The Report Committee for Lester Leland Lockhart III
Certifies that this is the approved version of the following report:

Nonlinear Mediation in Clustered Data: A Nonlinear Multilevel Mediation Model

APPROVED BY
SUPERVISING COMMITTEE:

Supervisor: ______________________________
Gary Borich

____
S. Natasha Beretvas

Nonlinear Mediation in Clustered Data: A Nonlinear Multilevel Mediation Model

by

Lester Leland Lockhart III, B. S.

Report

Presented to the Faculty of the Graduate School of

The University of Texas at Austin

in Partial Fulfillment

of the Requirements

for the Degree of

Master of Arts

The University of Texas at Austin

May 2011

Abstract

Nonlinear Mediation in Clustered Data: A Nonlinear Multilevel Mediation Model

Lester Leland Lockhart III, M. A.

The University of Texas at Austin, 2011

Supervisor: Gary Borich

Mediational analysis quantifies proposed causal mechanisms through which treatments act on outcomes. In the presence of clustered data, conventional multiple regression mediational methods break down, requiring the use of hierarchical linear modeling techniques. As an additional consideration, nonlinear relationships in multilevel mediation models require unique specifications that are ignored if modeled linearly. Improper specification of nonlinear relationships can lead to a consistently overestimated mediated effect. This has direct implications for inferences regarding intervention causality and efficacy. The current investigation proposes a nonlinear multilevel mediation model to account for nonlinear relationships in clustered data. A simulation study is proposed to compare the statistical performance of the proposed nonlinear multilevel mediation model with that of conventional methods.

Table of Contents

List of Figures

Chapter 1: Introduction

Mediational analysis quantifies proposed causal mechanisms through which treatments act on outcomes. In its most general conceptualization, mediation exists when the effect of a causal agent, T, on an outcome, Y, acts through at least one intervening variable, M (Hayes & Preacher, 2010). The validity of the causal implications resulting from a mediational analysis is largely dependent on the nature of the experimental design, as causality itself is an epistemological issue requiring the fulfillment of certain experimental conditions (Bauer, Preacher, & Gil, 2006). More specifically, causality can only be inferred when the variables involved covary with each other, when spurious results have been eliminated, and when hypothesized causes chronologically precede their presumed effects (Frazier, Tix, & Barron, 2004). Additionally, secondary manipulation of the mediating variable, M, along with experimental manipulation of the primary treatment variable, T, greatly strengthens the ensuing causal inferences (Spencer, Zanna, & Fong, 2005). With these issues in mind, mediational methods provide researchers the necessary statistical tools to investigate mediated mechanisms. This holds direct implications for modifying and improving treatment processes, as mediated effects shed light on the underlying causal mechanisms by which treatments are effective (Judd & Kenny, 1981). As such, this form of analysis is especially relevant to the social and behavioral sciences as investigators frequently seek causal explanations for treatment efficacy (Baron & Kenny, 1986).

Chapter 2: Literature Review

The current prospectus adds to the mediation literature by proposing an alternative parameterization to traditional mediation models. First, conventional linear mediation models are presented. Next, these traditional single-level methods are extended to address hierarchically nested data via mixed linear models. A thorough examination of indirect effect calculation and hypothesis testing ensues, as this computation lies at the heart of all mediational analyses. This is followed by a discussion of nonlinear multilevel extensions for non-normally distributed data. At this point, problems with measures of mediation are discussed, followed by presentation of a nonlinear multilevel mediation model (NMMM) designed to address these concerns. A simulation study is then proposed. Expected results, limitations, implications for applied research, and future directions are then discussed.

TRADITIONAL MEDIATION

Standard mediational analyses quantify the casual relationships between an independent variable (or treatment, T), an outcome variable (or criterion, Y), and a mediating variable (or mechanism, M). In conventional mediation, the treatment variable typically consists of an indicator variable coded one for those in the treatment condition and zero for those in the control group. Alternatively, the treatment variable may consist of a continuous variable corresponding to a treatment dosage such as the amount in milligrams of a particular drug. Mediating variables are often operationalized as constructs or measures hypothesized to describe how or why certain effects occur (Baron

& Kenny, 1986; Bruner, 1957). Conceptually, mediation exists when a significant portion of the treatment effect operates through a mediating variable. Using path notation (Klein, 2005), Figure 1 depicts the total effect of *T* on outcome *Y*. However, part of this effect may function through an intermediate variable, *M*. The effect of *T* on *Y* is then broken down into its constituent paths as presented in Figure 2. Here, *a* represents the effect of *T* on *M*, *b* represents the effect of *M* on *Y*, and *c′* represents the effect of *T* on *Y* that is independent of *M*. *T* is hypothesized to affect *Y* both directly (path *c'*) and indirectly through *M* (paths *a* and *b*). The portion of this effect that goes through *M* is coined the *indirect effect*, whose calculation lies at the heart of mediational analysis.

In the simplest single mediator model, paths *a*, *b*, *c*, and *c′* in Figures 1 and 2 can be computed as unstandardized ordinary least squares (OLS) regression coefficient estimates obtained from the estimation of three regression models (MacKinnon, 2008). First, the total effect of a treatment variable (*T*) on an outcome variable (*Y*) is defined as the *c* parameter (Figure 1) by using the following linear equation:

$$Y_i + \beta_{0(Y.T)} + cT_i + e_i, \tag{1}$$

where Y_i represents the outcome score for subject *i*, $\beta_{0(Y.T)}$ represents the intercept for the prediction of *Y*, T_i represents the value of the treatment variable for subject *i*, *c* represents the total effect of the treatment on the outcome, and e_i represents the error term for subject *i*. This model corresponds to the path diagram in Figure 1, representing an estimate of the total effect of *T* on *Y* without accounting for possible covariates or mediators.

