematically that under certain conditions, cross-sectional designs almost always fail to capture true mediational processes. More specifically, they showed that a cross-sectional analysis typically suggests that M does not fully mediate the relation when in fact it does from a longitudinal perspective. A limitation of Maxwell and Cole's (2007) work, however, is that it focused only on complete mediation. In other words, they began with a longitudinal model where some variable M fully mediated the relation between a presumed cause X and a presumed effect Y. Our overarching goal in the present study was to address this limitation.

In most psychological research, complete mediation is rare because of the difficulty of identifying all possible mediators of complex psychological relations. In reality, any given variable M likely only partially mediates the relation between X and Y. Maxwell and Cole (2007) did not consider a situation where M is only a partial mediator. The accuracy of a cross-sectional design for studying partial mediation remains unclear. The main goal of the present paper was to extend the earlier work of Maxwell and Cole (2007) by examining the extent to which cross-sectional designs can be relied on to provide an accurate indication of the extent to which some variable M either partially or completely mediates the relation between two other variables, X and Y.

A fundamental fact sometimes overlooked about mediation is that it ultimately involves questions about causation. To what extent does X cause M? Does X also directly cause Y? Or are X and Y related solely because X causes M, which in turn causes Y? Conceptualizing mediation in terms of causality opens a variety of ways of investigating mediation. One method (and the focus of the present paper) involves an autoregressive framework. Our autoregressive method is a special type of SEM. We chose this focus for several reasons: (a) the popular Baron and Kenny (1986) approach relies on an SEM formulation; (b) an autoregressive model represents the most straightforward way to extend Baron and Kenny's approach to a longitudinal framework and thus may provide the best opportunity for cross-sectional analyses to be accurate; (c) SEM has a long tradition in psychology and the other social sciences; (d) authors such as Pearl (2009) and Mulaik (2009) have shown that under specified conditions, SEM can provide a rigorous basis for causal inference, thus making it potentially appropriate for assessing mediation; and (e) Pearl (2011) showed that a Structural Causal Model framework can under certain circumstances lead to a situation where standard SEM formulas provide appropriate definitions of direct and indirect effects from a causal perspective.

Nevertheless, it is important to realize that SEM constitutes only one possible model for causation, and hence only one of many models for mediation. In particular, Rubin's causal model (Rubin, 1974; see also Shadish, 2010; West & Thoemmes, 2010) provides an alternative framework that has led to valuable insights about alternative approaches for studying causation in general and mediation in particular. Frangakis and Rubin's (2002) principal stratification (PS) approach offers an alternative framework to SEM for studying mediation. Jo (2008) described connections between the PS and SEM approaches to mediation, and proposed a cross-translation approach, which allows parameters to be translated back and forth between the PS and SEM models. Jo pointed out that the practical difference between PS and SEM derives from different identifying assumptions; thus, each approach has its strengths and weaknesses. Of particular relevance for our purposes was that the PS approach typically invokes an exclusion restriction, which implies the absence of a direct effect of X on Y. As Imai, Keele, and Tingley (2010) pointed out, this assumption makes this approach "less than ideal for the causal mediation analysis used in social science research" (p. 314). They presented an alternative framework for mediation that included SEM as a special case, thus providing a viable approach that in general requires fewer assumptions than are implicit in SEM.

We also want to acknowledge the existence of other longitudinal models for studying mediation. For example, much attention has been devoted to studying mediation at the level of the individual, as can be done in multilevel modeling and latent growth curve frameworks (e.g., Bauer, Preacher, & Gil, 2006; Kenny et al., 2003; Raykov & Mels, 2007; Selig & Preacher, 2009). Not only may there be important advantages to studying mediation as a within-person process instead of as a between-person process, but multilevel and growth curve models can often allow for individual differences in meditational parameters, unlike the approach we developed here. McArdle (2009) provided an excellent review of a variety of latent variable models for studying change.

In summary, we chose an autoregressive SEM model to serve as the basis for our examination of possible bias in typical cross-sectional designs. We want to emphasize that our reason was not necessarily because SEM is always the best way to study mediation. Instead, we made this choice largely because of the historical precedence and popularity of this model, the fact that under certain conditions a formal causal framework shows that SEM provides an appropriate method for assessing causal mediation parameters, and the fact that it is conceptually more similar to typical cross-sectional SEM models than are other alternative mediation models, so it seemed likely that a cross-sectional analysis might fare better for an underlying autoregressive SEM model than for other more complex models. From this perspective, our intent is to give traditional cross-sectional analyses as much opportunity as possible to be successful. Our belief that cross-sectional analyses would fare worse for other types of models is only conjectural, and awaits additional research.

EXAMPLE

Suppose a developmental psychopathologist wants to understand why depressed mothers tend to have depressed children. One possible explanation is that depressed mothers may engage in problematic parenting behaviors, which in turn leads to depression in their children. Of course, problematic parenting may reflect only one of several mechanisms whereby depression in parents leads to depression in children, in which case parenting would only partially mediate the relation between maternal depression and child depression.

Further suppose that this developmental psychopathologist designs a study to investigate the extent to which parenting mediates the relation between maternal depression and child depression. Following the general norm, we assumed that data are collected in a cross-sectional design. The standard mediation analysis for a simple cross-sectional design depends on the three bivariate correlations between each pair of maternal depression (which we labeled X), parenting (which we labeled M), and child depression (which we labeled Y). For simplicity, we assumed that correlations can be found for latent variables, so as to avoid distortions due to measurement error. As an example to motivate our presentation, we assumed that the following correlations are observed in a large sample: (a) the correlation between maternal depression and parenting is .512, (b) the correlation between parenting and child depression is .481, (c) and the correlation between maternal depression and child depression is .247.

The standard mediation analysis as described by Baron and Kenny (1986) proceeds with a series of regression analyses. First, the mediator (i.e., parenting) is regressed on the independent variable (i.e., maternal depression). The ensuing standardized regression coefficient simply equals the XM correlation. The value of .512 in our example would be statistically significant even with a moderate sample size. Second, the dependent variable (i.e., child depression) is regressed on the independent variable. Although the corresponding correlation is smaller in our data than the correlation between maternal depression and parenting, it would still be statistically significant at the .05 level with a sample size of 65 or larger. Third, the dependent variable is regressed on the independent variable and the mediator. For our data, this analysis yields a standardized regression weight of .48 for parenting and a standardized regression weight of .00 (to two decimal places) for maternal depression.

The standard cross-sectional mediation analysis reveals that there is no direct effect of maternal depression on child depression once parenting has been taken into account. Thus, this analysis appears to confirm that parenting mediates the relation between maternal depression and child depression. In fact, the analysis leads to a conclusion that parenting completely mediates the relation because maternal depression is no longer related to child depression after controlling for parenting. Such a conclusion appears to be of obvious scientific importance because it suggests that the mechanism has been discovered whereby maternal depression leads to child depression.

