Multivariate Behavioral Research

Publication details, including instructions for authors and subscription information:
http://www.tandfonline.com/loi/hmbr20

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Published online: 14 Jun 2013.

Multivariate Behavioral Research, 48:301–339, 2013

ISSN: 0027-3171 print/1532-7906 online
DOI: 10.1080/00273171.2013.784696

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Mediational studies are often of interest in psychology because they explore the underlying relationship between 2 constructs. Previous research has shown that cross-sectional designs are prone to biased estimates of longitudinal mediation parameters. The sequential design has become a popular alternative to the cross-sectional design for assessing mediation. This design is a compromise between the cross-sectional and longitudinal designs because it incorporates time in the model but has only 1 measurement each of X, M, and Y. As such, this design follows the recommendation of the MacArthur group approach, which stresses the importance of multiple waves of data for studying mediation. These 2 designs were compared to see whether the sequential design assesses longitudinal mediation more accurately than the cross-sectional design. Specifically, analytic expressions are derived for the bias of estimated direct and indirect effects as calculated from the sequential design when the actual mediational process follows a longitudinal autoregressive model. It was found that, in general, the sequential design does not assess longitudinal mediation more accurately than the cross-sectional design. As a result, neither design can be depended on to assess longitudinal mediation accurately.

Mediation plays a fundamental role in psychology because it explores underlying processes. Once a relationship is found among variables, researchers want to know why they are related and that can be determined using a mediational

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analysis. Most studies involving mediation use cross-sectional data and use the approach developed by Baron and Kenny (1986) and elaborated on by numerous others (Kenny, Kashy, & Bolger, 1998; MacKinnon, Lockwood, Hoffman, West, & Sheets, 2002; Shrout & Bolger, 2002). However, mediation represents a mechanism of change and it would be appropriate to acknowledge the role time (or time sequencing) plays in mediation. As early as 1985, Gollob and Reichardt warned researchers that using cross-sectional data to test longitudinal effects will lead to biased estimates because cross-sectional data do not include any time lags. They went so far as to state that a cross-sectional model is a misspecified model of change (Gollob & Reichardt, 1987). More recently, researchers are beginning to stress temporal precedence in studies of mediation (Kazdin & Nock, 2003; Kraemer, Kiernan, Essex, & Kupfer, 2008; Kraemer, Stice, Kazdin, Offord, & Kupfer, 2001; MacKinnon, 2008; MacKinnon, Fairchild, & Fritz, 2007; Rose, Holmbeck, Coakley, & Franks, 2004). Also, articles have been published that explicitly study the effect of time sequencing in mediational studies (Cole & Maxwell, 2003; Collins, Graham, & Flaherty, 1998; Kraemer, Yesavage, Taylor, & Kupfer, 2000; MacCallum & Austin, 2000; Maxwell & Cole, 2007; Maxwell, Cole, & Mitchell, 2011).

An alternative to the cross-sectional design is the sequential design, where a presumed cause X is measured prior to the presumed mediator M, which in turn is measured prior to the presumed outcome variable Y. The temporal precedence of the sequential design is a defining characteristic of the MacArthur approach to mediation (Kraemer et al., 2008), which requires a mediator M to be measured after its presumed cause X and prior to its presumed effect Y. The main difference between the MacArthur and Baron and Kenny approach to test mediation is the emphasis on time in the definition of mediators (Kraemer et al., 2008). In a sequential design, "directional influences in a model are hypothesized as operating over some time interval, and fitting the model to observed data yields estimates of such effects" (MacCallum & Austin, 2000, p. 206). Further, "the interpretation of such effects is bolstered by the use of a design that allows appropriate time for the effects to occur" (MacCallum & Austin, 2000, p. 206).

To assess the prevalence of different designs for studying mediation, a literature review was conducted. The five American Psychological Association journals that Maxwell and Cole (2007) cited as publishing the most mediation studies were examined to assess the designs being used to test mediation; the journals are as follows: *Journal of Personality and Social Psychology*, *Journal of Consulting and Clinical Psychology*, *Journal of Applied Psychology*, *Health Psychology*, and *Developmental Psychology*. In 2006, these journals published 84 articles (92 studies) that mentioned tests of mediation in their titles or abstracts. Only 90 studies were examined because 1 study was a meta-analysis of studies of mediation and a second study examined moderation instead of mediation. Of the published studies, 36 (38%) were completely cross-sectional. Fifty-four (59%) were described by the authors of the original articles as longitudinal. However, this perspective is incomplete. The number of fully longitudinal studies was only 14 (15%). Another 13 (14%) studies failed to take advantage of the longitudinal design by averaging across waves or only using one wave of data, so they are actually cross-sectional. An additional 12 (14%) studies were designed as half-longitudinal studies (either X and M or M and Y are measured contemporaneously, so there is only partial allowance for time in the design; Cole & Maxwell, 2003). Finally, the remaining 15 studies (16%) used a sequential design, where they measured X (the presumed independent variable) at Time 1, M (the presumed mediator) at Time 2, and Y (the presumed outcome) at Time 3. The 15 studies that used the sequential design are shown in Table 1. Researchers may use this design if they are "interested in the pattern of influences operating over time among different variables" (MacCallum & Austin, 2000, p. 205). They went on further to state that "the sequence and timing of measurements are designed to allow for these hypothesized effects to operate" (MacCallum & Austin, 2000, p. 205). Figure 1 shows an example of a sequential design to assess mediation.

As noted by MacCallum and Austin (2000) in their discussion of longitudinal designs, "An important aspect of such designs and models is the desirability of including autoregressive influences" (p. 206). However, as shown in Figure 1, the sequential design of the MacArthur approach does not include autoregressive influences. Similarly, the 15 studies listed in Table 1 did not include autoregressive influences.

Why might a researcher use the sequential design instead of a fully longitudinal design that could include autoregressive effects? One reason is that constraints on time or money may exist. Time, cost, and effort are clearly less if X is measured only at Time 1, M is measured only at Time 2, and Y is measured only at Time 3 than if all three variables are measured at all three time points. Even if practical limitations do not arise, another possible reason for adopting

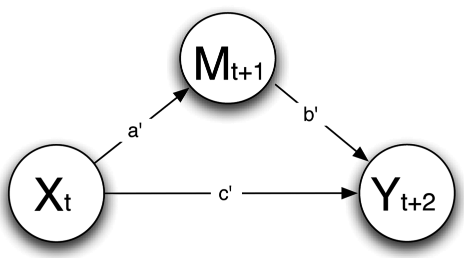

FIGURE 1 Sequential design mediation model.

TABLE 1
Authors That Published Articles Using the Sequential Design in 2006 in Five American Psychological Association Journals

Author(s)	*Article*	*Journal*
Srivastava et al.	Optimism in close relationships: How seeing things in a positive light makes them so	*Journal of Personality and Social Psychology*
Cury et al.	The social-cognitive model of achievement motivation and the 2 × 2 achievement goal framework	*Journal of Personality and Social Psychology*
Brown et al.	Proactive personality and the successful job search: A field investigation with college graduates	*Journal of Applied Psychology*
Edwards et al.	Relationships among team ability composition, team mental models, and team performance	*Journal of Applied Psychology*
Mathieu et al.	Empowerment and team effectiveness: An empirical test of an integrated model	*Journal of Applied Psychology*
Gong & Fan	Longitudinal examination of the role of goal orientation in cross-cultural adjustment	*Journal of Applied Psychology*
Porath & Bateman	Self-regulation: From goal orientation to job performance	*Journal of Applied Psychology*
Antoni, Lechner, & Kazi et al.	How stress management improves quality of life after treatment for breast cancer	*Journal of Consulting and Clinical Psychology*
Smits et al.	Cognitive mechanisms of social anxiety reduction: An examination of specificity and temporality	*Journal of Consulting and Clinical Psychology*
Gaudiano & Miller	Patients' expectancies, the alliance in pharmacotherapy, and treatment outcomes in bipolar disorder	*Journal of Consulting and Clinical Psychology*
Brody et al.	The strong African American families program: A cluster-randomized prevention trial of long-term effects and a mediational model	*Journal of Consulting and Clinical Psychology*
Reynolds et al.	Mediation of a middle school skin cancer prevention program	*Health Psychology*
Hampson et al.	Forty years on: Teachers' assessments of children's personality traits predict self-reported health behaviors and outcomes at midlife	*Health Psychology*
Chapman & Coups	Emotions and preventive health behavior: Worry, regret, and influenza vaccination	*Health Psychology*
Belsky et al.	Infant-mother attachment classification: Risk and protection in relation to changing maternal caregiving quality	*Developmental Psychology*

the simpler sequential design shown in Figure 1 may be that attempting to model all of the available data may pose its own difficulties. For example, suppose a researcher has collected (complete) data on latent variables (X, M, and Y) across three waves and therefore has a total of nine latent variables. Suppose further that each of those latent variables has at least three manifest variables for a total of at least 27 manifest variables. It may be extremely difficult to fit a fully longitudinal model with so many manifest variables. In addition, there could also be problems of nonconvergence and inadmissable parameter estimates. In that situation, a researcher may decide to limit the analysis to a subset of variables. Because three waves of data are available, it is natural to ask whether M at Time 2 mediates the effect of X at Time 1 on Y at Time 3. From this perspective, it might seem as if all other measures are not essential and that we only need to make use of each variable at a single point in time.

Our main question is whether researchers who use a sequential design instead of a longitudinal design can trust the results they obtain. Maxwell et al. (2011) examined the accuracy of cross-sectional designs for assessing mediation. They showed that cross-sectional designs often provide badly biased estimates of direct and indirect effects when the actual mediational process corresponds to a longitudinal autoregressive model. The primary purpose of the current article is to examine the accuracy of sequential designs for assessing mediation. Intuitively, it seems plausible that sequential designs will be superior to cross-sectional designs for assessing mediation because of the temporal precedence built into sequential designs. However, the absence of autoregressive effects in the model suggests the likelihood of biased parameter estimates, so on balance it is unclear how the sequential design may compare with the cross-sectional design for assessing mediation.

This article is a follow-up to Maxwell et al. (2011), which was published with three commentaries by Imai, Jo, and Stuart (2011), Reichardt (2011), and Shrout (2011). We note that the current article was under review before those commentaries were published. We now account for the excellent comments by the authors of the commentaries, but it should be emphasized that this article was originally written without benefit of those commentaries. In particular, astute readers can infer from Reichardt (2011) that the sequential design is likely to be biased under the conditions we study. However, it seems likely on purely intuitive grounds that the sequential design will at least succeed in producing less biased estimates of mediational effects than the corresponding cross-sectional design shown in Figure 2. If true, researchers might justify sequential designs as a reasonable way to approximate the estimation of mediational effects, especially if the bias that results from a sequential design tends to be small. Thus, our main goal is to assess the magnitude of bias in the sequential design and to evaluate how the sequential design compares not only with the cross-sectional design but also with the fully longitudinal design.