To obtain an estimate of path a in Figure 2, M is defined as a function of T according to the following model:

$$M_i = \beta_{0(M.T)} + aT_i + e_i \, , \tag{2}$$

where M_i represents the score on the mediator for subject *i,* $\beta_{0(M.T)}$ represents the intercept for the prediction of M, T_i represents the treatment variable for subject i as in Equation 1, and e_i represents the error term for subject i. A third regression equation is used to provide estimates of b and c'. Here, Y is defined as a function of both M and T:

$$Y_i = \beta_{0(Y.MT)} + bM_i + c' T_i + e_i \, . \tag{3}$$

In this equation, $\beta_{0(Y.MT)}$ represents the OLS intercept estimate for the prediction of Y from M and T, b represents the effect of the mediator on the outcome controlling for the effect of the treatment, c' represents the direct effect of the treatment on the outcome controlling for the mediator, and e_i represents the error term for subject i.

Once the path values are estimated, two primary methods exist for quantifying the indirect effect of T on Y through M (see, for example, MacKinnon, 2008). First, the indirect effect can be calculated according to the mathematical definition of the instantaneous indirect effect, θ (Stolzenberg, 1980):

$$\theta = \left(\frac{\partial M}{\partial T} \right) \left(\frac{\partial Y}{\partial M} \right). \tag{4}$$

As exposited by Hayes and Preacher (2010), the first partial derivative of a function with respect to a variable identifies the instantaneous rate of change of the former function (here, M) with respect to the second variable (here, T). In the context of multiple regression, this instantaneous rate of change is frequently interpreted as the effect of an independent variable on a criterion. Extending this to mediational analysis, the effect of T on M can be conceptualized as the rate of change of M with respect to T, $\left(\frac{\partial M}{\partial T}\right)$. Applying this to Equation 2,

$$\left(\frac{\partial M}{\partial T}\right) = a\,. \tag{5}$$

Similarly, the effect M on Y can be conceptualized as the rate of change of Y with respect to M (controlling for T) [i.e., $\left(\frac{\partial Y}{\partial M}\right)$]. Applying this to Equation 3,

$$\left(\frac{\partial Y}{\partial M}\right) = b\,. \tag{6}$$

Assuming linear relationships between T and M and between M and Y, a one-point increase in T results in an a-point increase in M, which in turn results in a b-point increase in Y. Within this framework, the mathematical formulation of θ (Equation 4) defines the indirect effect for linear single mediator models as the product of the a and b parameter estimates as follows:

$$\theta = \left(\frac{\partial M}{\partial T}\right)\left(\frac{\partial Y}{\partial M}\right) = ab\,. \tag{7}$$

More intuitively, path tracing rules (see, for example, Kline, 2005) applied to Figure 2 provide the same results.

Alternatively, the indirect effect may be defined as the difference between the total effect of T on Y and the direct effect of T on Y accounting for M. Mathematically, this conceptualization of the indirect effect is calculated as $(c - c')$, where c and c' stem from Equations 1 and 3, respectively. In standard single mediator model analyses, the *ab* and $(c - c')$ methods are equivalent under OLS regression (MacKinnon, 2008).

Use of OLS regression for estimating the various paths depicted in Figures 1 and 2 (see Equations 1 through 3) works well for single-level datasets with no clustering of level-one units within higher-level units. However, treatments, mediators, and outcomes are frequently nested within various hierarchical units as a result of both group-level interventions and the natural clustering often inherent in organizational settings. Mediational analyses involving these types of data require proper parameterization via hierarchical linear modeling techniques to correctly partition variance at the appropriate levels. The following section describes this use of multilevel modeling to investigate mediation with clustered data.

MULTILEVEL MEDIATION

Clustered data presents problems for conventional mediational analysis as described in the previous section. More specifically, the dependence of observations within a particular cluster results in inaccurate variance component partitioning if the clustering is ignored. In standard single-level multiple regression, correlated errors, as induced by the nesting of individuals within a level-two unit, lead directly to underestimated standard errors, overestimated test statistics, and inflated type I error rates (Raudenbush & Bryk, 2002; Barcikowski, 1981; Moulton, 1986; Scariano & Davenport, 1987; Scott & Holt, 1982; Walsh, 1947). Failure to properly model all levels of nesting further complicates the inferences based on hypothesis tests (Moerbeek, 2004). The severity of the clustering effect can be assessed with the intraclass correlation (ICC), interpreted as the correlation between individuals belonging to the same level-two unit:

$$ICC = \frac{MS_B - MS_W}{MS_B + (k-1)MS_w}, \tag{8}$$

where MS_B and MS_W represent the mean squared error between and within clusters, respectively, and k represents the number of subjects in each cluster (MacKinnon, 2008). The ICC ranges from $\frac{-1}{(k-1)}$ to 1, with positive values indicating a violation of the independence assumption required in standard multiple regression analyses. The ICC is chi-square distributed, and as such can be tested for significance according to the following F-statistic (MacKinnon, 2008):

$$F_{g-1,g(k-1)} = \left(\frac{1+(k-1)ICC}{1-ICC}\right), \tag{9}$$

where g is the number of level-two clusters and k is the number of subjects in each cluster. In reality, although the ICC may not be statistically significant for all clustered data structures, small values can still distort significance tests (Kreft, 1996; Muthén & Satorra, 1995). As such, non-zero ICC values require proper modeling via hierarchical linear modeling methods.