The results in this example would appear to be straightforward and clear. Later in this paper, however, we show that this apparent conclusion can be dramatically incorrect. These cross-sectional correlations are entirely consistent with a longitudinal model of mediation where the true relations are very different. In fact, these correlations are consistent with a longitudinal model in which parenting has no mediational effect whatsoever on the relation between maternal depression and child depression. In this longitudinal model, the entire relation between maternal depression and child depression is due to a direct effect of maternal depression on child depression. In addition, parenting has no effect on child depression in this model. Alarmingly, the cross-sectional analysis suggests exactly the opposite conclusion. The analysis could be further refined by using such methods as bootstrapping and Prodclin to form confidence intervals and statistical tests of the indirect effect (MacKinnon, 2008; Preacher & Hayes, 2004, 2008; Shrout & Bolger, 2002), but such intervals and tests are likely to be inaccurate if the underlying estimate itself is biased, so our emphasis throughout this article is on bias.

Maxwell and Cole (2007) focused on the problems that arise from using cross-sectional methods to estimate longitudinal mediation under the special condition when M completely mediates the longitudinal X-Y relation. In the present study we extended this work in two important ways. Our first goal was to examine what happens in the more frequent case of partial mediation. Our second goal was to consider situations in which M is not really a longitudinal mediator at all; in such cases, can a cross-sectional analysis spuriously suggest that M is a mediator?

MEDIATION FROM TWO AUTOREGRESSIVE PERSPECTIVES ON CHANGE

We considered mediation from two autoregressive models, depicted in Figures 1 and 2. In Model 1 (Figure 1) all cross-lag paths occur over one unit of time. To the extent that the path from X at time t to M at time $t + 1$ is nonzero and the path from M at time $t + 1$ to Y at time $t + 2$ is also nonzero, M mediates the effect of X on Y. The direct effect of X on Y is reflected by the path coefficient c. Unlike Maxwell and Cole, we allowed for the possibility of direct and indirect effects of X on Y over time.

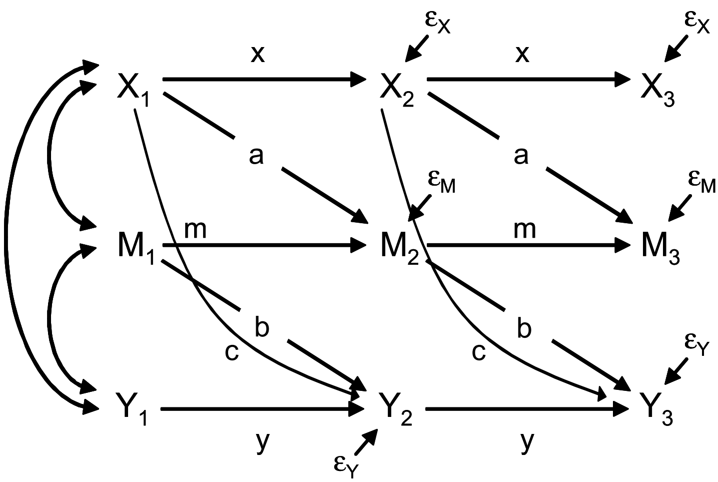

FIGURE 1 Longitudinal mediation model with one unit lag for direct effect of X on Y (Model 1).

The path diagram in Figure 1 can be written more formally in terms of the following equations:

$$M_{it+1} = mM_{it} + aX_{it} + \varepsilon_{Mit+1} \tag{1}$$

$$Y_{it+1} = yY_{it} + bM_{it} + cX_{it} + \varepsilon_{Yit+1} \tag{2}$$

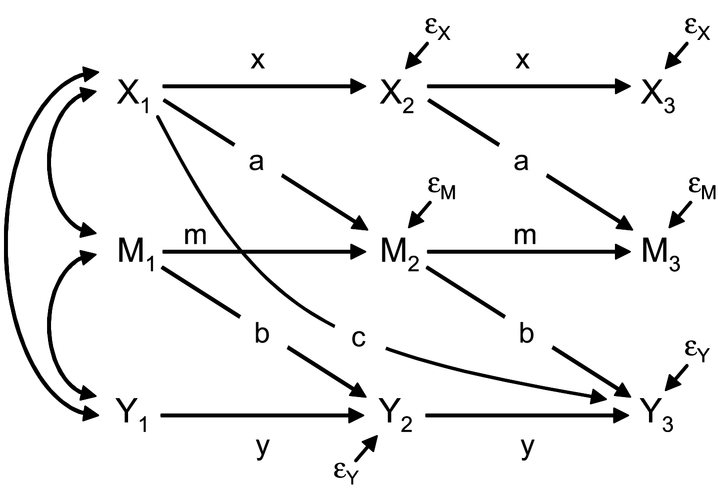

FIGURE 2 Longitudinal mediation model with two unit lag for direct effect of X on Y (Model 2).

where M_{it+1} is the score for individual i on variable M at time $t+1$, M_{it} is the score for individual i on variable M at the previous time point t, X_{it} is the score for individual i on variable X at time t, and ε_{Mit+1} is an error term reflecting other influences on M.

We assumed throughout that X, M, and Y at each time point have been standardized, which is why there is no intercept term in Equations (1) and (2). This also implies that m, a, b, y, and c are standardized coefficients. We also assumed throughout that all variables were latent variables or more generally variables measured without error. This provides a best-case assumption to evaluate the extent to which cross-sectional analyses can accurately recover parameters of longitudinal models. For simplicity, we also assumed that all parameter values were nonnegative.

The distinctive feature of Model 1 and Equation (2), as compared to Maxwell and Cole (2007), is the addition of a direct effect of X on Y. The time lag whereby X directly causes Y is the same as the lag whereby X causes M, and M in turn causes Y. Although this seems reasonable, it may also be important to consider a second perspective. According to Figure 1 and Equations (1) and (2), the indirect effect of X on Y takes two units of time, one for X to influence M and another for M to influence Y. If the indirect effect takes two units of time, an argument could be made for expecting the direct effect to take two units as well. This argument justifies Model 2 (see Figure 2). Model 2 assumes that the direct effect of X on Y takes two units of time. In the algebraic representation of this model, Equation (2) is replaced by Equation (3):

$$Y_{it+2} = yY_{it+1} + bM_{it+1} + cX_{it} + \varepsilon_{Yit+2} \tag{3}$$

In neither model do we make any assumptions about the absolute length of a unit of time. All that distinguishes Model 1 from Model 2 is the relative amount of time over which X directly affects Y.

The question of how best to consider time lags is clearly an unresolved issue in psychological research (Cole & Maxwell, 2009). In fact, one perspective offered by Gollob and Reichardt (1987) is that there is no true lag but instead the observed causal effect may simply depend on the lag chosen in any given situation. Taking a different perspective, econometricians have developed distributed lag models to deal with this issue. More recent interest has emerged in continuous time models, such as developed by Boker (2002, 2007) and Oud (2007).