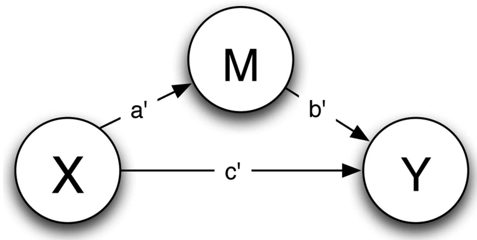

FIGURE 2 Cross-sectional mediation model.

MODELS OF CHANGE

Imai et al. (2011), Reichardt (2011), and Shrout (2011) have all recently emphasized that mediation reflects a causal process. Some variable X is presumed to cause M, which in turn is presumed to cause Y. These authors also emphasized that a number of causal models are possible for studying mediation. We have chosen to focus on an autoregressive framework, which is a special type of SEM, for several reasons: (a) Baron and Kenny (1986) is based on an SEM formulation, (b) Pearl (2012) has shown that a Structural Causal Model approach can be compatible with a situation where standard SEM formulas provide appropriate causal definitions of direct and indirect effects, and (c) analyses of data involving sequential designs are almost always analyzed as SEM models. By focusing on the autoregressive model, we do not intend to imply that other mediation models are misguided. Instead, our goal is to evaluate the sequential design in the context where it is most often used. Indeed, as we discuss in more detail later in the article, the sequential design can be thought of as a special case of the full longitudinal design on which we base our derivations, thus making it especially appropriate to evaluate how well the sequential design performs under more general conditions.

Suppose mediation follows an autoregressive model of change like the longitudinal mediation model in Figure 3 or Figure 4. Examination of the direct and indirect effect (and their potential biases[1]) are studied. Because this is central to this article, we briefly describe the longitudinal mediation models we consider. The time lag over which X directly causes Y will either be one unit of time (as shown in Figure 3) or two units of time (as shown in Figure 4). Mediation might occur over two lags because it is reasonable to believe that if it takes two units

[1]Expressions for biases are derived analytically because we have assumed the true or correct model. There are no simulations done for this project because we are not examining the biases that occur due to violations of assumptions. The bias in this case is the true effect subtracted from the expected value of the estimate of the effect.

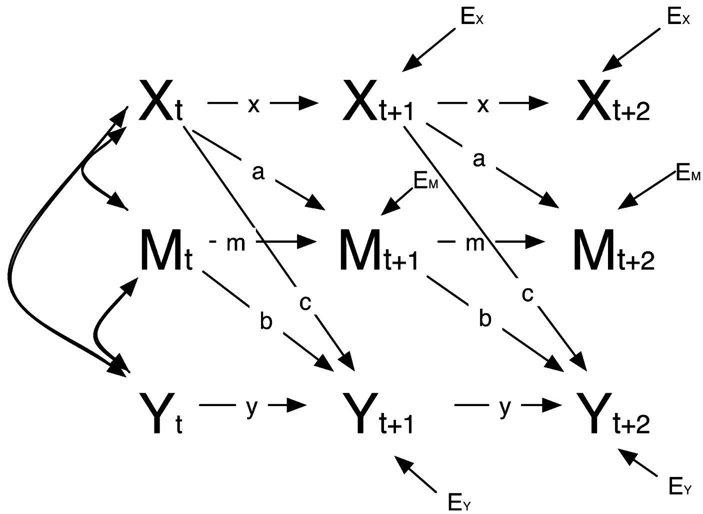

FIGURE 3 Longitudinal mediation model (mediation over one time lag).

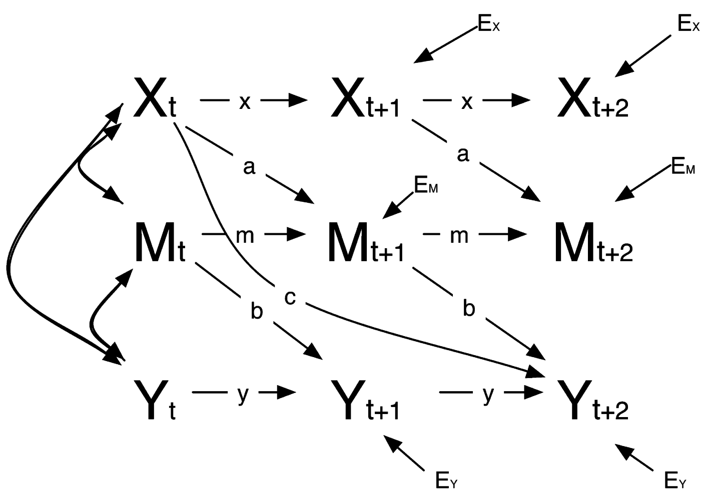

FIGURE 4 Longitudinal mediation model (mediation over two time lags).

of time for X to affect Y through M, then the direct effect of X on Y would also occur over two lags of time. However, if all variables in the model take only one unit of time to affect another variable (X_t takes one lag to affect X_{t+1} and M_{t+1}), it is reasonable to believe that the direct effect of X on Y should take only one lag. For both time lags, the first scenario that is studied is when mediation is complete, that is, $c = 0$ where c is the path from X_t to Y_{t+2} or the path from X_t to Y_{t+1}. Even though it is possible that M completely mediates the X-Y relationship, it is more likely that X also has some direct effect on Y. Therefore, the second scenario that is studied is when there is partial mediation, that is, $c \neq 0$.

Our derivations assume stationarity and equilibrium, as did the derivations of Maxwell and Cole (2007) and Maxwell et al. (2011). Stationarity refers to the extent to which the relationships among the variables stay the same over time (Kenny, 1979). For instance, the relationship between parent depression and child depression measured at one time point might be the same when measured at a different time point. Equilibrium can be described as occurring when there is temporal stability in covariance and variance patterns (Dwyer, 1983). Therefore, equilibrium holds if covariances are equivalent across waves. These are not unreasonable assumptions to make in this case because we are trying to assess the performance of the sequential design under a best case scenario. Another way to think about these assumptions is that they reflect the simplest model and if the sequential and cross-sectional designs are problematic in this case, there is no reason to believe that they would perform better in a more complex case.

CROSS-SECTIONAL ANALYSES OF CHANGE

If stationarity and equilibrium hold, it might appear to be appropriate to use a cross-sectional design and analysis. However, even if stationarity and equilibrium hold, Maxwell et al. (2011) proved that cross-sectional analyses do not generally reflect the longitudinal process that occurs in Figure 3 or Figure 4. Cross-sectional parameter estimates can either underestimate or overestimate the true longitudinal parameters, even in large samples (Maxwell et al., 2011). What may be more alarming is that cross-sectional analysis can imply the existence of a mediator when there is no true longitudinal mediation present; what may be a strong direct effect and no mediation can manifest itself as mediation in a cross-sectional model (Maxwell et al., 2011).

ALTERNATIVES TO CROSS-SECTIONAL ANALYSES

Given the limitations of cross-sectional designs for studying mediation, what should researchers do? It is understandable why researchers use cross-sectional studies to test mediation: there are many practical aspects to consider in designing a study. Cross-sectional designs do not require the passage of time so the problem of following the same group of people over any given time period is circumvented and data collection can be completed quickly. However, longitudinal studies are superior to test mediation. Longitudinal studies allow the researcher to include time in their analyses and control for autoregressive effects. Longitudinal data provide researchers with proof of temporal precedence, provide alternative explanations for mediated effects, and sometimes allow investigators to consider mediation from the perspective of more than a single underlying model (MacKinnon, 2008). Cross-sectional data provide a snapshot of a process but longitudinal data provide us with more information about the dynamic process.

The sequential design may appear to provide a reasonable compromise between the complexity of the full longitudinal design and the simplicity of the cross-sectional design. In particular, the sequential design shares an advantage of the longitudinal design in allowing for the passage of time, unlike the cross-sectional design. As such, it might seem to be more appropriate than the cross-sectional design for studying mediation. Indeed, this is a basic premise of the MacArthur approach (Kraemer et al., 2008), which argues that a serious limitation of the standard interpretation of the Baron and Kenny (1986) approach for studying mediators and moderators is that it fails to consider design issues. The Baron and Kenny approach to test mediation is usually implemented as

$$Y_t = cX_t + bM_t + \varepsilon_Y, \tag{1}$$

where Y_t is the dependent variable, ε_Y is the error, and M_t is the mediator where the mediator is expressed as

$$M_t = aX_t + \varepsilon_M, \tag{2}$$

where X_t is the independent variable and the errors, ε_M, are independent. The t subscript on X, M, and Y represents the time point where X, M, and Y are measured; all are measured at the same time point t in a cross-sectional design. The MacArthur framework stipulates, for mediation, that X must precede M, and M must precede Y, whereas the usual implementation of the Baron and Kenny approach implicitly assumes all variables are measured at the same time. In addition, the MacArthur approach includes the product XM as a predictor of Y but we assume that there is no X by M interaction for simplicity. Therefore, the MacArthur approach demands longitudinal data. In order for X to precede M, and M to precede Y, time needs to be incorporated in the data collection phase. In this model, like the Baron and Kenny approach, only one measurement each of X, M, and Y is needed but they need to be taken at different time points.

This approach can be expressed as

$$Y_{t+2} = cX_t + bM_{t+1} + \varepsilon_Y, \tag{3}$$

where the mediator is expressed as

$$M_{t+1} = aX_t + \varepsilon_M, \tag{4}$$

where the errors are independent. The t subscript in Equations 3 and 4 also represent the time of measurement. However, in Equations 3 and 4, the X, M, and Y subscripts show temporal precedence because only X is measured at time t, M is measured at some time after the initial time point, and Y is measured some time after M. Implicitly, the data analysis suggested in the MacArthur approach is based on the sequential design. As mentioned earlier, the MacArthur approach is a relatively new approach for studying mediation in psychology. One of the most accessible papers on this topic by Kraemer et al. (2008) appears in a 2008 issue of *Health Psychology*. Evidence of the emerging impact of the MacArthur approach is provided by the fact that this article has been cited over 100 times in the last 4 years according to Google Scholar. We believe that psychologists are using this approach without recognizing that formal literature is available to help guide them in designing their studies. For instance, Hoffmann (2004) used the MacArthur approach for his study and mentioned that he had a difficult time meeting the temporal precedence criteria required by the MacArthur approach.

As mentioned earlier, the sequential design involves measuring X, M, and Y, in that order, on three separate occasions. This is not a cross-sectional design because all measurements are not taken at a single time point and it is considered a special case of the longitudinal design. The sequential design flows logically and incorporates temporal precedence; therefore it should be explored as an alternative way to study mediation. Also, it has been used with some regularity by researchers without any real knowledge of how this compares with other methods. Furthermore, our literature review showed that mediation studies often use the sequential design without autoregressive effects. Thus it is possible that using this approach to study mediation is biased. The overarching goal of this article is to see how the sequential design compares with the cross-sectional design when testing longitudinal mediation and to see how well the sequential design assesses longitudinal mediation.

MEDIATION FROM AUTOREGRESSIVE POINT OF VIEW

It is known that cross-sectional designs can yield badly biased estimates of longitudinal mediation parameters. But how would the sequential design compare with the cross-sectional design? Would the sequential design perform better than the cross-sectional design or worse? In the remainder of this article, the implication of using the sequential design for longitudinal studies is of central focus—studying both the estimate of the direct effect of X on Y and the indirect effect of X on Y through M. The sequential design is then compared with its cross-sectional counterpart. This is done by assuming that the true model is a longitudinal autoregressive model. In all cases, mathematical derivations are used to find algebraic expressions for biases, and numerical analyses are employed to explore the likely magnitude of the biases.