Proper mediation model parameterization of clustered observations depends largely on the level at which the treatment, mediator, and outcome variables are measured. For example, in the context of group-based treatments, cluster randomized trials assign clusters of individuals to treatment conditions but assess outcomes at the level of the participant. The $L_T \rightarrow L_M \rightarrow L_Y$ notation (where L_k represents the level at which variable k is measured) is commonly used to distinguish the different possible mediation model designs. For example, in a cluster randomized trial, if the hypothesized mediator is measured at the individual level, this design is referred to as an "upper-level" or "2→1→1" mediation design (Kenny, Kashy, & Bolger, 1998). Alternatively, the mediator in a cluster randomized trial may be measured at the cluster level resulting in a "2→2→1" mediation design. These designs, when parameterized to account for clustering at level-two, produce fixed indirect effect estimates with the same statistical properties as those resulting from single-level designs.

For example, a typical 2→1→1 cluster randomized trial may randomly assign level-two units (e.g., treatment sites) to different treatments with outcomes and mediators measured at the level of the individual subjects within each site. In this instance, estimates of a, b, c, and c' should be obtained using multilevel modeling. Using standard HLM notation (Raudenbush & Bryk, 2002), the c parameter for the total effect of the treatment on the outcome is estimated by the following model for the outcome at level-one for subject i in site j:

$$Y_{ij} = \beta_{0(Y.T)j} + r_{ij}, \tag{10}$$

and at level-two:

$$\beta_{0(Y.T)j} = \gamma_{00} + cT_j + u_{0j}, \tag{11}$$

where Y_{ij} represents the outcome for subject *i* in site *j*, γ_{00} represents the overall intercept across all sites, *c* represents the total effect of the treatment on the outcome, T_j represents the level of treatment received in site *j*, and r_{ij} and u_{0j} represent level-one and level-two random effects, respectively. Here, clustering is accounted for by modeling $\beta_{0(Y.T)j}$ as randomly varying across level-two units, indicated by the inclusion of the level-two residual, u_{0j}. This model assumes that the level-one residuals, e_{ij}, are independently and normally distributed with a mean of zero and a constant variance. Additionally, the level-two residuals, u_{0j}, are assumed independently and normally distributed with a mean of zero and a constant variance in addition to being independent of the level-one residuals.

To estimate the *a* parameter, the mediator model for subject *i* in site *j* at level-one is specified as:

$$M_{ij} = \beta_{0(M.T)j} + r_{ij}, \tag{12}$$

and at level-two:

$$\beta_{0(M.T)j} = \gamma_{00} + aT_j + u_{0j}, \tag{13}$$

where M_{ij} represents the score on the mediator for subject *i* in site *j*, γ_{00} represents the overall intercept across all sites, *a* represents the effect of the treatment on the mediator, T_j represents the treatment condition administered to cluster *j*, and r_{ij} and u_{0j} represent level-one and -two residuals, respectively. Again, level-one residuals are assumed independently and normally distributed with a mean of zero and a constant variance. Level-two residuals are assumed independently and normally distributed with a mean of zero and a constant variance in addition to being independent of the level-one residuals. Similarly, b and c' estimates are obtained from a multilevel model for the outcome variable with the following equation at level-one:

$$Y_{ij} = \beta_{0(Y.MT)j} + bM_{ij} + r_{ij}, \tag{14}$$

and at level-two:

$$\beta_{0(Y.MT)j} = \gamma_{00} + c'T_j + u_{0j}, \tag{15}$$

where Y_{ij} represents the outcome score for subject i in site j, b and c' correspond to the paths depicted in Figure 2, γ_{00} represents the overall intercept across all sites, and r_{ij} and u_{0j} represent level-one and -two residuals, respectively. Statistical assumptions for the level-one and level-two residuals are identical to those mentioned above for Equations 10 through 13. The estimate of a obtained in Equation 13 is multiplied by the estimate of b in Equation 14 to obtain the indirect effect, which can then be tested for statistical significance (MacKinnon, 2008). Alternatively, the c' parameter obtained in Equation 15 can be subtracted from the c parameter obtained in Equation 11 to yield a $(c - c')$ estimate of the indirect effect. Although the ab and $(c - c')$ methods for calculating the indirect effect are not mathematically equivalent in multilevel mediation models, the difference in the indirect effect calculated by the ab method and the $(c - c')$ method is typically negligible (MacKinnon, 2008).