Maxwell and Cole's (2007) literature review showed that many researchers rely on cross-sectional designs to study mediation. Figure 3 shows a typical path diagram depicting a situation where M appears to mediate the relation of X on Y in a cross-sectional design. To the extent that a' and b' are nonzero, M is said to mediate at least some of the effect of X on Y. To the extent that c'

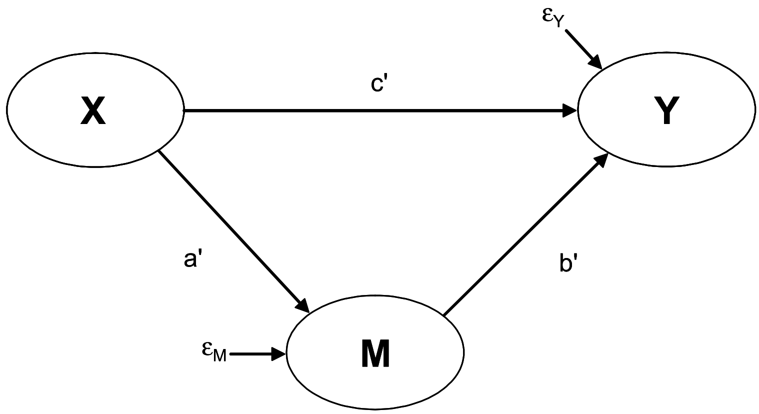

FIGURE 3 Cross-sectional mediation model.

is nonzero, X is said to have a direct effect of X on Y. Our basic question is how accurate are these assertions if in fact the true mediational process follows a longitudinal model, such as that depicted in either Figure 1 or 2. From the perspective of either Model 1 or 2, it may seem reasonable to expect a cross-sectional design to yield appropriate estimates of mediational effects under two specific conditions. First, the path coefficients connecting any pair of variables over time may be expected to be invariant to the choice of the particular point in time. For example, the effect of maternal depression on parenting may be the same in October as in March. Second, the system may have reached equilibrium, so that the correlations among X, M, and Y are the same at every time point. Under these conditions, a longitudinal design may seem unnecessary because a cross-sectional design in October would yield the same parameter estimates as a cross-sectional design in March.[1] The next sections of the article assess the extent to which this conjecture is correct.

Autoregressive Model 1

Estimating the cross-sectional direct effect of X *on* Y. Appendix A shows the derivations of the cross-sectional zero-order correlations among X, M, and Y from the model shown in Figure 1. All appendices are available at http://www.nd.edu/~smaxwell. These derivations assume that (a) X may have a direct effect and an indirect effect on Y, (b) all direct effects occur over one unit

[1] On the other hand, if these simplifying assumptions do not hold, a cross-sectional design would generate highly time-specific results that would call into question the utility of a cross-sectional design for completely different reasons.

of time, (c) the path coefficients a, b, x, m, y, and c are invariant over time, and (d) the system has reached equilibrium so that the cross-sectional correlations among X, M, and Y do not depend on the time of measurement. Under these conditions, the appendix shows that the population cross-sectional correlations are given by

$$\rho_{X_t M_t} = \frac{ax}{1 - mx} \tag{4}$$

$$\rho_{X_t Y_t} = \frac{cx + bx\rho_{X_t M_t}}{1 - xy} \tag{5}$$

$$\rho_{M_t Y_t} = \frac{ac + (ab + cm)\rho_{X_t M_t} + ay\rho_{X_t Y_t} + bm}{1 - my} \tag{6}$$

The essential question is how well do the cross-sectional parameters a', b', and c' as shown in Figure 3 accurately represent the underlying longitudinal mediational process. We begin with c', which represents the direct effect of X on Y controlling for M. Because the population value of c' in the cross-sectional analysis is a standardized regression coefficient, it can be derived from the correlations shown in Equations (4) through (6) as

$$c' = \frac{\rho_{X_t Y_t} - \rho_{X_t M_t}\rho_{M_t Y_t}}{1 - \rho^2_{X_t M_t}} \tag{7}$$

Appendix B shows that c' can be rewritten in terms of the longitudinal model parameters as

$$c' = \frac{(c + b\rho_{X_t M_t})(\rho_{X_t X_{t-1}} - \rho_{M_t M_{t-1}} + cm(1 - xy)(1 - \rho^2_{X_t M_t})}{(1 - \rho^2_{X_t M_t})(1 - xy)(1 - my)} \tag{8}$$

where $\rho_{X_t X_{t-1}}$ and $\rho_{M_t M_{t-1}}$ refer to the stability of X and M, respectively, between two adjacent time points. In other words, $\rho_{X_t X_{t-1}}$ is the correlation between X at any time t and X at the previous time point $t - 1$. Similarly, $\rho_{M_t M_{t-1}}$ is the correlation between M at any time t and M at the previous time point $t - 1$.

The bias in c' as a cross-sectional estimator of the longitudinal parameter c is of special interest. Subtracting c from c' as shown in Equation (8) and then simplifying terms shows that the difference between c' and c can be written as

$$c' - c = \frac{(c + b\rho_{X_t M_t})(\rho_{X_t X_{t-1}} - \rho_{M_t M_{t-1}})}{(1 - \rho^2_{X_t M_t})(1 - xy)(1 - my)} + \frac{cm}{1 - my} - c \tag{9}$$

In the special case of complete mediation (where $c = 0$), Equation (9) shows that as long as b and $\rho_{X_t M_t}$ are nonzero, c' equals c if and only if X and M are equally stable. However, this equation also shows that c' will not generally equal c even when X and M are equally stable unless c equals 0. In other words, when mediation is partial instead of complete, the cross-sectional parameter c' generally is different from the corresponding longitudinal parameter c.

Equation (9) also reveals that c' is sometimes larger than c, but other times it is smaller than c. In particular, the term immediately to the right of the equals sign is positive if X is more stable than M, but is negative if X is less stable than M. Furthermore, the difference between $cm/(1 - my)$ and c is positive if $m(y + 1)$ exceeds 1.0, but is negative otherwise. Thus, without detailed knowledge of unknown longitudinal parameter values, it is impossible to know whether the cross-sectional value c' is likely to be larger or smaller than the longitudinal parameter c.

The fact that c' is generally different from c still leaves open a question of whether the discrepancy is likely to be large enough to be of any scientific or practical significance. Table 1 shows examples of parameter values of c' and c under plausible conditions. Before discussing the scientific and practical implications of Table 1, we explain the table entries themselves. The rows of the table represent specific examples or combinations of values for the longitudinal parameters a, b, c, x, m, and y. For example, row 1 depicts an example where $a = 0.3$, $b = 0.3$, $c = 0.2$, $x = 0.9$, $m = 0.3$, and $y = 0.6$. Additional columns show the corresponding cross-sectional correlations ρ_{XM}, ρ_{MY}, and ρ_{XY}, calculated from Equations (4), (6), and (5), respectively. For Case 1, $\rho_{XM} = 0.37$, $\rho_{MY} = 0.38$, and $\rho_{XY} = 0.61$. Notice that across the various examples in Table 1, these correlations generally fall into a range that would be regarded as medium (0.30) to large (0.50) according to Cohen's (1988) conventions for effect sizes. The fact that some of the correlations are slightly larger than 0.50 is plausible because we assume throughout that these are correlations between latent variables, not manifest variables. The two rightmost columns in the table pertain to the direct effect. In particular, the column labeled "c'" shows the population value of the cross-sectional direct effect as calculated from Equation (8). The final column, labeled as "bias $(c' - c)$," shows the population difference between the direct effect c' calculated from the cross-sectional design and the longitudinal direct effect c.