ESTIMATING THE DIRECT EFFECT OF X ON Y: AUTOREGRESSIVE MODEL

Maxwell et al. (2011) found that cross-sectional estimates of the longitudinal mediation direct effect would be biased, often unpredictably so (sometimes underestimating the effect and other times overestimating the effect). A central question of this article is whether the sequential design provides less biased estimates than the cross-sectional design; to test this, focus is put on a three-wave autoregressive model where mediation occurs (refer to Figures 3 and 4). The only difference in Figures 3 and 4 is the time lag over which X directly affects Y. First, we look at a special case where the mediation is full or complete. Then we go into more likely cases where mediation is partial and can occur over one or two time lags.

Full Mediation Scenario

Appendix A shows the derivation of the correlations between X_t and M_{t+1}, M_{t+1} and Y_{t+2}, and X_t and Y_{t+2} in Figure 3 or Figure 4 where the c path is zero. Because $c = 0$, expressions for correlations based on either figure are the same. The derivations assume the same three conditions as Maxwell and Cole (2007): (a) M at time $t + 1$ fully mediates the relationship between X at time t and Y at time $t + 2$, (b) path coefficients a, b, m, x, and y are invariant over time; and (c) the system has reached equilibrium so correlations among the variables do not depend on the time of measurement. In addition, we assume that the variables of interest are latent variables to provide the best case scenario where variables are measured without error. Assumptions 2 and 3 remain constant throughout the article. Appendix A shows the population correlations for the sequential design. Because the sequential design is being compared with the cross-sectional design, it is important to consider the cross-sectional correlations that were derived by Maxwell and Cole. Interested readers are advised to refer to that paper for the derivations of cross-sectional correlations and corresponding parameter biases.

Direct effect bias. In the full mediation scenario, the true longitudinal direct effect equals zero. However, the population direct effect in the sequential and cross-sectional designs may or may not equal zero. Thus, it is important to determine how much bias, if any, exists in these designs. The expressions for the bias in the direct effect for the sequential and cross-sectional designs are

$$c'_{seq} = \frac{\rho_{X_t Y_{t+2}} - \rho_{X_t M_{t+1}} \rho_{M_{t+1} Y_{t+2}}}{1 - \rho^2_{X_t M_{t+1}}} \quad (5)$$

$$c'_{cs} = \frac{abx(x(1-m+m^2-a^2m)-m)}{(1-my)(1-xy)[(1-mx)^2-a^2x^2]} \quad (6)$$

(see Appendix B for the derivation of c'_{seq}). Ideally, it would be possible to compare Equations 11 and 12 algebraically to determine how the bias from the sequential design compares with the bias in the cross-sectional design as a function of the model parameters. Unfortunately, the complexity of these expressions precludes any simple algebraic comparison. Because these expressions are not directly comparable or interpretable, a numerical analysis was conducted. Before discussing the results at hand, a brief explanation of the numerical analysis that was done for this case and for the following cases is given.

No simulations were conducted because the expressions for biases could be found analytically. However, the practical implications of the analytical solutions are generally not immediately clear. Therefore, a numerical analysis was conducted in R 2.11.1. Reasonable values[2] for a, b, m, x, and y were chosen to correspond to plausible correlations among X, M, and Y. The correlations reported in the studies in Table 1 range between 0.00 and 0.87. Therefore, we chose parameter values that reflect that wide range of correlations.[3] Calculating the bias of the cross-sectional and sequential designs involved varying values for the six parameters of the presumed longitudinal model (or five parameters in the special case of full mediation). Values for a, b, c, m, x, and y were varied from 0.05 to 0.70 in a factorial design. For example, the parameter values for one cell of the design would be $a = .15$, $b = .50$, $c = .35$, $m = .05$, $x = .20$, and $y = .45$. These values were used to calculate the correlations between each pair of latent variables in the model, and then bias values were calculated from the

[2]Reasonable values for path coefficients are values that are more likely to occur in the population simultaneously. For all comparisons, appropriate path coefficient values were found using the model-implied correlation matrix (Bollen, 1989) and any combination of path coefficient values that led to off-diagonal elements greater than one or eigenvalues less than zero were not included.

[3]Cases that produce correlations greater than one are impossible because correlations are bounded between positive and negative one. Consider the following parameter values: $a = 0.60$, $b = 0.70$, $c = 0.25$, $m = 0.50$, $x = 0.80$, and $y = 0.40$ under the partial mediation over two lags scenario. This yields a correlation of 1.509 between X_t and Y_{t+2}, which cannot occur in reality; therefore this set of parameter values would be ignored and not used to calculate any biases.

correlations. We then considered another set of parameter values and calculated the bias for that combination. Given the number and range of parameter values, some combinations result in correlations of greater than 1 or less than −1. More generally, some combinations result in a correlation matrix that is not positive definite. Any time a set of parameters resulted in an impossible correlation matrix, that set of parameters was ignored (see Footnote 3 for an example). Finally, showing all viable combinations of parameter values in any given figure made it very different to interpret the figure because the plots were so dense in certain regions, so we randomly selected a subset of values to depict in each figure. Thus, the resultant figure is representative but not exhaustive of all possible results. To gain a greater understanding of the nuances of these mediation models, we created tables to mirror Maxwell and Cole (2007) and Maxwell et al. (2011) so that interested researchers could compare the findings across studies.

Figure 5 is a scatter plot of the absolute value of the biases in the cross sectional and sequential design. Examining Figure 5, it is immediately clear that there are three distinct clusters. When the stability of X is greater than the stability of M, the cross-sectional design tends to be more biased than the sequential design, and the opposite is true when the stability of M is greater than the stability of X. When the stability of X equals the stability of M, the cross-sectional design is not biased but the sequential design is biased. Regardless of the magnitude of the bias, both of these designs often estimate a nonzero direct effect when there is no direct effect in the population.

Table 2 considers the algebraic (signed) bias for illustrative values of the parameters. We created Table 2 to have the same parameter values as tables in Maxwell and Cole (2007) for continuity between studies. Therefore Table 2 does not reflect the same combination of parameter values as Figure 5 but instead reflects parameter values that were examined in previous studies. When the stability of X is greater than the stability of M, the sequential design will sometimes overestimate but other times underestimate the indirect effect. When the stability of M is greater than the stability of X, the sequential design will generally underestimate the direct effect. When the stabilities of X and M are equal, the cross-sectional design is not biased whereas the sequential design is biased. Table 2 as well as Figure 5 show that in general the bias from the sequential design can be substantial.

Partial Mediation Scenario

Because the true model in this case is presumed to be a three-wave autoregressive model, situations were studied when the direct effect occurs over one or two time lags. In both situations the interest is in comparing the sequential and cross-sectional designs when assessing longitudinal mediation. To be clear, one or two

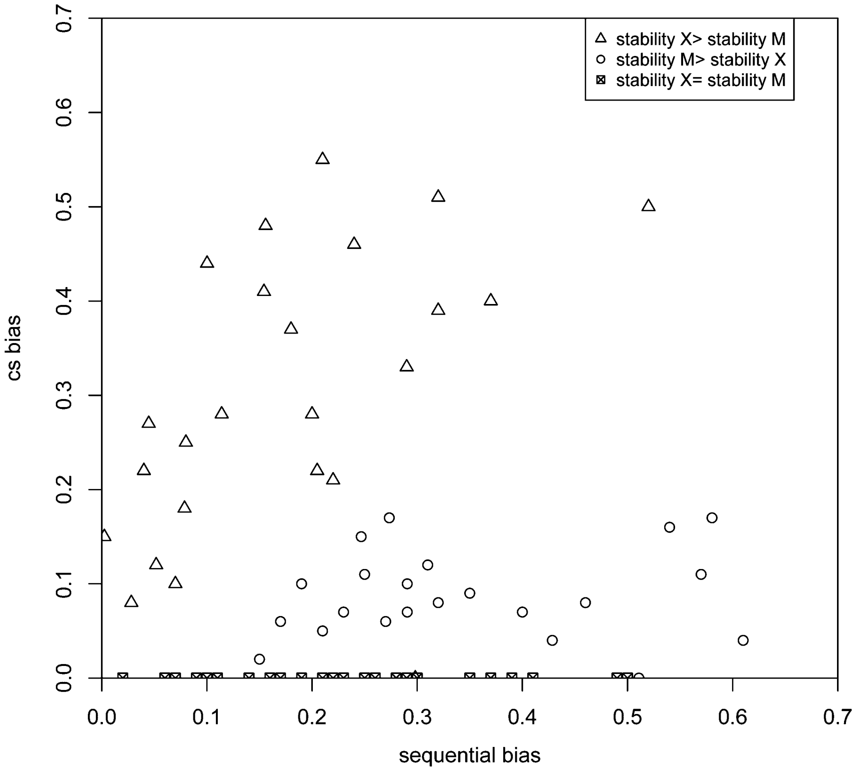

FIGURE 5 Scatter plot of biases in the direct effect-full mediation (FM) scenario.

time lags refers to the time lag it takes for the direct effect of X on Y. For the generated sets of parameter values, reasonable values for a, b, c, m, x, and y were chosen to reflect the range of correlations provided in the studies in Table 1 (i.e., parameter values that will have correlations for X, M, and Y between 0.00 and 0.87). Essentially, the same data generation procedure was followed as in the full mediation scenario with the exception that c is no longer constrained to be zero.

Partial Mediation Over One Lag Scenario

Appendix C shows the derivation of the correlations between X_t and M_{t+1}, M_{t+1} and Y_{t+2}, and X_t and Y_{t+2} in Figure 3 where the c path is not necessarily zero. Maxwell et al. (2011) presented the cross-sectional correlations for this case.