As an alternative to the 2→1→1 design, treatment sites (i.e., level-two units) may be randomly assigned to treatment conditions, with a mediator measured at the site level and the outcome measured at the individual level. This results in a 2→2→1 design, again requiring a series of multilevel models to estimate the relevant parameters. First, the total effect, c, of the treatment on the outcome is specified as in the 2→1→1 design (Equations 10 and 11). Next, because the mediator and the treatment variables are measured at the same level (level-two), OLS regression is used to estimate the effect of the treatment on the mediator (a). This is modeled as in Equation 2. Finally, since the outcome, Y, is nested within level-two units, a multilevel model is used to model this clustering in estimating the b and c parameters. At level-one, the model for the outcome for subject i in level-two unit j is:

$$Y_{ij} = \beta_{0(Y.MT)j} + r_{ij}, \qquad (16)$$

and at level-two:

$$\beta_{0(Y.MT)j} = \gamma_{00} + bM_j + c'T_j + u_{0j} \qquad (17)$$

where γ_{00} represents the overall intercept, and all other parameters are defined as above. As before, this model assumes that the level-one residuals are independently and normally distributed with a mean of zero and a constant variance. Additionally, the level-two residuals are assumed independently and normally distributed with a mean of zero and a constant variance in addition to being independent of the level-one residuals. As with 2→1→1 designs, ab or $(c-c')$ estimates of the indirect effect can be calculated from their respective parameter estimates. These multilevel specifications of 2→1→1 and 2→2→1 cluster randomized designs are not exhaustive of all cluster randomized data structures or model specifications. See Pituch and Stapleton (2008) and Pituch, Stapleton, and Kang (2006) for other exemplar multilevel model specifications.

In contrast to designs where sites or clusters are randomly assigned to treatment conditions, an intervention may be randomly assigned to individuals that are clustered within higher-level units. In these "1→1→1" or "lower-level" mediation designs (Kenny, Korchmaros, & Bolger, 2003), all variables included in the mediation analysis are measured at the lowest level (level-one). These designs may contain either fixed or random indirect effect estimates that may require unique considerations. Given the clustering of individuals within relevant contexts (e.g., students within classrooms, patients within hospitals, etc.) multilevel modeling must be used to handle the resulting dependence of observations.

In lower-level mediation models, the total effect c is estimated with the following model at level-one:

$$Y_{ij} = \beta_{0(Y.T)j} + c_j T_{ij} + r_{ij}, \tag{18}$$

and at level-two:

$$\begin{cases} \beta_{0(Y.T)j} = \gamma_{00} + u_{0j} \\ c_j = c + u_{1j} \end{cases}, \tag{19}$$

where Y_{ij} represents the outcome for subject i in cluster j, γ_{00} represents the overall intercept, T_{ij} represents the value on the treatment variable for subject i in cluster unit j, c_j represents the total effect of the treatment on the outcome for cluster j, r_{ij} represents the level-one residual, and u_{0j} and u_{1j} represent random effects corresponding to $\beta_{0(Y.T)j}$ and c_j, respectively. This model assumes that the level-one residuals are independently and normally distributed with a mean of zero and a constant variance. The level-two residuals are assumed independently and normally distributed with a mean of zero and a constant variance in addition to being independent of the level-one residuals. In this instance with multiple level-two residuals, however, the level-two residuals are allowed to covary with each other. The *a*, *b*, and *c′* parameters can be obtained by estimating the following multilevel model for the mediator, *M*, at level-one:

$$M_{ij} = \beta_{0(M.T)j} + a_j T_{ij} + r_{ij}, \tag{20}$$

and at level-two:

$$\begin{cases} \beta_{0(M.T)j} = \gamma_{M0} + u_M \\ a_j = a + u_{1j} \end{cases}, \tag{21}$$

and for the outcome, *Y*, at level-one:

$$Y_{ij} = \beta_{0(Y.MT)j} + b_j M_{ij} + c' T_{ij} + r_{ij}, \tag{22}$$

and at level-two:

$$\begin{cases} \beta_{0(Y.MT)j} = \gamma_{Y0} + u_{Yj} \\ b_j = b + u_{2j} \\ c'_j = c' + u_{3j} \end{cases}, \tag{23}$$

where *a*, *b*, and *c'* represent the corresponding parameters used in the indirect effect calculation. The statistical assumptions for the residuals are identical to those for the residuals in Equations 18 and 19. The intercept terms $\beta_{0(M.T)j}$ and $\beta_{0(Y.MT)j}$ have similar random effects specifications at level-two, indicating that the intercept for each level-two unit varies from the overall mean intercept. In Equations 21 and 23, the effects of the *a*, *b*, and *c'* parameters are modeled as varying across level-two units, as indicated by the presence of the u_{1j} term in Equation 21 and the u_{2j} and u_{3j} terms in Equation 23. In this scenario with random *a* and *b* parameters, however, the expected value of the *ab* product is no longer the product of the individual fixed effects parameters. Instead, the expected value for the indirect effect is as follows (Goodman, 1960):

$$E(a_j b_j) = ab + \sigma_{ab}, \tag{24}$$

where a_j and b_j represent random variables, and σ_{ab} represents the covariance between them. This covariance term must be included for unbiased estimation of the indirect effect. However, if either the *a* or the *b* parameter is modeled as fixed, (i.e., if u_{1j} or u_{2j} are removed from the model in Equations 21 and 23), the indirect effect is now fixed with an expected value equal to the *ab* product as before (Kenny et al., 2003).

Bauer et al. (2006) outlined a method for dataset configuration and linear model parameterization that provides estimates of *a*, *b*, and σ_{ab} when calculating the indirect effect in a 1→1→1 design. To do so, Bauer et al. combine the models for both the mediator (Equations 20 and 21) and the outcome (Equations 22 and 23) into a single equation by including a dummy coded variable for each portion of the model pertaining to each outcome. Combining these two models' equations to allow estimation of a single model requires stacking the dataset so that each subject's data is contained in two observations or rows. One observation contains variables relevant to the mediator model (Equations 20 and 21), and the other observation contains variables necessary for the outcome model (Equations 22 and 23). Each observation also contains two dummy-coded indicators, S_Y and S_M, coded such that S_Y equals one if the associated observation contains data for the outcome model and zero otherwise, and S_M equals one if that observation contains data for the mediator model and zero otherwise.