We begin our interpretation of Table 1 by focusing on the odd-numbered rows. We subsequently return to the even-numbered rows. The odd-numbered rows represent examples in which the direct effect as calculated from a cross-sectional design provide a badly biased estimate of the actual longitudinal direct effect parameter c. Rows 1, 3, 5, 7, 9, and 11 show that for a variety of longitudinal parameter values, the direct effect calculated from a cross-sectional design can seriously overestimate the longitudinal direct effect parameter. For

TABLE 1
Bias in the Estimated Direct Effect of *X* on *Y*: Autoregressive Model 1

	Longitudinal Parameters[a]						*Cross-sectional Parameters*[b]				
Row	*a*	*b*	*c*	*x*	*m*	*y*	ρ_{XM}	ρ_{MY}	ρ_{XY}	c'	*Bias* $(c' - c)$
1	0.3	0.3	0.2	0.9	0.3	0.6	0.37	0.38	0.61	0.54	0.34
2	0.4	0.2	0.5	0.8	0.2	0.3	0.38	0.41	0.61	0.53	0.03
3	0.3	0.3	0.3	0.9	0.3	0.5	0.37	0.41	0.67	0.60	0.30
4	0.4	0.0	0.5	0.8	0.2	0.5	0.38	0.41	0.67	0.60	0.10
5	0.5	0.3	0.1	0.9	0.3	0.6	0.62	0.51	0.56	0.65	0.25
6	0.5	0.0	0.6	0.8	0.4	0.2	0.59	0.54	0.57	0.39	−0.21
7	0.3	0.4	0.2	0.8	0.3	0.6	0.32	0.40	0.50	0.42	0.22
8	0.3	0.1	0.6	0.7	0.5	0.2	0.32	0.41	0.51	0.43	−0.17
9	0.4	0.5	0.1	0.8	0.3	0.6	0.42	0.49	0.48	0.33	0.23
10	0.5	0.3	0.4	0.7	0.2	0.4	0.41	0.49	0.51	0.37	−0.03
11	0.4	0.2	0.3	0.8	0.3	0.6	0.42	0.48	0.59	0.47	0.17
12	0.5	0.1	0.6	0.7	0.2	0.3	0.41	0.50	0.57	0.43	−0.17
13	0.5	0.2	0.4	0.5	0.6	0.3	0.36	0.59	0.28	0.08	−0.32
14	0.3	0.5	0.1	0.7	0.6	0.4	0.36	0.58	0.27	0.07	−0.03
15	0.6	0.2	0.4	0.5	0.4	0.3	0.38	0.54	0.28	0.09	−0.31
16	0.3	0.4	0.1	0.7	0.6	0.5	0.36	0.54	0.26	0.08	−0.02
17	0.7	0.2	0.3	0.5	0.4	0.4	0.44	0.56	0.24	0.00	−0.30
18	0.5	0.4	0.1	0.6	0.5	0.5	0.43	0.55	0.23	−0.01	−0.11
19	0.7	0.2	0.4	0.5	0.3	0.3	0.41	0.56	0.28	0.07	−0.33
20	0.4	0.5	0.0	0.7	0.5	0.6	0.43	0.57	0.26	0.02	0.02
21	0.7	0.2	0.4	0.6	0.3	0.3	0.51	0.60	0.37	0.08	−0.32
22	0.5	0.5	0.1	0.7	0.4	0.5	0.49	0.60	0.37	0.10	0.00
23	0.5	0.2	0.4	0.6	0.6	0.3	0.47	0.65	0.36	0.07	−0.33
24	0.7	0.0	0.6	0.5	0.4	0.3	0.44	0.68	0.35	0.07	−0.53

[a]Hypothetical path coefficients for the longitudinal model depicted in Figure 1.
[b]The cross-sectional parameters that would emerge for the model in Figure 3 if the longitudinal model in Figure 1 were the true model.

example, row 1 shows an example where the actual value of c equals 0.20, but the population value of the cross-sectional direct effect equals 0.54, resulting in a bias of 0.34.

Rows 13, 15, 17, 19, 21, and 23 show examples in which the direct effect calculated from a cross-sectional design can badly underestimate the longitudinal direct effect. For example, row 13 shows a scenario where the actual value of c equals 0.40, but the population value of the cross-sectional direct effect is only 0.08, resulting in a bias of −0.32.

The fundamental implication of Table 1 is that cross-sectional designs cannot necessarily be expected to provide even a rough approximation to longitudinal direct effects under the autoregressive model shown in Figure 1. Even in a very large sample, a cross-sectional direct effect value can be either much smaller or much larger than the actual longitudinal direct effect.

Can we use cross-sectional correlations to anticipate the direction and magnitude of bias in the longitudinal direct effect? Even though cross-sectional direct effects can be badly biased, a cross-sectional design may be informative if the pattern of cross-sectional correlations could be used to discern the likely magnitude and direction of bias in c'. For example, it may be possible that a certain configuration of cross-sectional correlations would correspond to negative bias, whereas another configuration may correspond to positive bias. Unfortunately, an examination of adjacent odd and even rows in Table 1 shows this goal is not generally achievable. For example, consider row 1. The three correlations of $\rho_{XM} = 0.37$, $\rho_{MY} = 0.38$, and $\rho_{XY} = 0.61$ imply that $c' = 0.54$, when longitudinal $c = .20$. If we could know that this configuration of correlations is prone to a large positive bias, researchers could at least be aware that analyses obtained from this pattern of correlations should be interpreted accordingly. However, row 2 shows that almost exactly the same correlations can also correspond to very different longitudinal parameters. In particular, $c = 0.5$ in row 2, so here c' is almost an unbiased estimator of c. Closer inspection of Table 1 shows that for every odd-numbered row, the next even-numbered case depicts virtually identical cross-sectional correlations with very different values of the longitudinal direct effect parameter c.[2] The practical implication is that a given pattern of cross-sectional correlations may correspond to a wide range of values for the underlying longitudinal direct effect, essentially rendering any interpretation of the cross-sectional correlations as meaningless. Duplicate values arise here because multiple combinations of six longitudinal parameter values exist to fit the values of only three cross-sectional correlations. From the perspective of SEM, this is akin to an underidentified model, where it is impossible to find a unique solution for parameter estimates.

Estimating the cross-sectional indirect effect of X on Y. The previous sections demonstrated that cross-sectional estimates of the direct effect of X on Y can be seriously biased. We now turn our attention to cross-sectional estimation of the indirect effect. Appendix C shows that the indirect effect $a'\ b'$

[2]The reason the correlations in each even-numbered row are slightly different from the corresponding correlations in each odd-numbered row is because only model parameters with exactly one nonzero value to the right of the decimal are considered here. If model parameters were allowed to have a second nonzero value to the right of the decimal, the correlations could be made essentially the same. For example, row 2 parameter values of $a = 0.38$, $c = 0.50$, $x = 0.81$, $m = 0.20$, $b = 0.18$, and $y = 0.30$ lead to correlations of 0.37, 0.38, and 0.61, all of which are exactly the same as the row 1 correlations rounded to two decimal places.

in the cross-sectional analysis can be written as

$$\frac{(c + b\rho_{X_t M_t})(\rho_{M_t M_{t-1}} - x\rho^2_{X_t M_t} - xmy - xmy\rho^2_{X_t M_t}) - m(1 - \rho^2_{X_t M_t})(1 - xy)}{(1 - \rho^2_{X_t M_t})(1 - xy)(1 - my)} \quad (10)$$

Unfortunately, this expression is not easily interpreted and cannot be simplified. The very complexity of the expression, however, reveals that $a'b'$ will generally not equal ab, so cross-sectional analyses typically yield a biased estimate of the longitudinal indirect effect. Some specific examples help to clarify this point.