TABLE 2
Bias in the Estimated Direct Effect of the Cross-Sectional and Sequential Designs When There Is Full Mediation

Stability of X	*Stability of M*	a	b	c	x	m	y	c'_{cs}	c'_{seq}
1	0.7	0.5	0.4	0	1	0.33	0.5	0.48	0.23
1	0.5	0.5	0.4	0	1	0.19	0.5	0.44	0.22
1	0.5	0.5	0.4	0	1	0.04	0.5	0.41	0.20
0.9	0.7	0.5	0.4	0	0.9	0.36	0.5	0.22	0.15
0.9	0.5	0.5	0.4	0	0.9	0.22	0.5	0.27	0.17
0.9	0.3	0.5	0.4	0	0.9	0.06	0.5	0.28	0.17
0.8	0.7	0.5	0.4	0	0.8	0.40	0.5	0.08	0.07
0.8	0.5	0.5	0.4	0	0.8	0.25	0.5	0.15	0.12
0.8	0.3	0.5	0.4	0	0.8	0.09	0.5	0.18	0.13
0.7	0.7	0.5	0.4	0	0.7	0.45	0.5	0.00	−0.01
0.7	0.9	0.5	0.4	0	0.7	0.60	0.5	−0.17	−0.29
0.7	0.8	0.5	0.4	0	0.7	0.52	0.5	−0.07	−0.08
0.7	0.7	0.5	0.4	0	0.7	0.45	0.5	0.00	−0.01
0.6	0.9	0.5	0.4	0	0.6	0.65	0.5	−0.17	−0.31
0.6	0.8	0.5	0.4	0	0.6	0.57	0.5	−0.09	−0.14
0.6	0.7	0.5	0.4	0	0.6	0.49	0.5	−0.04	−0.06
0.5	0.9	0.5	0.4	0	0.5	0.71	0.5	−0.15	−0.32
0.5	0.8	0.5	0.4	0	0.5	0.62	0.5	−0.10	−0.18
0.5	0.7	0.5	0.4	0	0.5	0.53	0.5	−0.06	−0.09

Direct effect bias. The expressions for the direct effect bias for the sequential and cross-sectional designs are

$$c'_{seq} = \frac{\rho_{X_t Y_{t+2}} - \rho_{X_t M_{t+1}} \rho_{M_{t+1} Y_{t+2}}}{1 - \rho^2_{X_t M_{t+1}}} - c \tag{7}$$

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

(see Appendix D for the derivation for c'_{seq}). Given the complexity of the expressions for the direct effect in both designs, little can be ascertained from the expressions themselves.

Figure 6 is a scatter plot of the absolute value of the biases in the cross-sectional and sequential design. Comparing Figure 6 with Figure 5 reveals that there are very clear differences between the full and partial mediation cases. Unlike the full mediation case, there are no longer distinct clusters of data. In some cases, regardless of the stability, the cross-sectional and sequential designs

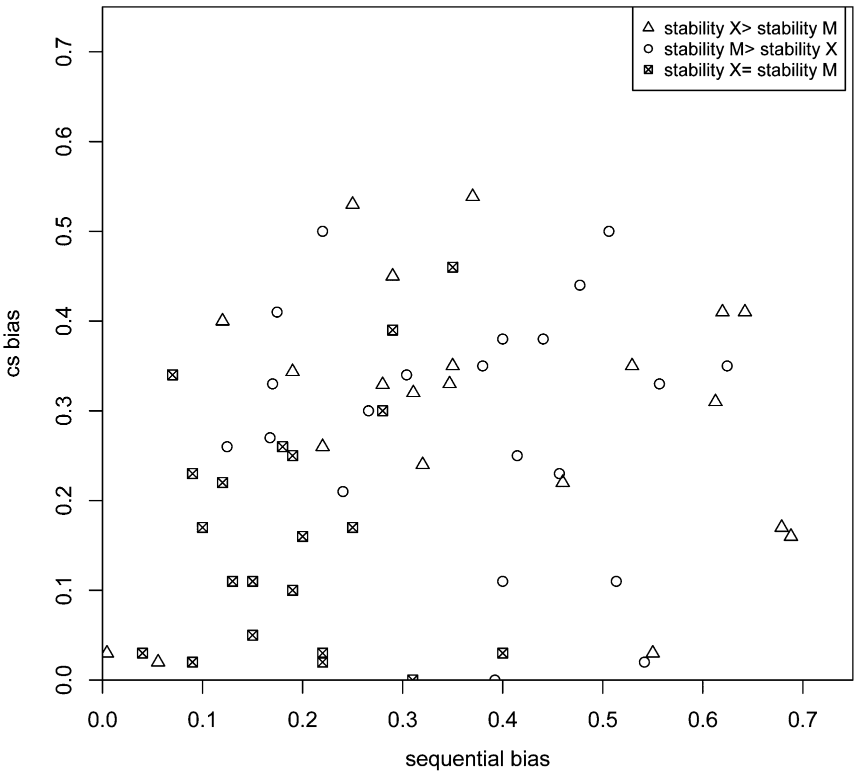

FIGURE 6 Scatter plot of biases in the direct effect-partial mediation (PM) over one lag scenario.

have approximately the same value because some points generally fall near a 45-degree line from the origin indicating that the biases produced by both designs are often similar. When the stability of X is greater than the stability of M, both designs can produce highly biased estimates. When the stability of M is greater than the stability of X, the bias in both designs is less than if the stability of X is greater than the stability of M, as a majority of those points are scattered across the central portions of the graph. When the stability of X is greater than the stability of M, the sequential design is more biased than the cross-sectional design 50% of the time for this set of parameter values. When the stability of M is greater than the stability of X, the sequential design is more biased than the cross-sectional design 62% of the time. When the stability of X and M are equal, the sequential design is more biased than the cross-sectional design 57% of the time.

TABLE 3
Bias in the Estimated Direct Effect of the Cross-Sectional and Sequential Designs When Mediation Occurs Over One Lag

a	b	c	x	m	y	c'_{cs}	c'_{seq}	*cs bias*	*seq bias*
0.3	0.3	0.2	0.9	0.3	0.6	0.54	0.55	0.34	0.35
0.4	0.2	0.5	0.8	0.2	0.3	0.53	0.62	0.03	0.12
0.3	0.3	0.3	0.9	0.3	0.5	0.60	0.62	0.30	0.32
0.4	0	0.5	0.8	0.2	0.5	0.60	0.81	0.10	0.31
0.5	0.3	0.1	0.9	0.3	0.6	0.65	0.4	0.55	0.30
0.5	0	0.6	0.8	0.4	0.2	0.39	0.62	−0.21	0.02
0.3	0.4	0.2	0.8	0.3	0.6	0.42	0.49	0.22	0.29
0.3	0.1	0.6	0.7	0.5	0.2	0.43	0.56	−0.17	−0.04
0.4	0.5	0.1	0.8	0.3	0.6	0.33	0.36	0.23	0.26
0.5	0.3	0.4	0.7	0.2	0.4	0.37	0.54	−0.03	0.14
0.4	0.2	0.3	0.8	0.3	0.6	0.47	0.64	0.17	0.34
0.5	0.1	0.6	0.7	0.2	0.3	0.43	0.65	−0.17	0.05
0.5	0.2	0.4	0.5	0.6	0.3	0.08	0.28	−0.32	−0.12
0.3	0.5	0.1	0.7	0.6	0.4	0.07	0.12	−0.03	0.02
0.6	0.2	0.4	0.5	0.4	0.3	0.09	0.31	−0.31	−0.09
0.3	0.4	0.1	0.7	0.6	0.5	0.08	0.14	−0.02	0.04
0.7	0.2	0.3	0.5	0.4	0.4	0.00	0.14	−0.30	−0.16
0.5	0.4	0.1	0.6	0.5	0.5	−0.01	0.05	−0.11	−0.05
0.7	0.2	0.4	0.5	0.3	0.3	0.07	0.30	−0.33	−0.10
0.4	0.5	0	0.7	0.5	0.6	0.02	0.02	0.02	0.02
0.7	0.2	0.4	0.6	0.3	0.3	0.08	0.35	−0.32	−0.05
0.5	0.5	0.1	0.7	0.4	0.5	0.10	0.17	0.00	0.07
0.5	0.2	0.4	0.6	0.6	0.3	0.07	0.31	−0.33	−0.09
0.7	0	0.6	0.5	0.4	0.3	0.07	0.44	−0.53	−0.16

Parameter values that were examined in Maxwell et al. (2011) were used to create Table 3. In Table 3, both the sequential and cross-sectional designs overestimate and underestimate the direct effect. The magnitude of bias tends to be larger in the cross-sectional design. In some cases the sequential design is close in absolute value to the true direct effect. There are more cases here where the cross-sectional design is more biased than the sequential design although both designs provide biased estimates of the direct effect.

Partial Mediation Over Two Lags Scenario

Appendix E shows the derivation of the correlations between X_t and M_{t+1}, M_{t+1} and Y_{t+2}, and X_t and Y_{t+2} for the model in Figure 4. The population cross-sectional design correlations are provided in Maxwell et al. (2011).

Direct effect bias. The expressions for the direct effect bias for the sequential and cross-sectional design are

$$c'_{seq} = \frac{\rho_{X_t Y_{t+2}} - \rho_{X_t M_{t+1}} \rho_{M_{t+1} Y_{t+2}}}{1 - rho^2_{X_t M_{t+1}}} - c \tag{9}$$

$$c'_{cs} = \frac{\begin{array}{c}(cx + b\rho_{X_t M_t})(\rho_{X_t X_{t-1}} - \rho_{M_t M_{t-1}}) \\ + cm(1 - xy)(x - m\rho^2_{X_t M_t} - ax\rho_{X_t M_t})\end{array}}{(1 - \rho^2_{X_t M_t})(1 - my)(1 - xy)} - c \tag{10}$$

(see Appendix F for the derivation for c'_{seq}). The direct effect bias in both cases cannot be easily explained or simplified.

The results for this case are similar to that of the one lag case both for the cross-sectional and sequential designs. The magnitude of the bias tends to be larger in the sequential design than the cross-sectional design although both often provide poor estimates of the true direct effect. Looking at a scatter plot, similar to Figure 5, did not enhance understanding of this scenario beyond the previous scenario. Because this pattern is evident and in some cases the results are redundant, no table or plot is provided for this case. However, we include percentages of biases that correspond to the different stability conditions. Looking within different stability scenarios, when the stability of X is greater than the stability of M, the sequential design is more biased than the cross-sectional design 48% of the time. When the stability of M is greater than the stability of X, the sequential design is more biased than the cross-sectional design 60% of the time. When the stability of X and M equal each other, the sequential design is more biased than the cross-sectional design 53% of the time.

ESTIMATING THE INDIRECT EFFECT OF X ON Y: AUTOREGRESSIVE MODEL

From the previous section, we can state that the sequential and cross-sectional designs can yield very different estimates of the direct effect and that the estimates can also be severely biased. We now turn our attention to the indirect effect.

Full Mediation Scenario

When the longitudinal path c is equal to zero, the general expression for the difference between the indirect effect ab in the longitudinal model and $a'b'$ in the sequential design is

$$bias_{seq} = a'b' - ab = \rho_{X_t Y_{t+2}} - c'_{seq} - ab. \tag{11}$$

From Maxwell and Cole (2007), the bias for the cross-sectional design is equal to

$$bias_{cs} = a'b' - ab = \rho_{X_tY_t} - c'_{cs} - ab. \tag{12}$$

Indirect effect bias. The expressions for the indirect effect bias for the sequential and cross-sectional designs are

$$bias_{seq} = \frac{ab}{(1-mx)(1-xy)} - c'_{seq} - ab \tag{13}$$

$$bias_{cs} = \frac{ab[x^2 - (1-mx)(1-xy)]}{(1-mx)(1-xy)} - c'_{cs}, \tag{14}$$

where Equation 13 follows from Equation 11. Refer to Maxwell and Cole (2007) for the derivation of $bias_{cs}$. Similar to the direct effect bias case, these expressions are neither comparable nor easily interpretable. Numerical analysis was again employed to compare the indirect effect bias in the sequential and cross-sectional design.

Figure 7 is a scatter plot of the absolute value of the biases in the cross-sectional and sequential design. Examining Figure 7, there seems to be evidence that when the stability of X and M are equal, the bias tends to be worse in the sequential design. Otherwise, results seem less systematic.