For $S_Y = 1$, a new outcome variable, Z, contains Y values, and for $S_M = 1$, $Z = M$. This results in the following model at level-one:

$$Z_{ij} = S_M(\beta_{0(M.T)j} + a_j T_{ij}) + S_Y(\beta_{0(Y.MT)j} + b_j M_{ij} + c'_j T_{ij}) + r_{Zij}\,, \tag{25}$$

and at level-two:

$$\begin{cases} \beta_{0(M.T)j} = \gamma_{M0} + u_{Mj} \\ a_j = a + u_{1j} \\ \beta_{0(Y.MT)j} = \gamma_{Y0} + u_{Yj} \\ b_j = b + u_{2j} \\ c_j = c + u_{3j} \end{cases}, \tag{26}$$

producing the following combined equation:

$$Z_{ij} = S_M[(\gamma_{M0} + u_M) + (a + u_j)T_{ij}] + S_Y[(\gamma_{Y0} + u_{Yj}) + (b + u_{2j})M_{ij} + (c' + u_{3j})T_{ij}] + r_{Zij}\,. \tag{27}$$

This specification simultaneously provides all parameters for both the mediator and outcome model specifications, including covariance estimates for all model parameters. The random effects in Equation 26 are assumed normally distributed with means equal to the average population effects, and a covariance structure that, at its most parameterized, can assume that each random effect covaries with every other random effect. Borrowing notation from Bauer et al. (2006), the following covariance structure can be assumed:

$$\begin{pmatrix} \beta_{0(M.T)j} \\ a_j \\ \beta_{0(Y.MT)j} \\ b_j \\ c_j \end{pmatrix} \sim N\left[\begin{pmatrix} \gamma_{M0} \\ a \\ \gamma_{Y0} \\ b \\ c \end{pmatrix}, \begin{pmatrix} \sigma^2_{\beta_{0(M.T)j}} & & & & \\ \sigma_{\beta_{0(M.T)j},a_j} & \sigma^2_{a_j} & & & \\ \sigma_{\beta_{0(M.T)j},\beta_{Yj}} & \sigma_{a_j,\beta_{0(Y.MT)j}} & \sigma^2_{\beta_{0(Y.MT)j}} & & \\ \sigma_{\beta_{0(M.T)j},b_j} & \sigma_{a_j,b_j} & \sigma_{\beta_{0(Y.MT)j},b_j} & \sigma^2_{b_j} & \\ \sigma_{\beta_{0(M.T)j},c_j} & \sigma_{a_j,c_j} & \sigma_{\beta_{0(Y.MT)j},c_j} & \sigma_{b_j,c_j} & \sigma^2_{c_j} \end{pmatrix}\right]. \quad (28)$$

All level-two random effects are assumed independent of the level-one residuals. The covariance between the *a* and *b* estimates, σ_{ab}, can then be obtained from estimating this model. Alternatively, the $(c-c')$ method of calculating the indirect effect avoids the need to estimate the covariance between the *a* and *b* parameters. This is because the total effect, *c*, in a 1→1→1 mediation model is defined according to the following equation (Kenny et al., 2003):

$$c = c' + ab + \sigma_{ab} \quad (29)$$

Subtracting *c'* from both sides of the equation provides the following formula for $(c-c')$ in 1→1→1 mediation:

$$(c-c') = ab + \sigma_{ab}. \quad (30)$$

Hence, in the 1→1→1 scenario, the $(c-c')$ method for calculating the indirect effect provides an easier solution to the problem of estimating the indirect effect because it eliminates the need to estimate the covariances between any parameters. Again, although the *ab* and $(c-c')$ methods for calculating the indirect effect are not mathematically equivalent in multilevel mediation models, the difference is typically negligible (MacKinnon, 2008). With the *a*, *b*, *c*, and *c'* parameters thus calculated, the statistical significance of the indirect effect may be tested. The following section outlines commonly used methods of assessing the statistical significance of indirect effects, identifying the strengths and weaknesses of each test.

TESTS OF THE STATISTICAL SIGNIFICANCE OF THE INDIRECT EFFECT

Once the indirect effect is calculated, various procedures exist to assess its statistical significance. Of these, the causal steps approach outlined by Baron and Kenny (1986) is by far the most commonly used procedure (Preacher & Hayes, 2008). The causal steps procedure requires that parameters c, a, and b be sequentially statistically significant in order to infer mediation. Partial or complete mediation is then based on the significance of the c' parameter. This approach, although computationally simple, lacks statistical power and lowers the observed Type I error rate (MacKinnon, 2008; Pituch, Whittaker, & Stapleton, 2005). Additionally, some researchers (Collins, Graham, & Flaherty, 1998; Judd & Kenny, 1981; MacKinnon, Krull, & Lockwood, 2000; Shrout & Bolger, 2002) have suggested that a significant c parameter is not necessary for mediation to occur, calling into question the viability of the causal steps method.

To address this, the joint significance approach (MacKinnon, Lockwood, Hoffman, West, & Sheets, 2002) has been suggested. This approach requires that both the a and b parameters be statistically significant in order to infer mediation. This procedure does not require statistical significance of the estimate of the c parameter. This approach has been found to improve power when compared to the causal steps approach when used with single-level designs (MacKinnon et al., 2002), although use of this approach is often criticized for failing to provide a confidence interval for the indirect effect (e.g., Pituch et al., 2006). Additionally, simulation studies have found that this approach provides lower power in multilevel designs than other statistical tests of the indirect effect (Pituch et al., 2005; Pituch et al., 2006).