Table 2 shows examples of parameter values of $a'b'$ and ab under plausible conditions. The general format of this table is similar to that of Table 1, except

TABLE 2
Bias in the Estimated Indirect Effect of X on Y: Autoregressive Model 1

	Longitudinal Parameters[a]						*Cross-sectional Parameters*[b]				
Row	*a*	*b*	*c*	*x*	*m*	*y*	ρ_{XM}	ρ_{MY}	ρ_{XY}	$a'b'$	*Bias* $(a'b' - ab)$
1	0.50	0.30	0.0	0.7	0.6	0.6	0.60	0.53	0.22	0.37	0.22
2	0.45	0.10	0.1	0.7	0.7	0.7	0.62	0.50	0.22	0.36	0.32
3	0.50	0.20	0.3	0.7	0.5	0.5	0.54	0.66	0.44	0.32	0.22
4	0.50	0.00	0.7	0.6	0.7	0.1	0.52	0.67	0.45	0.31	0.31
5	0.50	0.20	0.2	0.7	0.6	0.5	0.60	0.63	0.35	0.40	0.30
6	0.70	0.40	0.1	0.7	0.3	0.5	0.62	0.60	0.37	0.37	0.09
7	0.70	0.05	0.3	0.6	0.7	0.7	0.41	0.67	0.33	0.27	0.25
8	0.70	0.20	0.4	0.5	0.4	0.4	0.44	0.69	0.30	0.30	0.16
9	0.50	0.20	0.4	0.7	0.6	0.3	0.60	0.72	0.46	0.42	0.32
10	0.65	0.50	0.2	0.7	0.3	0.4	0.58	0.71	0.47	0.38	0.05
11	0.50	0.20	0.2	0.7	0.6	0.6	0.60	0.73	0.39	0.47	0.37
12	0.55	0.50	0.2	0.7	0.5	0.2	0.59	0.70	0.40	0.42	0.14
13	0.60	0.60	0.0	0.5	0.3	0.6	0.35	0.44	0.15	0.16	−0.20
14	0.20	0.30	0.0	0.8	0.7	0.6	0.36	0.43	0.17	0.16	0.10
15	0.70	0.60	0.2	0.5	0.3	0.3	0.41	0.63	0.26	0.26	−0.16
16	0.50	0.00	0.3	0.5	0.7	0.7	0.38	0.61	0.23	0.24	0.24
17	0.70	0.60	0.0	0.5	0.3	0.4	0.41	0.42	0.15	0.18	−0.24
18	0.50	0.00	0.2	0.5	0.7	0.7	0.38	0.41	0.15	0.16	0.16
19	0.60	0.60	0.1	0.5	0.3	0.5	0.35	0.52	0.21	0.18	−0.18
20	0.15	0.50	0.0	0.9	0.7	0.3	0.36	0.49	0.22	0.17	0.10
21	0.50	0.60	0.1	0.5	0.3	0.5	0.29	0.44	0.18	0.12	−0.18
22	0.20	0.30	0.1	0.7	0.7	0.5	0.27	0.44	0.20	0.11	0.05
23	0.50	0.60	0.0	0.5	0.3	0.6	0.29	0.37	0.13	0.11	−0.19
24	0.15	0.25	0.0	0.8	0.7	0.7	0.27	0.39	0.12	0.10	0.07

[a]Hypothetical path coefficients for the longitudinal model depicted in Figure 1.

[b]The cross-sectional parameters that would emerge for the model depicted in Figure 3 if the longitudinal model in Figure 1 were the true model.

now the focus is on the indirect effect instead of the direct effect. In particular, the column labeled "$a'b'$" shows the population value of the cross-sectional indirect effect as calculated from Equation (10). The final column, labeled "bias ($a'b' - ab$)," shows the population difference between the indirect effect $a'b'$ calculated from the cross-sectional design and ab calculated from the corresponding longitudinal model.

As we did with Table 1, we begin our interpretation of Table 2 by concentrating on the odd-numbered rows. These rows of the table show that the indirect effect as calculated from a cross-sectional design can provide a badly biased estimate of the actual longitudinal indirect effect ab. Rows 1, 3, 5, 7, 9, and 11 show that for a variety of longitudinal parameter values, cross-sectional estimates of the indirect effect seriously overestimate the longitudinal indirect effect. For example, row 1 shows a scenario where the actual value of ab equals 0.15 but the cross-sectional value of $a'b'$ equals 0.37, thus yielding a population value that is 0.22 larger than the longitudinal value. Several other rows of the table show that even larger positive bias can occur.

Rows 13, 15, 17, 19, 21, and 23 show that cross-sectional estimates of the indirect effect can also seriously underestimate the longitudinal indirect effect. For example, row 13 shows an example where the actual longitudinal value of the indirect effect equals 0.36, but the population value of the cross-sectional indirect effect is only 0.16, resulting in a bias of −0.20.

In summary, Table 2 shows that cross-sectional designs cannot necessarily be expected to provide a good approximation of longitudinal indirect effects under the autoregressive model shown in Figure 1. As we saw for the direct effect, the direction of bias in the indirect effect is generally unpredictable. A cross-sectional design can seriously overestimate or underestimate the magnitude of a longitudinal indirect effect.

Can we use cross-sectional correlations to anticipate the direction and magnitude of bias in the longitudinal indirect effect? We saw earlier that any given set of cross-sectional correlations among X, M, and Y at a fixed point in time could possibly reflect either of two very different values of the longitudinal direct effect. Table 2 shows that the same is true of the indirect effect. For example, consider row 1. The three correlations of $\rho_{XM} = 0.60$, $\rho_{MY} = 0.53$, and $\rho_{XY} = 0.22$ imply that $a'b' = 0.37$. Row 2 reflects an alternate scenario where b is one third as large as in Row 1 and yet the cross-sectional correlations are virtually identical. Additional inspection of Table 2 shows that for every odd-numbered case, the next even-numbered case depicts virtually identical cross-sectional correlations with very different values for either or both of the longitudinal indirect effect parameters a and b. The practical implication is that a given pattern of cross-sectional correlations may correspond to a wide range of values for the underlying longitudinal indirect effect parameters, thus essentially rendering any interpretation of the cross-sectional correlations as virtually meaningless.