Table 4 reveals a pattern hidden by the presentation of absolute bias values in Figure 6. Namely, for the set of parameter values depicted in the table, the cross-sectional design usually underestimates the indirect effect whereas the sequential design overestimates it. In addition, it is clear that both designs can produce substantially biased estimates of the indirect effect.

Partial Mediation

Partial Mediation Over One Lag

Indirect effect bias. The expressions for the indirect effect bias for the sequential and cross-sectional design are

$$bias_{seq} = cx + \frac{ab}{(1-mx)(1-xy)} + \frac{cy}{1-xy} - c'_{seq} - ab \tag{15}$$

$$bias_{cs} = \frac{(c + b\rho_{X_tM_t})(\rho_{M_tM_{t-1}} - x\rho^2_{X_tM_t} - xmy - xmy\rho^2_{X_tM_t}) - m(1-\rho^2_{X_tM_t})(1-xy)}{(1-\rho^2_{X_tM_t})(1-xy)(1-my)} - ab, \tag{16}$$

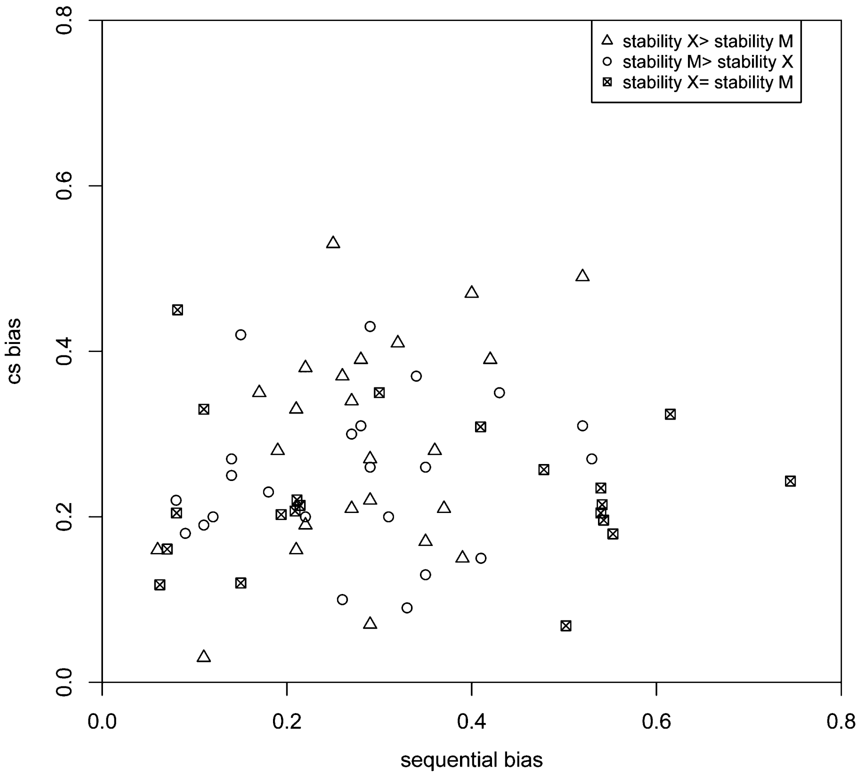

FIGURE 7 Scatter plot of biases in the indirect effect-full mediation (FM) scenario.

where Equation 15 follows from Equation 11. Refer to Maxwell et al. (2011) for the derivation of $bias_{cs}$. Given the nature of Equations 15 and 16 a numerical analysis was conducted to compare the indirect effect bias. Unlike in Equations 13 and 14, c is no longer zero and thus plays a role in the indirect effect bias.

Figure 8 is a scatter plot of the absolute value of the biases in the cross-sectional and sequential design. From Figure 8, there is no pattern evident to determine the bias in the sequential and cross-sectional designs. It is clear that for most cases, the bias will tend to be between 0.10 and 0.40 for both designs. It appears that the bias in the sequential design can approach 0.60 whereas the cross-sectional design bias can be as large as approximately 0.40. Summarizing Figure 8, when the stability of X is greater than the stability of M, the sequential design is more biased than the cross-sectional design about 55% of the time. When the stability of M is greater than the stability of X, the sequential design is more biased than the cross-sectional design about 59% of the time. When the

TABLE 4
Bias in the Estimated Indirect Effect of the Cross-Sectional and Sequential Designs When There Is Full Mediation

Stability of X	*Stability of M*	*a*	*b*	*c*	*x*	*m*	*y*	*cs bias*	*seq bias*
1	0.7	0.5	0.4	0	1	0.33	0.5	−0.08	0.15
1	0.5	0.5	0.4	0	1	0.19	0.5	−0.15	0.07
1	0.5	0.5	0.4	0	1	0.04	0.5	−0.19	0.01
0.9	0.7	0.5	0.4	0	0.9	0.36	0.5	0.02	0.19
0.9	0.5	0.5	0.4	0	0.9	0.22	0.5	−0.10	0.07
0.9	0.3	0.5	0.4	0	0.9	0.06	0.5	−0.17	0.02
0.8	0.7	0.5	0.4	0	0.8	0.4	0.5	0.03	0.22
0.8	0.5	0.5	0.4	0	0.8	0.25	0.5	−0.09	0.10
0.8	0.3	0.5	0.4	0	0.8	0.09	0.5	−0.15	0.03
0.7	0.7	0.5	0.4	0	0.7	0.45	0.5	0.02	0.25
0.6	0.7	0.5	0.4	0	0.6	0.49	0.5	−0.01	0.26
0.5	0.8	0.5	0.4	0	0.5	0.62	0.5	−0.01	0.36
0.5	0.7	0.5	0.4	0	0.5	0.53	0.5	−0.05	0.25

stability of X and M equal each other, the sequential design is more biased than the cross-sectional design about 37% of the time.

Table 5 shows illustrative values of the bias in the indirect effect in the cross-sectional and sequential designs. In the first several rows of the table, both a and b are nonzero, so mediation is present. The lower portion of the table depicts several scenarios where $b = 0$ and thus mediation is absent. The most obvious result shown in the table is that there can be substantial bias for both designs. In particular, when mediation is present, the population value of the indirect effect in the sequential design can at times be 4 or 5 times larger than the indirect effect in the underlying longitudinal design. When mediation is absent, both designs tend to overestimate the indirect effect, sometimes severely.

The entries in Table 5 reveal an important difference between the cross-sectional and sequential designs. Notice that the bias in the cross-sectional design is sometimes positive but other times negative. In contrast, all of the bias entries for the sequential design in Table 5 are positive. Although it is mathematically possible for the bias in Equation 16 to be either positive or negative, in practice the sign of the bias depends on the sign of c, the direct effect. We varied a and b from 0.0 to 0.8 and varied m, x, and y from 0.3 to 0.8. We considered all possible combinations of these parameter values as long as no cross-sectional correlation or sequential correlation was greater than .80. In all cases, the indirect effect bias for the sequential design was positive when c was positive and was negative when c was negative. In the specific case where $b = 0$, it is possible

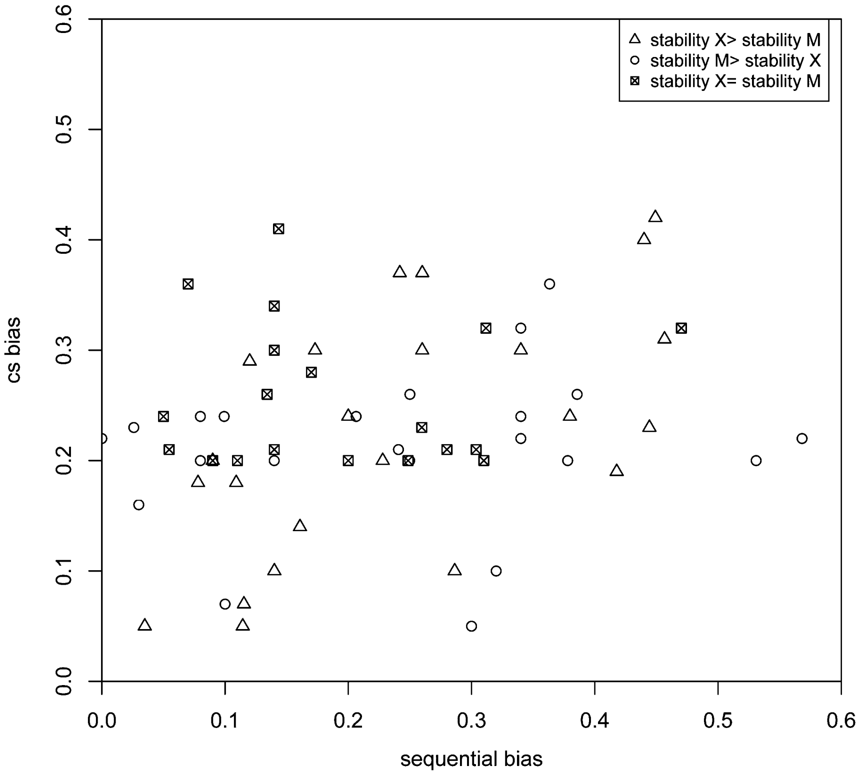

FIGURE 8 Scatter plot of biases in the indirect effect-partial mediation (PM) over one lag scenario.

to simplify Equation 15:

$$bias_{seq} = \frac{a^2cmy^2(1-x^2)}{(1-my)(1-xy)((1-mx)^2-a^2)}. \tag{17}$$

This equation clearly shows that in the special case where b equals zero, the sign of the bias for the sequential design will depend on the sign of c, the direct effect. In most situations, the direct effect is likely to be positive (assuming that variables are coded so as to correlate positively with one another), but it is certainly possible that it could be negative. In any case, there is no mathematical guarantee that the bias of the sequential design will be positive, but in practice it will be positive except in the somewhat unusual case where the direct effect is negative. Notice that the sequential design differs from the cross-sectional