In an effort to provide confidence limits for the indirect effect, Sobel (1982, 1986) used Gaussian confidence limits through the calculation of a z-score based on the *ab* product and its standard error. However, the assumption of normality associated with use of a standard normal critical statistic is violated because the sampling distribution of estimates of the *ab* product is skewed, kurtotic, or both depending on the true value of the *ab* parameter (Springer & Thompson, 1966; Craig, 1936; Lomnicki, 1967). As such, z-score-based critical values and associated confidence intervals as used with Sobel's test are inappropriate. This is evidenced by simulation study results showing an asymmetry in the proportion of replications in which the true value falls to the left versus the right of the Sobel-calculated confidence interval estimates (Stone & Sobel, 1990; MacKinnon, Lockwood, & Williams, 2004).

To address these issues, bootstrap resampling methods (Shrout & Bolger, 2002) have been used to derive the empirical sampling distribution of the *ab* product estimate. Confidence intervals can be constructed using the resulting empirical distribution, with intervals excluding zero interpreted as evidence for mediation. Although several versions of bootstrapping exist, the bias corrected parametric percentile bootstrap has been shown to be the most accurate of the available methods for both single-level and multilevel designs (MacKinnon et al., 2004; Pituch et al., 2006). In multilevel designs, researchers are given two options when bootstrapping confidence intervals for the indirect effect:

bootstrapping of residuals or bootstrapping of cases. In general, bootstrapping of residuals is preferred to bootstrapping of cases for two reasons (Pituch et al., 2006). First, multilevel models assume that values for the explanatory variables are fixed across samples; resampling cases with replacement would clearly change the distribution of the explanatory variables, violating this assumption. Second, resampling cases at any one level of a multilevel design may fail to reproduce the dependency present in the data or may produce inefficient parameter estimates. Pituch et al. (2006) outline the steps required to produce appropriate bias-corrected parametric percentile bootstrap estimates of the indirect effect. To summarize their steps for a $1 \rightarrow 1 \rightarrow 1$ design:

1. Sample residuals for as many level-one and level-two units (as corresponds with the original sample) from a normal distribution with a mean of zero and variance equal to the estimated variance associated with that level.
2. Substitute the residuals and the original sample's observed values on the treatment variable into the equation for the mediator (Equations 20 and 21) to produce each case's value on the mediator variable.
3. Substitute values of the mediator obtained in Step 2 along with the resampled level-one and two residuals and treatment variable values into the equations for the outcome, Y (Equations 22 and 23).
4. Rerun the original mediation analysis (Equations 20 through 23) on the bootstrapped data to obtain estimates of the a, b, and c' parameters for each bootstrapped sample.

5. Repeat steps 1 through 4 for each of the number, n_b, of bootstrapped samples required (typically 1,000).
6. Compute the *ab* product for each of the n_b replications. The values at the 2.5th and 97.5th percentile in this distribution serve as the lower and upper limits of a non-bias corrected 95% confidence interval.
7. Calculate the z-score (z_0) in the standard normal distribution that corresponds to the percentile of the original *ab* estimate in the sample of 1,000 bootstrap estimates.
8. Calculate $2z_0 \pm 1.96$ and identify the percentiles in the standard normal distribution associated with the resulting upper and lower limits' values. The bias-corrected upper limit of the 95% confidence interval is the bootstrap estimate of *ab* that corresponds to the percentile equivalent of $2z_0 + 1.96$. Similarly, the bias corrected lower confidence interval limit is the bootstrap estimate of *ab* that corresponds to the percentile equivalent of $2z_0 - 1.96$.

This method, while both accurate and powerful, is computationally intensive and complicated to use for most researchers.

As an alternative, the empirical M-test (MacKinnon, Fritz, Williams, & Lockwood, 2007a; Arojan, 1944) provides asymmetric confidence intervals for the *ab* estimates by providing standardized critical values based on an approximation to the *ab* product's sampling distribution. This procedure performs similarly to bootstrap resampling methods, providing comparable statistical performance for single-level designs (MacKinnon et al., 2004), multilevel designs (Pituch et al., 2005; Pituch et al., 2006), and for non-normally distributed data (Pituch & Stapleton, 2008). To facilitate utilization of the empirical-M test, MacKinnon et al. (2007a) created the PRODCLIN program to compute asymmetric confidence intervals based on this approximation to the *ab* sampling distribution given a sample's estimates of *a* and *b* and their respective standard error values. Although bootstrap resampling methods provide slightly better statistical power, PRODCLIN's ease of use and overall accuracy render it preferable to all but bootstrap resampling procedures for linear models (Pituch et al., 2006).