Does cross-sectional mediation necessarily reflect longitudinal mediation? A very important question arises when a cross-sectional analysis suggests that M mediates the effect of X on Y. To what extent can such an analysis be trusted? We show algebraically and numerically that it is quite possible for a cross-sectional analysis to imply complete mediation when, according to Model 1, there is actually no mediation whatsoever. We focus on a situation where there is no direct effect of M on Y, in which case $b = 0$, while allowing X to have a direct effect on M. For example, initial research may strongly support a conclusion that X influences an intermediate outcome M, but further work is needed to ascertain whether M then has an additional influence on Y, the outcome of ultimate interest.[3]

From an algebraic perspective, when $b = 0$, Equation (10) for the cross-sectional indirect effect $a'b'$ simplifies to

$$\frac{c(\rho_{M_t M_{t-1}} - x\rho^2_{X_t M_t} - xmy - xmy\rho^2_{X_t M_t}) - m(1 - \rho^2_{X_t M_t})(1 - xy)}{(1 - \rho^2_{X_t M_t})(1 - xy)(1 - my)} \tag{11}$$

Equation (11) shows that the cross-sectional indirect effect equals 0 only when both c and m equal zero, except for unlikely combinations of the other parameter values. The important practical implication is that even when $b = 0$ (so that no longitudinal mediation exists whatsoever), the cross-sectional indirect effect will quite likely be nonzero, falsely suggesting the existence of mediation.

Table 3 shows a variety of longitudinal parameters sharing only the fact that $b = 0$, so there is no direct effect of M on Y and hence no longitudinal mediation. The two rightmost columns show that a very different conclusion would be reached from a cross-sectional analysis. In all of the cases shown in the table, the cross-sectional population indirect effect is substantial, as reflected by sizable values of the product $a'b'$. Furthermore, the cross-sectional population direct effect, c', is zero to two decimal places. Of course, in a sample, c'

[3]We focus on $b = 0$ instead of $a = 0$ for two closely related reasons. First, it seems less likely that researchers may mistakenly infer that M mediates a relation between X and Y if $a = 0$ because, according to the autoregressive model, X and M are uncorrelated when $a = 0$. Unless X correlates with M, researchers are unlikely to infer mediation. Second, initial stages of research are more likely to reveal whether X and M are correlated. If not, it is unlikely that researchers may proceed to consider Y. Even if Y is initially included, mediation is unlikely to occur as an explanation when X and M are uncorrelated. On the other hand, it is easy to imagine scenarios where initial research shows that X and M are correlated, leading to later research investigating Y. In addition, if b is the only parameter equal to zero in the autoregressive model, all three cross-sectional bivariate correlations among X, M, and Y will be positive, and in fact all three correlations can be quite substantial, thus failing to provide a clue that the longitudinal indirect effect may be zero.

TABLE 3
Bias in the Estimated Indirect Effect of X on Y When $b = 0$: Autoregressive Model 1

	Longitudinal Parameters[a]						*Cross-sectional Parameters*[b]				
Row	a	b	c	x	m	y	ρ_{XM}	ρ_{MY}	ρ_{XY}	$a'b'$	c'
1	0.40	0.00	0.15	0.75	0.70	0.70	0.63	0.38	0.24	0.24	0.00
2	0.40	0.00	0.25	0.75	0.70	0.70	0.63	0.63	0.39	0.40	0.00
3	0.45	0.00	0.15	0.75	0.65	0.60	0.66	0.31	0.20	0.20	0.00
4	0.45	0.00	0.25	0.65	0.70	0.65	0.54	0.53	0.28	0.29	0.00
5	0.45	0.00	0.25	0.70	0.70	0.50	0.62	0.43	0.27	0.27	0.00
6	0.50	0.00	0.20	0.65	0.65	0.55	0.56	0.36	0.20	0.20	0.00
7	0.50	0.00	0.30	0.50	0.70	0.70	0.38	0.61	0.23	0.24	0.00
8	0.50	0.00	0.30	0.60	0.70	0.50	0.52	0.50	0.26	0.26	0.00
9	0.50	0.00	0.40	0.60	0.70	0.50	0.52	0.66	0.34	0.34	0.00
10	0.55	0.00	0.30	0.50	0.70	0.50	0.42	0.48	0.20	0.20	0.00
11	0.55	0.00	0.30	0.70	0.55	0.60	0.63	0.58	0.36	0.36	0.00
12	0.60	0.00	0.25	0.60	0.55	0.50	0.54	0.40	0.21	0.21	0.00
13	0.60	0.00	0.30	0.55	0.55	0.60	0.47	0.52	0.25	0.24	0.00
14	0.60	0.00	0.35	0.60	0.55	0.50	0.54	0.56	0.30	0.30	0.00
15	0.60	0.00	0.45	0.50	0.65	0.40	0.44	0.63	0.28	0.28	0.00
16	0.65	0.00	0.25	0.55	0.50	0.55	0.49	0.41	0.20	0.20	0.00
17	0.65	0.00	0.40	0.40	0.60	0.50	0.34	0.58	0.20	0.20	0.00
18	0.65	0.00	0.50	0.35	0.70	0.40	0.30	0.67	0.20	0.20	0.00
19	0.70	0.00	0.25	0.55	0.40	0.60	0.49	0.41	0.21	0.21	0.00
20	0.75	0.00	0.35	0.45	0.35	0.70	0.40	0.57	0.23	0.23	0.00

[a] Hypothetical path coefficients for the longitudinal model depicted in Figure 1.
[b] The cross-sectional parameters that would emerge for the model depicted in Figure 3 if the longitudinal model in Figure 1 were the true model.

may deviate from zero simply because of sampling error, but a statistical test would almost always identify this effect as nonsignificant. The combination of a sizable indirect effect with absolutely no direct effect would typically appear to imply that M completely mediates the relation between X and Y. Although this conclusion seems clear and may satisfy a researcher's desire to identify a mediating variable, the conclusion is exactly opposite in the longitudinal model. The fact that $b = 0$ implies that there is no longitudinal mediation whatsoever, contrary to the apparent implications of the cross-sectional analysis.

Autoregressive Model 2

Estimating cross-sectional direct and indirect effects. We now shift our attention to the second autoregressive model. Similar to the first model, Model 2 assumes that it takes 1 unit of time for X to influence M and also 1 unit of time for M to influence Y. However, unlike the first model, the second model assumes that it takes 2 units of time for X to influence Y directly, as shown in Figure 2. We take the same approach with Model 2 as we did with Model 1, but provide less detail because it is shown that the general pattern of findings is largely unchanged.