TABLE 5
Bias in the Estimated Indirect Effect of the Cross-Sectional and Sequential Designs When Mediation Occurs Over One Lag

a	*b*	*c*	*x*	*m*	*y*	$a'b'_{cs}$	$a'b'_{seq}$	*cs bias*	*seq bias*
0.2	0.3	0.0	0.8	0.7	0.6	0.16	0.25	0.10	0.19
0.2	0.3	0.1	0.7	0.7	0.5	0.11	0.19	0.05	0.13
0.2	0.3	0.1	0.8	0.8	0.6	0.30	0.39	0.24	0.33
0.4	0.3	0.0	0.5	0.6	0.7	0.12	0.37	0.00	0.25
0.4	0.4	0.0	0.3	0.6	0.7	0.07	0.39	−0.09	0.23
0.4	0.4	0.0	0.6	0.7	0.6	0.30	0.66	0.14	0.50
0.4	0.5	0.0	0.4	0.5	0.6	0.09	0.40	−0.11	0.20
0.4	0.7	0.0	0.3	0.3	0.3	0.04	0.35	−0.24	0.07
0.5	0.2	0.2	0.5	0.7	0.5	0.23	0.43	0.13	0.33
0.5	0.2	0.3	0.7	0.5	0.5	0.32	0.32	0.22	0.22
0.5	0.2	0.2	0.7	0.6	0.5	0.40	0.51	0.30	0.41
0.5	0.2	0.2	0.7	0.6	0.6	0.47	0.69	0.37	0.59
0.5	0.3	0.0	0.7	0.6	0.6	0.37	0.77	0.22	0.62
0.5	0.3	0.2	0.6	0.6	0.5	0.33	0.54	0.18	0.39
0.5	0.6	0.3	0.3	0.3	0.3	0.07	0.38	−0.23	0.08
0.6	0.2	0.2	0.3	0.6	0.7	0.13	0.53	0.01	0.41
0.6	0.3	0.2	0.4	0.6	0.6	0.23	0.74	0.05	0.56
0.6	0.4	0.2	0.4	0.6	0.4	0.22	0.62	−0.02	0.38
0.6	0.5	0.3	0.3	0.3	0.3	0.09	0.39	−0.21	0.09
0.6	0.6	0.1	0.3	0.3	0.3	0.07	0.47	−0.29	0.11
0.4	0.0	0.4	0.7	0.7	0.5	0.30	0.14	0.30	0.14
0.4	0.0	0.6	0.8	0.6	0.3	0.26	0.03	0.26	0.03
0.5	0.0	0.2	0.5	0.7	0.7	0.16	0.22	0.16	0.22
0.5	0.0	0.3	0.5	0.7	0.8	0.30	0.55	0.30	0.55
0.5	0.0	0.3	0.5	0.7	0.7	0.24	0.34	0.24	0.34
0.5	0.0	0.4	0.6	0.6	0.5	0.25	0.12	0.25	0.12
0.5	0.0	0.4	0.6	0.6	0.7	0.36	0.35	0.36	0.35
0.5	0.0	0.4	0.7	0.5	0.6	0.29	0.13	0.29	0.13

design because the direction of the bias in the cross-sectional design is less systematic. As a result, there may be some situations where it is reasonable to interpret the bias of the sequential design as being positive. Although there may be some value in being able to anticipate the sign of the bias in the sequential design, Table 5 shows that the magnitude of the bias is still difficult to judge, thus limiting the utility of any estimate from the sequential design.

Partial Mediation Over Two Lags

Indirect effect bias. The expressions for the indirect effect bias for the sequential and cross-sectional design are

$$bias_{seq} = c + \frac{ab}{(1-mx)(1-xy)} + \frac{cxy}{1-xy} - c'_{seq} - ab \tag{18}$$

$$bias_{cs} = \frac{c\rho_{XM}((\rho_{XM}(m^2(1-xy) - x^2(1-my))) + ax(1+m-mxy))}{(1-\rho^2_{X_tM_t})(1-xy)(1-my)}$$

$$+ \frac{b\rho_{XM}(\rho_{M_tM_{t-1}} - mxy + mxy\rho^2_{XM} - x^2\rho_{XM})}{(1-\rho^2_{X_tM_t})(1-xy)(1-my)} - ab, \tag{19}$$

where Equation 18 follows from Equation 11. Refer to Maxwell et al. (2011) for the derivation of $bias_{cs}$. Given the nature of Equations 18 and 19 a numerical analysis was conducted to compare the indirect effect bias. c was not constrained to be zero so it does play a role in the indirect effect bias.

Although the values for the estimated indirect effect (and its bias) are not the same in the one and two lag case, the magnitude of the bias, on average, for this case is similar to that of the one lag case; therefore we do not present additional details. However, we provide percentages of how the designs fare in terms of bias as we have for the previous case. In summary, when the stability of X is greater than the stability of M, the sequential design is more biased than the cross-sectional design about 56% of the time. When the stability of M is greater than the stability of X, the sequential design is more biased than the cross-sectional design about 55% of the time. When the stability of X and M equal each other, the sequential design is more biased than the cross-sectional design about 41% of the time.

NUMERICAL EXAMPLE

Algebraic results show that both the cross-sectional and sequential designs can suffer from severe biases in estimating longitudinal direct and indirect effects. Although we have documented serious magnitudes of bias for a variety of parameter values, it may still be worthwhile to explore the magnitude of apparent bias in actual empirical studies. To obtain this perspective, we searched for recent articles that assess mediation from a longitudinal perspective. We identified three recent articles (Eisenberg et al., 2005; Gondoli, Grundy, Salafia, & Bonds, 2008; Morrissey & Gondoli, 2012) that used the longitudinal design shown in our Figure 4 and that also provided correlations for all variables included in the model. The question of interest to us was how different these investigators' results would have been if they had used a sequential design instead of a full longitudinal design. Thus, we reanalyzed their data using a sequential design instead of a longitudinal design. The difference in results produced by the two types of designs was very similar for all three studies, so we provide details only for Eisenberg et al. (2005). These authors examined the role of children's effortful control as a possible mediator of the relationship between positive parenting and children's externalizing problems. They measured all three variables at three time points and assessed mediation in a fully longitudinal design. Their analysis supported the role of effortful control as a mediator.

From a methodological perspective, how different might their results have been if they had used a sequential design instead of a fully longitudinal design? In other words, suppose they had measured positive parenting only at Time 1, effortful control only at Time 2, and externalizing problems only at Time 3? This design would have allowed them to assess mediation using a sequential design. Because Eisenberg et al. (2005) provided a correlation matrix for their data,[4] we were able to reanalyze their data using a sequential design. The published analysis revealed a statistically significant indirect effect. So did our reanalysis using a sequential design. However, the estimated magnitude of the indirect effect was very different in the two designs. The estimated indirect effect based on the full longitudinal design was −0.02, whereas the estimate from the sequential design was −0.43. (The sign is negative here because more positive parenting leads to more effortful control, but more effortful control leads to less externalizing.) The same pattern held for both of the other studies as well. Namely, for Gondoli et al. (2008), the estimated indirect effect from the longitudinal design was 0.05 compared with an estimate of 0.21 in the sequential design. For Morrissey and Gondoli (2012), the corresponding values were 0.02 and 0.10. Notice that the results in all three studies are consistent with Table 5 in that the sequential design consistently implies a much larger indirect effect (in absolute value) than the longitudinal design. Most important, the results for these three studies suggest that the magnitude of bias associated with the sequential design can be severe in practice.

DISCUSSION

Recently, methodologists have urged researchers studying mediation to include temporal precedence in their experimental design because otherwise they may risk producing biased results. In turn, some researchers have begun to use the sequential design as a way to include temporal precedence. The sequential design has intuitive appeal because it incorporates time while still being simple to implement. It is a compromise between the longitudinal and cross-sectional design.

[4]Their published correlation matrix did not provide the correlation between the two manifest indicators of externalizing at the final wave but did provide the comparable correlation at the two earlier waves. The correlation at the first wave was .32, and the correlation at the second wave was .33. Given this consistency across waves, we assumed a value of .32 for the correlation at the final wave.

It resembles the cross-sectional design in that there is only one measurement of each variable and resembles the longitudinal design because it incorporates time by measuring the variables sequentially. However, little work has been done studying the effects of using the sequential design until now. Therefore, researchers have been using an untested design to carry out their studies.

This work shows how well the sequential design assesses longitudinal mediation and compares its performance with that of the cross-sectional design. When there is full mediation, the sequential design provides biased estimates of longitudinal mediation parameters, sometimes overestimating and other times underestimating the effects of interest. The sequential design tends to produce less biased estimates of the direct effect under full mediation if X is more stable than M. Unfortunately, there was no pattern evident for the indirect effect for full mediation although it seems that when the stability of M is greater than the stability of X, the cross-sectional may be more biased than the sequential design. Overall, the magnitude of bias for the sequential design is larger than that of the cross-sectional design. Both designs provide poor estimates of longitudinal mediation parameters and can indicate that mediation is present even when it is actually absent.

When there is partial mediation, over either one or two lags, both designs generally provide biased estimates of longitudinal mediation parameters, overestimating or underestimating the effects of interest. Arguably, the sequential design has one advantage over the cross-sectional design for estimating the indirect effect under partial mediation. Namely, the direction of the bias for the sequential design is generally more predictable than for the cross-sectional design. Whereas the cross-sectional design may either greatly underestimate or overestimate the indirect effect, the sequential design will typically overestimate the indirect effect. However, the magnitude of possible bias greatly limits the value of either design.

The sequential design is often no better (and sometimes worse) than the cross-sectional design because it fails to consider autoregressive effects. If all the autoregressive effects (x, m, y) are zero, the sequential correlations for the full mediation case derived in Appendix A can be simplified as

$$\rho_{X_t M_{t+1}} = \frac{a}{1 - mx} = a$$

$$\rho_{X_t Y_{t+2}} = \frac{ab}{(1 - mx)(1 - xy)} = ab$$

$$\rho_{M_{t+1} Y_{t+2}} = b + \frac{bmy}{1 - my} + \frac{a^2 bxy}{(1 - mx)(1 - my)(1 - xy)} = b.$$

If the autoregressive effects were completely absent, the sequential design would provide unbiased estimates of mediational effects. This is also true when partial mediation occurs over two lags but is slightly more complicated over one lag. If the autoregressive effects are zero for the one lag case, the sequential correlations become

$$\rho_{X_t M_{t+1}} = \frac{a}{1 - mx} = a$$

$$\rho_{X_t Y_{t+2}} = cx + \frac{ab}{(1 - mx)(1 - xy)} + \frac{cy}{1 - xy} = ab$$

$$\rho_{M_{t+1} Y_{t+2}} = b + \frac{acx}{1 - mx} + y\rho_{M_t Y_t} = b.$$

In this case, the sequential design would provide an unbiased estimate of the indirect effect but would provide a biased estimate of the direct effect over two lags, exemplifying Reichardt's (2011) point about the importance of the spacing between waves of the design. For the two lag case, the expressions become

$$\rho_{X_t M_{t+1}} = \frac{a}{1 - mx} = a$$

$$\rho_{X_t Y_{t+2}} = c + \frac{ab}{(1 - mx)(1 - xy)} + \frac{cxy}{1 - xy} = ab + c$$

$$\rho_{M_{t+1} Y_{t+2}} = b + \frac{acx}{1 - mx} + y\rho_{M_t Y_t} = b.$$

In this case, the sequential design would provide unbiased estimates of the direct effect as well as the indirect effect. However, as we have seen, when autoregressive effects are present, the sequential design generally produces biased estimates of the direct and indirect effects.

Although the sequential design has the appeal of including time in the design, it requires what is almost always two unrealistic assumptions. First, it assumes that all causes of M at time $t + 1$ other than X at time t are uncorrelated with X at time t. This is plausible if individuals have been randomly assigned to levels of X but otherwise is highly unlikely to be true. Second, it assumes that all causes of Y at time $t + 2$ other than M at time $t + 1$ are uncorrelated with M at time $t + 1$. Again, this is highly implausible and in fact remains implausible even if individuals have been randomly assigned to levels of X. We should immediately add that the full longitudinal design (of Figures 3 and 4) also requires assumptions that at first glance are similar to those of the sequential design. Namely, the longitudinal design assumes that all causes of M at time $t + 1$ other than X at time t and M at time t are uncorrelated with X at time

t. It also assumes that all causes of Y at time $t + 2$ other than M at time $t + 1$ and Y at time $t + 1$ are uncorrelated with M at time $t + 1$. Although there is certainly no guarantee that these assumptions will be met, they are much weaker assumptions than those of the sequential design because measures of M and Y at previous waves will often capture much if not all of what would otherwise be confounding common causes. On balance, the sequential design requires much more stringent assumptions than the longitudinal design, and our results show that the sequential design often yields very biased estimates of mediational parameters when its more stringent assumptions are not satisfied.

Although the appeal for having unbiased estimates of longitudinal mediation parameters is strong, the full longitudinal design can be time consuming and expensive and in some cases, it may be difficult to estimate all of the parameters of the model. A viable alternative is a lower triangular design that would be the lower triangular part of the model shown in Figure 3 or Figure 4. This model includes X at Time 1, M at Time 1 as well as Time 2, and Y at Times 1 and 2 as well as at Time 3. This design includes temporal precedence and controls for the relationship between predictors of the mediator and dependent variable. If the assumed model, the three-wave autoregressive model, is true in the population then the lower triangular design does not leave out any effects and is specified correctly. This design would guarantee unbiased parameter estimates if there is no specification error in the model. Thus, this design can serve as a more economical alternative to the full longitudinal model shown in Figure 3 or Figure 4. In the special case where individuals are randomly assigned to levels of X, the model can be simplified yet further by including M at only Time 2.

It should be acknowledged that time lags are essentially an unknown quantity when it comes to studying mediation. Many researchers have addressed the importance of time lags but no study has shown what time lags are applicable in certain areas of study—most only saying that it takes time t for X to affect M and for M to affect Y (Cole & Maxwell, 2009; Gollob & Reichardt, 1987; Maxwell & Cole, 2007). This study does not address the length of time it takes for mediation to occur in general. We have assumed one or two time lags but that does not measure any quantity of time in terms of minutes, days, or weeks. How long it takes for one variable to affect another depends solely on the phenomenon studied. As Reichardt (2011) emphasized, researchers should realize that their results may very well depend on their choice of timing and should carefully consider how best to design their study. Among other things, this may also imply the desirability of including more than three waves in their design.

There are of course limitations to the current study. This study only looked at one model of change, the autoregressive model. Assumptions were made to simplify the derivations and may not always be realistic. Figures 3 and 4 portray just two possible scenarios of longitudinal mediation that we modeled after work done by Maxwell and Cole (2007) and Maxwell et al. (2011). Figures 3 and 4 are hypothetical models but they are not the only valid models of change. We have also assumed a discrete time model and it may be beneficial to look at continuous time models. It may be of interest to look at situations where X is fixed over time (such as an intervention) whereas this article only considered variables all of which are changing over time. Another avenue of research would be examining the lower triangular design as suggested earlier as an alternative to the full longitudinal design.

In conclusion, this work, along with earlier work (Cole & Maxwell, 2003; Maxwell & Cole, 2007; Maxwell et al., 2011) reiterates the need for longitudinal designs when assessing longitudinal mediation. Both the cross-sectional design and the sequential design of the MacArthur approach may exaggerate or underestimate direct and indirect effects. Thus, we recommend that researchers who are studying mediation rely on other approaches for assessing mediational effects. We believe that the longitudinal models shown in Figures 3 and 4 (or their lower triangular counterparts) provide better alternatives for understanding mediational processes. Nevertheless, we also believe it would be a mistake for researchers to limit their attention to these designs. Instead, we strongly encourage researchers to consider the wide variety of designs for studying mediation as described by such sources as Imai et al. (2011) and Shrout (2011). We also encourage researchers to take seriously the importance of timing of measurements as described by Cole and Maxwell (2009), Kraemer (2010), and Reichardt (2011). More generally, these recent methodological advances have the potential to reveal more clearly and accurately the nature of important psychological processes.

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APPENDIX A

Derivations of Correlations for the Sequential Design From Autoregressive Model Assuming Full Mediation

1. *Expression for the X_tM_{t+1} correlation*. From the model depicted in Figure 3 or Figure 4, we see that

$$X_t = xX_{t-1} + \varepsilon_{X_t} \tag{A-1}$$

$$M_{t+1} = mM_t + aX_t + \varepsilon_{M_{t+1}}. \tag{A-2}$$

The covariance of X_t and M_t can be expressed as

$$C(X_t, M_{t+1}) = xmC(X_{t-1}, M_t) + ax. \tag{A-3}$$

Assuming that all variables are standardized, covariance are equal to correlations and we can express the correlation between X_t and M_{t+1} as

$$\rho_{X_t,M_{t+1}} = xm\rho_{X_{t-1},M_{t+1}} + ax. \tag{A-4}$$

At equilibrium, correlations are equal across waves, which implies that

$$\rho_{X_t,M_{t+1}} = \rho_{X_{t-1},M_t}. \tag{A-5}$$

Substituting Equation A-5 into Equation A-4 yields

$$\rho_{X_t,M_{t+1}} = xm\rho_{X_t,M_{t+1}} + ax. \tag{A-6}$$

Rearranging terms and solving for the correlation between X_t and M_{t+1}

$$\rho_{X_t,M_{t+1}} = \frac{a}{1-mx}. \tag{A-7}$$

2. *Expression for the $X_t Y_{t+2}$ correlation.* From the model depicted in Figure 3 or Figure 4 (because we have full mediation), we see that

$$X_t = xX_{t-1} + \varepsilon_{X_t} \tag{A-8}$$

$$Y_{t+2} = bM_{t+1} + yY_{t+1} + \varepsilon_{Y_{t+2}}. \tag{A-9}$$

The covariance of X_t and Y_{t+2} can be in terms of covariances at time t as

$$C(X_t, Y_{t+2}) = ab + bmC(X_t, M_t) + byC(X_t, M_t) + y^2C(X_t, Y_t). \tag{A-10}$$

Assuming standardized variables and equal correlations across waves, Equation A-10 can be rewritten as

$$\rho_{X_t,Y_{t+2}} = ab + bm\rho_{X_t,M_t} + by\rho_{X_t,M_t} + y^2\rho_{X_t,Y_t}. \tag{A-11}$$

At equilibrium, correlations are equal across waves, which implies that

$$\rho_{X_t,Yt+2} = \rho_{X_{t-1},Y_{t+1}}. \tag{A-12}$$

Rearranging terms and solving for the correlation yields

$$\rho_{X_t,Y_{t+2}} = \frac{ab}{(1-mx)(1-xy)}. \tag{A-13}$$

3. *Expression for the $M_{t+1}Y_{t+2}$ correlation.* From the model depicted in Figure 3 or Figure 4, we see that

$$M_{t+1} = mM_t + \varepsilon_{M_{t+1}} \tag{A-14}$$

$$Y_{t+2} = bM_{t+1} + yY_{t+1} + \varepsilon_{Y_{t+2}}. \tag{A-15}$$

The covariance of M_{t+1} and Y_{t+2} can be expressed as

$$C(M_{t+1}, Y_{t+2}) = b + yC(M_{t+1}, Y_{t+1}). \tag{A-16}$$

Assuming standardized variables and equal correlations across waves, Equation A-17 can be rewritten as

$$\rho_{M_{t+1},Y_{t+2}} = b + y\rho_{M_{t+1},Y_{t+1}}. \tag{A-17}$$

Rearranging terms and solving for the correlation yields

$$\rho_{M_{t+1},Y_{t+2}} = b + \frac{bmy}{1-my} + \frac{a^2bxy}{(1-mx)(1-xy)(1-my)}. \tag{A-18}$$

APPENDIX B

Derivation of the Direct Effect Bias When Full Mediation is Assumed

The general form of the direct effect is

$$c'_{seq} = \frac{\rho_{X_t Y_{t+2}} - \rho_{X_t M_{t+1}} \rho_{M_{t+1} Y_{t+2}}}{1 - \rho^2_{X_t M_{t+1}}} \tag{B-1}$$

for the sequential approach.

Using Equation B-1 to find the direct effect bias for the sequential design and focusing on the second half of the numerator,

$$\rho_{X_t M_{t+1}} \rho_{M_{t+1} Y_{t+2}} = \frac{ab}{1-mx} + \frac{a^2bcx}{(1-mx)^2} + \frac{abmy}{(1-mx)(1-my)} + \frac{a^3bxy}{(1-mx)^2(1-my)(1-xy)}. \tag{B-2}$$

So the numerator equals

$$\rho_{X_t Y_{t+2}} - \rho_{X_t M_{t+1}} \rho_{M_{t+1} Y_{t+2}} = \frac{ab}{(1-mx)(1-xy)} - \frac{ab}{1-mx} - \frac{a^2bcx}{(1-mx)^2} - \frac{abmy}{(1-mx)(1-my)} - \frac{a^3bxy}{(1-mx)^2(1-my)(1-xy)}, \tag{B-3}$$

which simplifies to

$$\frac{abm(x - y + x^2y^2 - mx^2 + m^2xy - mx^2y^2) + a^2bc(y - x + x^2y - mx^2y) - a^3bxy}{(1-mx)^2(1-my)(1-xy)}. \tag{B-4}$$

The denominator in this case equals

$$1 - \rho^2_{X_t M_{t+1}} = \frac{(1-mx)^2 - a^2}{(1-mx)^2}. \tag{B-5}$$

Dividing the numerator and the denominator to get the direct effect bias,

$$c'_{seq} = \frac{\begin{array}{c} abm(x - y + x^2y^2 - mx^2 + m^2xy - mx^2y^2) \\ +a^2bc(y - x + x^2y - mx^2y) \end{array}}{[(1 - mx)^2 - a^2](1 - mx)(1 - xy)} - \frac{a^3bxy}{[(1 - mx)^2 - a^2](1 - mx)(1 - xy)}. \quad \text{(B-6)}$$

The expression for the direct effect of the cross-sectional design was simplified from the expression given in Maxwell and Cole (2007) and is not given here. The direct effect bias would be the true direct effect (zero in this case) subtracted from the estimated direct effect.

APPENDIX C

Derivations of Correlations for the Sequential Design From Autoregressive Model Assuming Partial Mediation Over One Lag

1. *Expression for the X_tM_{t+1} correlation.* The correlation between X_t and M_{t+1} is the same in the partial mediation over one lag case and in the full mediation case so the result is not repeated here but can be found in Appendix A.