Appendix D shows the derivations of the cross-sectional zero-order correlations among X, M, and Y from the model shown in Figure 2. These derivations assume that (a) X may have a direct effect and an indirect effect on Y, (b) the direct effect of X on Y occurs over two units of time, (c) the path coefficients a, b, x, m, y, and c are invariant over time and (d) the system has reached equilibrium so that the cross-sectional correlations among X, M, and Y do not depend on the time of measurement. Under these conditions, Appendix D shows that the population cross-sectional correlations are given by

$$\rho_{X_t M_t} = \frac{ax}{1 - mx} \tag{12}$$

$$\rho_{X_t Y_t} = \frac{cx^2 + bx\rho_{X_t M_t}}{1 - xy} \tag{13}$$

$$\rho_{M_t Y_t} = \frac{acx + (ab + cm^2)\rho_{X_t M_t} + ay\rho_{X_t Y_t} + acmx + bm}{1 - my} \tag{14}$$

Once again, the essential question is how well do the cross-sectional parameters a', b' and c' as shown in Figure 3 accurately represent the underlying longitudinal mediational process. We begin with c', which represents the direct effect of X on Y controlling for M. Because the population value of c' in the cross-sectional analysis is a standardized regression coefficient, it can be derived from the correlations shown in Equations (12) through (14) as

$$c' = \frac{\rho_{X_t Y_t} - \rho_{X_t M_t}\rho_{M_t Y_t}}{1 - \rho^2_{X_t M_t}} \tag{15}$$

Appendix E shows that c' can be rewritten in terms of the longitudinal model parameters as

$$c' = \frac{(cx + b\rho_{X_t M_t})(\rho_{X_t X_{t-1}} - \rho_{M_t M_{t-1}}) + cm(1 - xy)(x - m\rho^2_{X_t M_t} - ax\rho_{X_t M_t})}{(1 - \rho^2_{X_t M_t})(1 - xy)(1 - my)} \tag{16}$$

Comparing the expression for c' in Equation (16) to the expression for c' in Model 1 (i.e., Equation 8) shows that although the expressions are not mathematically equivalent, they are similar to one another.

The bias in c' as a cross-sectional estimator of the longitudinal parameter c is of particular interest. Subtracting c from c' as shown in Equation (16) and then simplifying terms shows that the difference between c' and c can be written as

$$c' - c = \frac{(cx + b\rho_{X_t M_t})(\rho_{X_t X_{t-1}} - \rho_{M_t M_{t-1}})}{(1 - \rho^2_{X_t M_t})(1 - xy)(1 - my)} + \frac{cm(x - m\rho^2_{X_t M_t} - ax\rho_{X_t M_t})}{(1 - \rho^2_{X_t M_t})(1 - my)} - c \tag{17}$$

As is true in Model 1, c' will not generally equal c even when X and M are equally stable unless c equals 0. In other words, when mediation is partial instead of complete, the cross-sectional parameter c' will generally be different from the corresponding longitudinal parameter.

Equation (17) also reveals that c' sometimes is larger than c, but other times is smaller than c. Although the precise numerical values for bias in Model 2 are not identical to those in Model 1, the general pattern of results is very similar. For that reason, we refrain from presenting detailed tables of the bias in the direct effect. Instead, we briefly consider the bias in the indirect effect, and then focus on the critical question of whether mediation can appear to exist in a cross-sectional design even when no mediation whatsoever occurs in the longitudinal model.

Appendix F shows that the indirect effect $a'b'$ in the cross-sectional analysis can be written as

$$a'b' = \frac{\begin{array}{c} c\rho_{X_t M_t}((\rho_{X_t M_t}(m^2(1 - xy) - x^2(1 - my))) + ax(1 + m - mxy)) \\ + \; b\rho_{X_t M_t}(\rho_{M_t M_{t-1}} - mxy + mxy\rho^2_{XM} - x\rho^2_{X_t M_t}) \end{array}}{(1 - \rho^2_{X_t M_t})(1 - xy)(1 - my)} \tag{18}$$

The complexity of Equation (18) makes it all too apparent that the cross-sectional indirect effect $a'b'$ will rarely equal the longitudinal indirect effect ab. As a result, a cross-sectional design almost always yields a biased estimate of the longitudinal indirect effect. Examining specific examples for Model 2 yields results that are very similar to those for Model 1; consequently, we do not present additional details here.

Does cross-sectional mediation necessarily reflect longitudinal mediation? As we previously showed for Model 1, we now show that it is quite possible for a cross-sectional analysis to imply complete mediation when (a) Model 2 is the true model and (b) longitudinal mediation does not exist. As before, we focus on a situation where there is no longitudinal direct effect of M on Y because $b = 0$, yet X has a direct effect on M.

From an algebraic perspective, when $b = 0$, Equation (18) for the cross-sectional indirect effect $a'b'$ simplifies to

$$a'b' = \frac{c\rho_{X_t M_t}((\rho_{X_t M_t}(m^2(1-xy) - x^2(1-my))) + ax(1+m-mxy))}{(1-\rho^2_{X_t M_t})(1-xy)(1-my)}. \tag{19}$$

Equation (19) shows that the cross-sectional indirect effect equals 0 only when either c or ρ_{XM} equals 0 except for unlikely combinations of other parameter values. However, Equation (14) shows that if b and c equal 0, M and Y will be uncorrelated. Thus, the cross-sectional indirect effect equals 0 when $b = 0$ only if X and M are uncorrelated or M and Y are uncorrelated. Researchers will presumably not be terribly interested in studying mediation if either of these correlations is zero. The important practical implication is that even when $b = 0$ and thus there is literally no longitudinal mediation whatsoever, it is quite likely that the cross-sectional indirect effect is nonzero, falsely suggesting the existence of mediation.

Table 4 shows a variety of longitudinal parameters sharing only the fact that $b = 0$, so there is no effect of M on Y and hence no longitudinal mediation. The two rightmost columns, however, show that a very different conclusion would be reached from a cross-sectional analysis. In the cases shown in the table, the cross-sectional population indirect effect is substantial, as reflected by sizable values of the product $a'b'$. Furthermore, the cross-sectional direct effect, c', is zero to two decimal places. A sizable cross-sectional indirect effect in combination with absolutely no cross-sectional direct effect spuriously implies that M completely mediates the relation between X and Y, despite the fact that $b = 0$ in the longitudinal model precludes the possibility of any mediation whatsoever.

CONCLUSION

The principal conclusion of this article is that cross-sectional estimates of mediation typically generate biased estimates of longitudinal mediation parameters even in very large samples. Cross-sectional estimates can either seriously underestimate or overestimate longitudinal parameters. This conclusion holds for direct effects as well as indirect effects. Furthermore, the direction of bias is generally impossible to discern from the cross-sectional data.

An especially important conclusion is that cross-sectional analyses can imply the existence of a mediator variable when in reality there is no underlying longitudinal meditational process whatsoever. Cross-sectional data that seem to imply complete mediation can in fact reflect longitudinal data where there is a strong direct effect but a complete lack of mediation. In other words, the cross-sectional

TABLE 4
Bias in the Estimated Indirect Effect of X on Y When $b = 0$: Autoregressive Model 2