2. *Expression for the X_tY_{t+2} correlation.* From the model depicted in Figure 3a, we see that

$$X_t = xX_{t-1} + \varepsilon_{X_t} \quad \text{(C-1)}$$

$$Y_{t+2} = cX_{t+1} + bM_{t+1} + yY_{t+1} + \varepsilon_{Y_{t+2}}. \quad \text{(C-2)}$$

The covariance of X_t and Y_{t+2} can be expressed as

$$C(X_t, Y_{t+2}) = cC(X_t, X_{t+1}) + bC(X_t, M_{t+1}) + yC(X_t, Y_{t+1}). \quad \text{(C-3)}$$

Assuming standardized variables and equal correlations across waves, Equation C-3 can be rewritten as

$$\rho_{X_t,Y_{t+2}} = cx + b\rho_{X_tM_{t+1}} + y\rho_{X_tY_{t+1}}, \quad \text{(C-4)}$$

where

$$\rho_{X_t Y_{t+1}} = C(X_t, Y_{t+1}) = cC(X_t, X_t) + bC(X_t, M_t) + yC(X_t, Y_t) \quad \text{(C-5)}$$

$$= c + b\rho_{X_t,M_t} + y\rho_{X_t,Y_t} \quad \text{(C-6)}$$

$$= c + b(\frac{ax}{1-mx}) + y(\frac{cx + bx\rho_{X_t M_t}}{1-xy}) \quad \text{(C-7)}$$

$$= c + \frac{abx}{1-mx} + \frac{cxy + bxy\rho_{X_t M_t}}{1-xy} \quad \text{(C-8)}$$

$$= \frac{abx + c(1-mx)}{(1-mx)(1-xy)}. \quad \text{(C-9)}$$

Substituting C-9 into C-4 yields

$$\rho_{X_t,Y_{t+2}} = c + b(\frac{a}{1-mx}) + y(\frac{abx + c(1-mx)}{(1-mx)(1-xy)})$$

$$= cx + \frac{ab}{(1-mx)(1-xy)} + \frac{cy}{1-xy}. \quad \text{(C-10)}$$

3. *Expression for the $M_{t+1}Y_{t+2}$ correlation*. From the model depicted in Figure 3a, we see that

$$M_{t+1} = mM_t + \varepsilon_{M_{t+1}} \quad \text{(C-11)}$$

$$Y_{t+2} = cX_{t+1} + bM_{t+1} + yY_{t+1} + \varepsilon_{Y_{t+2}}. \quad \text{(C-12)}$$

The covariance of M_{t+1} and Y_{t+2} can be expressed as

$$C(M_{t+1}, Y_{t+2}) = b + cC(M_{t+1}, X_{t+1}) + yC(M_{t+1}, Y_{t+1}). \quad \text{(C-13)}$$

Assuming standardized variables and equal correlations across waves, Equation A-17 can be rewritten as

$$\rho_{M_{t+1},Y_{t+2}} = b + c\rho_{M_{t+1},X_{t+1}} + y\rho_{M_{t+1},Y_{t+1}}. \quad \text{(C-14)}$$

Rearranging terms and solving for the correlation yields

$$\rho_{M_{t+1},Y_{t+2}} = b + \frac{acx}{1-mx} + y\rho_{M_t Y_t}, \quad \text{(C-15)}$$

where $\rho_{M_t Y_t} = \frac{ac+(ab+cm)\rho_{X_t M_t} + ay\rho_{X_t Y_t} + bm}{1-my}$ (Maxwell, Cole, & Mitchell, 2011).

APPENDIX D

Derivation of the Direct Effect Bias Where Partial Mediation Occurs Over One Lag

To solve for the direct effect bias, we need to use Equation B-1. First we focus on the numerator, $\rho_{X_tY_{t+2}} - \rho_{X_tM_{t+1}}\rho_{M_{t+1}Y_{t+2}}$. Focusing on the second half of the numerator,

$$\rho_{X_tM_{t+1}}\rho_{M_{t+1}Y_{t+2}} = \frac{a}{1-mx}\left(b + \frac{acx}{1-mx} + y\rho_{M_tY_t}\right) \tag{D-1}$$

$$= \frac{ab}{1-mx} + \frac{a^2cx}{(1-mx)^2} + \frac{ay\rho_{M_tY_t}}{1-mx}. \tag{D-2}$$

So the numerator equals

$$\begin{aligned}\rho_{X_tY_{t+2}} - \rho_{X_tM_{t+1}}\rho_{M_{t+1}Y_{t+2}} &= cx + \frac{ab}{(1-mx)(1-xy)} + \frac{cy}{1-xy} - \frac{ab}{1-mx} \\ &\quad - \frac{a^2cx}{(1-mx)^2} - \frac{ay\rho_{M_tY_t}}{1-mx} \\ &= cx + \frac{ab}{(1-mx)(1-xy)} + \frac{cy}{1-xy} \\ &\quad - \frac{a(b + y\rho_{M_tY_t})}{1-mx} - \frac{a^2cx}{(1-mx)^2}. \end{aligned} \tag{D-3}$$

The denominator equals

$$1 - \rho^2_{X_tM_{t+1}} = \frac{(1-mx)^2 - a^2}{(1-mx)^2}. \tag{D-4}$$

Dividing the numerator and the denominator to get c'_{seq},

$$c'_{seq} = \frac{\begin{array}{c} cx + \frac{ab}{(1-mx)(1-xy)} + \frac{cy}{1-xy} \\ -\frac{a(b + y\rho_{M_tY_t})}{1-mx} - \frac{a^2cx}{(1-mx)^2} \end{array}}{\frac{(1-mx)^2 - a^2}{(1-mx)^2}}. \tag{D-5}$$

The bias of the direct effect in the sequential design is $c'_{seq} - c$.

The bias for the direct effect in the cross-sectional design for this case is taken from the expression for the direct effect given in Maxwell, Cole, & Mitchell (2011) and is not repeated here.

APPENDIX E

Derivations of Correlations for the Sequential Design From Autoregressive Model Assuming Partial Mediation Over Two Lags

1. *Expression for the X_tM_{t+1} correlation.* The correlation between X_t and M_{t+1} is the same in the partial mediation over two lag case and in the full mediation case so the result is not repeated here but can be found in Appendix A.

2. *Expression for the X_tY_{t+2} correlation.* From the model depicted in Figure 4, we see that

$$X_t = xX_{t-1} + \varepsilon_{X_t} \tag{E-1}$$

$$Y_{t+2} = yY_{t+1} + bM_{t+1} + cX_t + \varepsilon_{Y_{t+2}}. \tag{E-2}$$

The covariance of X_t and Y_{t+2} can be expressed as

$$C(X_t, Y_{t+2}) = cC(X_t, X_t) + bC(X_t, M_{t+1}) + yC(X_t, Y_{t+1}). \tag{E-3}$$

Assuming standardized variables and equal correlations across waves, Equation E-3 can be rewritten as

$$\rho_{X_t,Y_{t+2}} = c + b\rho_{X_t,M_{t+1}} + y\rho_{X_tY_{t+1}} \tag{E-4}$$

$$= c + b\frac{a}{1-mx} + y\rho_{X_tY_{t+1}}, \tag{E-5}$$

where

$$\rho_{X_tY_{t+1}} = cx + b\rho_{X_tM_t} + y\rho_{X_tY_t} \tag{E-6}$$

$$= cx + b(\frac{ax}{1-mx}) + y(\frac{cx^2 + bx\rho_{X_tM_t}}{1-xy}) \tag{E-7}$$

$$= \frac{x(c - cmx + ab)}{(1-mx)(1-xy)}. \tag{E-8}$$

Substituting Equation E-8 into E-5 yields

$$\rho_{X_t,Y_{t+2}} = c + b(\frac{a}{1-mx}) + y(\frac{x(c-cmx+ab)}{(1-mx)(1-xy)})$$
$$= c + \frac{ab}{1-mx} + \frac{xy(c-cmx+ab)}{(1-mx)(1-xy)}, \quad \text{(E-9)}$$

which, after some algebraic manipulation, equals

$$\rho_{X_t,Y_{t+2}} = c + \frac{ab}{(1-mx)(1-xy)} + \frac{cxy}{1-xy}. \quad \text{(E-10)}$$

3. *Expression for the* $M_{t+1}Y_{t+2}$ *correlation.* From the model depicted in Figure 4, we see that

$$M_{t+1} = mM_t + \varepsilon_{M_{t+1}} \quad \text{(E-11)}$$

$$Y_{t+2} = cX_t + bM_{t+1} + yY_{t+1} + \varepsilon_{Y_{t+2}}. \quad \text{(E-12)}$$

The covariance of M_{t+1} and Y_{t+2} can be expressed as

$$C(M_{t+1}, Y_{t+2}) = b + cC(M_{t+1}, X_t) + yC(M_{t+1}, Y_{t+1}). \quad \text{(E-13)}$$

Assuming standardized variables and equal correlations across waves, Equation E-13 can be rewritten as

$$\rho_{M_{t+1},Y_{t+2}} = b + c\rho_{X_t,M_{t+1}} + y\rho_{M_{t+1},Y_{t+1}}. \quad \text{(E-14)}$$

Rearranging terms and solving for the correlation yields

$$\rho_{M_{t+1},Y_{t+2}} = b + \frac{ac}{1-mx} + y\rho_{M_tY_t}, \quad \text{(E-15)}$$

where $\rho_{M_tY_t} = \frac{acx+(ab+cm^2)\rho_{X_tM_t}+ay\rho_{X_tY_t}+acmx+bm}{1-my}$ (Maxwell, Cole, & Mitchell, 2011).

APPENDIX F

Derivation of the Direct Effect Bias Where Partial Mediation Occurs Over Two Lags

To solve for the direct effect bias, we need to use Equation B-1. First we focus on the numerator, $\rho_{X_tY_{t+2}} - \rho_{X_tM_{t+1}}\rho_{M_{t+1}Y_{t+2}}$. Focusing on the second half of the numerator,

$$\rho_{X_t M_{t+1}} \rho_{M_{t+1} Y_{t+2}} = \frac{a}{1-mx}\left(b + \frac{ac}{1-mx} + y\rho_{M_t Y_t}\right)$$
$$= \frac{ab}{1-mx} + \frac{a^2 c}{(1-mx)^2} + \frac{ay\rho_{M_t Y_t}}{1-mx}. \quad \text{(F-1)}$$

So the numerator equals

$$\rho_{X_t Y_{t+2}} - \rho_{X_t M_{t+1}} \rho_{M_{t+1} Y_{t+2}} = c + \frac{ab}{(1-mx)(1-xy)} + \frac{cxy}{1-xy} - \frac{ab}{1-mx} - \frac{a^2 c}{(1-mx)^2} - \frac{ay\rho_{M_t Y_t}}{1-mx}$$
$$= c + \frac{ab}{(1-mx)(1-xy)} + \frac{cxy}{1-xy} - \frac{a(b + y\rho_{M_t Y_t})}{1-mx} - \frac{a^2 c}{(1-mx)^2}. \quad \text{(F-2)}$$

The denominator equals

$$1 - \rho^2_{X_t M_{t+1}} = \frac{(1-mx)^2 - a^2}{(1-mx)^2}. \quad \text{(F-3)}$$

Dividing the numerator and the denominator to get c'_{seq},

$$c'_{seq} = \frac{c + \frac{ab}{(1-mx)(1-xy)} + \frac{cxy}{1-xy} - \frac{a(b+y\rho_{M_t Y_t})}{1-mx} - \frac{a^2 c}{(1-mx)^2}}{\frac{(1-mx)^2 - a^2}{(1-mx)^2}}. \quad \text{(F-4)}$$

The direct effect bias in the sequential design is $c'_{seq} - c$.

The direct effect in the cross-sectional design for this case is provided in Maxwell, Cole, and Mitchell (2011) and is not repeated here.