	Longitudinal Parameters[a]						*Cross-sectional Parameters*[b]				
Row	a	b	c	x	m	y	ρ_{XM}	ρ_{MY}	ρ_{XY}	$a'b'$	c'
1	0.40	0.00	0.25	0.80	0.65	0.65	0.67	0.50	0.33	0.33	0.00
2	0.40	0.00	0.40	0.70	0.70	0.50	0.55	0.55	0.30	0.30	0.00
3	0.45	0.00	0.35	0.60	0.65	0.60	0.44	0.45	0.20	0.20	0.00
4	0.45	0.00	0.40	0.80	0.60	0.55	0.69	0.66	0.46	0.46	0.00
5	0.45	0.00	0.50	0.70	0.65	0.35	0.58	0.56	0.32	0.32	0.00
6	0.50	0.00	0.20	0.70	0.55	0.70	0.57	0.34	0.19	0.20	0.00
7	0.50	0.00	0.40	0.60	0.60	0.50	0.47	0.44	0.21	0.21	0.00
8	0.50	0.00	0.40	0.75	0.55	0.60	0.64	0.65	0.41	0.41	0.00
9	0.50	0.00	0.50	0.55	0.60	0.55	0.41	0.53	0.22	0.22	0.00
10	0.55	0.00	0.35	0.65	0.50	0.65	0.53	0.48	0.26	0.26	0.00
11	0.55	0.00	0.60	0.50	0.55	0.55	0.38	0.56	0.21	0.21	0.00
12	0.60	0.00	0.35	0.65	0.45	0.55	0.55	0.42	0.23	0.23	0.00
13	0.60	0.00	0.45	0.60	0.45	0.60	0.49	0.51	0.25	0.25	0.00
14	0.60	0.00	0.45	0.65	0.45	0.60	0.55	0.57	0.31	0.32	0.00
15	0.60	0.00	0.55	0.60	0.45	0.60	0.49	0.62	0.31	0.31	0.00
16	0.65	0.00	0.40	0.60	0.40	0.50	0.51	0.40	0.21	0.20	0.00
17	0.65	0.00	0.50	0.50	0.40	0.70	0.41	0.48	0.19	0.20	0.00
18	0.65	0.00	0.60	0.50	0.40	0.60	0.41	0.52	0.21	0.21	0.00
19	0.70	0.00	0.50	0.55	0.35	0.50	0.48	0.44	0.21	0.21	0.00
20	0.75	0.00	0.60	0.55	0.30	0.40	0.49	0.48	0.23	0.24	0.00

[a] Hypothetical path coefficients for the longitudinal model depicted in Figure 2.
[b] The cross-sectional parameters that would emerge for the model depicted in Figure 3 if the longitudinal model in Figure 2 were the true model.

data can clearly suggest a meditational process when the longitudinal reality is that no mediation exists at all. Unfortunately, this raises serious questions about the value of preliminary cross-sectional studies as a prelude to more time-consuming and expensive longitudinal designs. Our results show that a variable that appears to be a candidate mediator based on a preliminary cross-sectional analysis may not be a mediator at all longitudinally, and just as troubling, a variable that appears not to be a candidate mediator cross-sectionally may in fact be an important mediator longitudinally.

A related limitation of cross-sectional designs for studying mediation is that a given pattern of cross-sectional correlations can arise from very different combinations of underlying longitudinal parameters. As a result, any attempt to infer the properties of the underlying longitudinal process on the basis of the cross-sectional parameters is almost certainly futile. Perhaps it should come as no surprise that accurate understanding of processes that develop over time is likely to demand longitudinal designs, but the literature shows that many researchers ignore this point when studying mediation.

The fundamental reason for the inability of the cross-sectional model to capture longitudinal processes is that it fails to represent effects of X on M and Y and of M on Y over time. As such, the cross-sectional model is misspecified, and parameter estimates are generally biased. In fact, Judd and Kenny (1981) explicitly pointed out many years ago that failing to include prior assessments of the mediator and the outcome could lead to bias. Reichardt and Gollob (1986) noted that the cross-sectional model is misspecified for a second reason: it fails to allow causation to occur over time and instead presumes that X at time t causes M at the same time t.

The fact that cross-sectional parameters generally differ from corresponding longitudinal parameters implies that cross-sectional hypothesis tests will also be biased (cf. Casella & Berger, 2002). When the population value of a longitudinal parameter is zero, we have seen that the corresponding cross-sectional parameter can be very different from zero. In such situations, Type I errors become almost inevitable, especially in large samples. Similarly, statistical tests can have very low power, because a cross-sectional parameter value close to zero can correspond to a sizable longitudinal parameter value.

Unfortunately, relying on confidence intervals is no better. The likelihood that a confidence interval around a biased cross-sectional parameter estimate will include the longitudinal parameter of interest will approach zero as the sample size increases. What appears to be a very accurate interval (because of a large sample size) may in fact have an upper and lower bound, neither of which is remotely close to the true longitudinal parameter value. The important practical point here is that the substantial bias that typically exists in cross-sectional analyses of mediation can render p values or confidence intervals obtained from cross-sectional data essentially meaningless.

As MacKinnon et al. (2002) emphasized, another important advantage of longitudinal designs is that they can yield information about temporal precedence, and thus allow examination of which variables are causes and which variables are effects. For example, although maternal depression may well contribute to child depression, the opposite can also be true. Longitudinal designs are especially well suited to examine such complex causal relations.

Several limitations of the present work suggest avenues for future researchers. First, in the present study we did not examine the role of time lag duration in the design and analysis of longitudinal studies. As has been discussed elsewhere (e.g., Cole & Maxwell, 2003; Gollob & Reichardt, 1985, 1987, 1991), estimates of effects in longitudinal models can change greatly depending on the chosen time lag. In this respect, continuous time models (e.g., Boker, 2002, 2007; Oud, 2007) offer an interesting alternative because parameter values are unaffected by choice of time lag between adjacent measurement occasions.

A second limitation is that in the present study we focused specifically on mediation when X, M, and Y are all changing over time. However, important special cases exist in which X is fixed in time. In experimental designs or intervention studies, where participants are assigned to treatment or control conditions, investigators are often interested in the potential mediators of the manipulation or treatment. In fact, a strong case can be made that this type of design provides the optimal approach for studying mediation, because random assignment ensures that effects of X on M and of X on Y (but not M on Y unless M is manipulated in a separate study) are in fact causal and not simply correlational. It should be noted that such designs are inherently longitudinal, because any posttest assessment necessarily occurs after the implementation of the intervention. Even so, a question exists about the necessity of measuring M and/or Y at multiple time points in this design.

Third, we have shown that cross-sectional estimates of meditational processes are biased only in a very special case where the true underlying model is presumed to be a longitudinal autoregressive model. In particular, we have not considered how accurate cross-sectional analyses may be when underlying processes correspond to a different type of model, such as a continuous time model, a potential outcomes model, or a multilevel model. Given that traditional cross-sectional analyses of mediation can be badly misleading for relatively simple underlying SEM models that in many ways are similar to popular cross-sectional SEM models, it seems unlikely that they will be more accurate for more complex types of underlying models. That said, further research is clearly needed to verify or disconfirm our expectations.

In summary, we find that cross-sectional approaches to longitudinal mediation can substantially over- or underestimate longitudinal effects even when longitudinal parameter estimates are completely stable, and there is no measurement error. Even cross-sectional correlations that appear to support complete mediation may in fact reflect a longitudinal process with no mediation whatsoever. As Judd and Kenny (1981) stated nearly 30 years ago, “While the estimation of longitudinal multiple indicator process models is complex, it is also likely to be quite rewarding, since only through such an analysis can we glimpse the process whereby treatment effects are produced” (p. 613). We urge researchers interested in mediational processes to collect multiple waves of data and use the increasing collection of data analytic methods that formally take the passage of time into account.

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