The $(c - c')$ estimate of the indirect effect may also be tested for statistical significance by dividing this estimate by one of many analytically derived standard errors (see MacKinnon et al., 2002, for specific standard error formulations), and comparing the resulting value to a *t*- or *z*-distribution. As previously mentioned, the $(c - c')$ method is identical to the *ab* method under OLS regression, but is slightly different from the *ab* estimate in multilevel analyses because of the discrepancy in the weighting matrices used to estimate the relevant fixed effects (Krull & MacKinnon, 2001). Although this difference is typically negligible, the *ab* estimator of the indirect effect is more efficient than the $(c - c')$ estimator in multilevel analyses. Additionally, the *ab* estimator provides information regarding specific indirect effects in multiple mediator models (Krull & Mackinnon, 1999), whereas the $(c - c')$ estimator provides only an estimate of the total mediated effect (Krull & MacKinnon, 1999). Furthermore, all but bootstrap resampling methods for significance testing of the indirect effect estimate assume linear relationships between T, M, and Y. However, this assumption may be invalid. For example, the relationship between the exogenous variable, T, and the mediator, M, could be nonlinear. The mediator could be a discrete, dichotomous variable containing only values of zero or one, requiring a nonlinear specification to model the relationship between T and M. The particular function of interest in the current study refers to scenarios in which the relationship between T and M follows an ogive pattern (such as the functions depicted in Figure 3). This logistic ogive pattern can also occur when the interval-scaled mediator is a measure of a construct on which the scores exhibit a ceiling and/or floor effect.

Although the current investigation is specifically focused on modeling a nonlinear relationship between T and M, mediational analysis for nonlinear relationships is historically rooted in the development of path analytic methods for dichotomous outcomes (see Winship & Mare, 1983). As such, the following section outlines the issues associated with nonlinear mediation for dichotomous outcomes, as these are special cases of a more generalized nonlinear trajectory. This is followed by a discussion of a generalized approach to nonlinear mediation for mediating variables that are not necessarily dichotomous but with scores that exhibit floor and ceiling effects.

NONLINEAR MEDIATION FOR DICHOTOMOUS OUTCOMES

Dichotomous outcomes require special consideration in the context of statistical modeling. In many investigations, the outcome of interest, Y, consists of a dichotomous variable used to indicate the presence or absence of a specific condition. For example, in mediational analyses in the field of medicine, Y may represent a binary variable indicating the presence or absence of a specific disease in a patient after undergoing some intervention, T. In this situation, conventional linear analyses of the indirect effect are no longer appropriate for three primary reasons (MacKinnon, 2008). First, linear analyses produce predicted values outside of the range of possible values (of zero to one). For example, using standard multiple regression to predict a binary outcome can result in predicted values less than zero or greater than one for certain observed combinations of the independent variables. Second, the standard errors of the regression coefficients are inaccurate, complicating the interpretation of the ensuing inferential statistics. Finally, the residuals for models containing binary outcomes are not normally distributed, directly violating the assumption of normally distributed errors associated with linear regression model estimation techniques. Given these issues, standard linear models (and their multilevel extensions) are inappropriate in cases where the criterion of interest is binary.

Logistic regression addresses problems associated when estimating models with binary outcomes and is the method most commonly used to analyze dichotomous dependent variables (Hosmer & Lemeshow, 2000). This class of procedures provides upper and lower asymptotes of zero and one, respectively, which correspond to the maximum and minimum values of the observed dichotomous outcomes. These models estimate the log-odds of success on the outcome variable according to the following specification:

$$\ln\left(\frac{p_i}{1-p_i}\right) = \beta_0 + \beta_1 X_1 + \ldots + \beta_n X_n , \tag{31}$$

where p_i is the probability of success on the outcome variable for case i, $1-p_i$ is the probability of failure for case i, $\left(\frac{p_i}{1-p_i}\right)$ is the odds of success, X_1 through X_n are a set of n predictor variables, and β_0 to β_n are the parameter estimates associated with their respective independent variables. Solving for the probability of success (e.g., Howell, 2002):

$$p_i = \frac{1}{1+\exp[-(\beta_0+\beta_1 X_1+...+\beta_n X_n)]}. \quad (32)$$

Previous research on nonlinear mediation analysis has focused primarily on scenarios with a dichotomous distal outcome variable, *Y* (Mackinnon, 2008). In these analyses, path *a* in Figure 2 can be estimated via conventional linear methods when it is assumed that the relationship between *T* and *M* is linear. However, paths *b*, *c*, and *c*' require the use of logistic techniques when *Y* is dichotomous. In this situation, binary outcomes are commonly modeled as having continuous underlying counterparts whose latent values are deterministically or stochastically related to the observed dichotomous outcome (Winship & Mare, 1983). Although several latent variable conceptualizations exist to model the relationship between the unobserved latent variable, *Y**, and the observed dichotomous outcome, *Y*, the threshold model as outlined by Winship and Mare is commonly used to specify this relationship in meditational analysis (e.g., MacKinnon, D. P., Lockwood, C. M., Brown, C. H., Wang, W., & Hoffman, J. M., 2007b; MacKinnon, 2008). This model specifies that the observed dichotomous outcome, *Y*, and the unobserved latent continuous variable, *Y**, are related as follows:

$$\begin{cases} Y = 1\, if\ Y^* \geq L \\ Y = 0\, if\ Y^* < L \end{cases}, \tag{33}$$

where Y^* is assumed to have a mean of zero and a variance of one, and L is the threshold across which Y changes from zero to one. This implies that Y and Y^* are directly related through a nonlinear transformation such that all values of Y^* greater than or equal to L have been transformed to 1, and all values of Y^* below L have been transformed to zero (Winship & Mare, 1983). In turn, Y^* may be related to a set of observed continuous predictors, $X_1,...., X_n$. This relationship may be modeled linearly as:

$$Y^* = \beta_0 + \beta_1 X_1 + ... + \beta_n X_n + \varepsilon_{Y^*} \tag{34}$$

Here, β_0 through β_n are parameters to be estimated, and ε_{Y*} is an error term that is assumed to be uncorrelated with all X_n. If ε_{Y^*} is assumed to follow an extreme value distribution (i.e., is considered a value in excess of a predetermined threshold; Johnson & Kotz, 1970) then Equations 33 and 34 above define a logit, or logistic regression, model as presented in Equations 31 and 32 (McFadden, 1974; Winship & Mare, 1983). Given this framework, logistic regression methods are equivalent to structural equation models whereby a dichotomous variable serves as an indicator of an unobserved latent variable (i.e., the natural log of the odds of subject i possessing a score of one on the outcome variable). Alternatively, if ε_{Y^*} is assumed normally distributed, then Equations 33 and 34 define a probit, or probit regression, model (Winship & Mare, 1983; Hanushek & Jackson, 1977). The choice of the ε_{Y^*} distribution is somewhat arbitrary, as the logit and probit models are essentially interchangeable given the similarity between the logistic and cumulative normal distribution functions (Winship & Mare, 1983; Hanushek & Jackson, 1977). The current investigation will focus on the logistic regression conceptualization, with the understanding that probit regression provides an alternative and nearly equivalent mode of analysis.

The model specified within the threshold framework described by Equations 33 and 34 is underidentified because the scale of the unobserved latent variable *Y** is not directly observed (Winship & Mare, 1983). To address this, a scaling assumption is required regarding either the variance of *Y** or of ε_{Y^*}. Commonly, the standard logistic regression error, $\frac{\pi^2}{3}$, is substituted for ε_{Y^*} to identify the model. Winship and Mare provide a discussion of this issue.With the threshold model for a dichotomous outcome thus defined, both linear and logistic regression models are used to estimate the *a*, *b*, *c*, and *c'* parameters in a meditational analysis. First, the total effect is estimated from the logistic regression of the outcome, *Y*, on the treatment, *T*:

$$\ln\left(\frac{p_i}{1-p_i}\right) = \beta_{0(Y^*.T)} + cT_i \tag{35}$$

where p_i is the probability of subject *i* containing a 1 on the outcome variable, $\beta_{0(Y^*.T)}$ represents the intercept for the logistic equation, and all other variables are defined as before. Next, the *a* parameter is estimated linearly as in Equation 2. Finally, the *b* and *c'* parameters are estimated from the logistic regression of *Y* on *T* and *M*:

$$\ln\left(\frac{p_i}{1-p_i}\right) = \beta_{0(Y^*.MT)} + bM_i + c'T_i \tag{36}$$

where $\beta_{0(Y^*.MT)}$ is the intercept for the logistic regression, and all other parameters are defined as before. As mentioned above, the residual error in Equations 35 and 36 is fixed at $\frac{\pi^2}{3}$.

Once the scale of the residual variance in logistic regression is fixed at $\frac{\pi^2}{3}$, the scale for the latent continuous outcome variable *Y** can vary across models (MacKinnon & Dwyer, 1993; Jasti, Dudley, & Goldwater, 2008). Put differently, the variance of the dependent variable in logistic regression is fixed at $\frac{\pi^2}{3}$ regardless of the number of variables included as predictors. Consequently, because the model for the mediator (Equation 36) includes fewer predictors than the model for the total effect (Equation 35), estimates for the *b*, *c,* and *c*' parameters depend heavily on the other variables in the model, and the $(c - c')$ method of calculating the indirect effect is no longer equivalent or nearly equivalent to the *ab* method (MacKinnon & Dwyer, 1993). Under these conditions, simulation studies suggest that the *ab* estimate of the indirect effect is less biased and more robust to assumption violations than is the $(c - c')$ estimate (MacKinnon, 2008; MacKinnon & Dwyer, 1993).

Additionally, MacKinnon and Dwyer (1993) suggest standardizing estimates of the *b* parameter and its standard error. In logistic regression, this is accomplished by dividing each parameter in the logistic model by the standard error for the logistic regression model (i.e., the square root of the residual error variance for the logistic regression model). As described by Winship and Mare (1983) and MacKinnon et. al.

(2007b), the residual error variance, σ_{Y^*}, for a logistic regression mediation model is calculated as follows:

$$\sigma_{Y^*} = \sqrt{c'^2\,\sigma_T^2 + b^2\sigma_M^2 + 2c'\,b\sigma_{TM} + \frac{\pi^2}{3}}\,, \tag{37}$$

where *c'* and *b* represent the parameter estimates from the logistic regression, σ_T^2 represents the variance of the treatment variable, σ_M^2 represents the variance of the mediator, σ_{TM}^2 represents the covariance between the treatment and the mediator, and $\frac{\pi^2}{3}$ is the fixed residual variance from the logistic regression. Once standardized, model coefficients now represent the effect of a unit change in a respective independent variable in standard deviations of the latent variable *Y** (Winship & Mare, 1983). For example, the estimate of the *b* parameter standardized by dividing by Equation 37 provides an estimate of the change (in standard deviation units) on the continuous latent variable *Y** that results from of a one unit change in the mediator, *M*, controlling for the treatment variable.

Once *b* and its standard error are appropriately standardized, three methods exist for quantifying the indirect effect. First, the product of *a* with standardized *b* may be used as an estimate of the mediated effect (MacKinnon & Dwyer, 1993), and the standard error of *a* and the standardized standard error of *b* can be used to test the statistical significance of the *ab* product. This is directly analogous to the use of the *ab* product as an estimate of the indirect effect assuming a linear relationship between all variables. However, when the outcome, *Y*, is dichotomous, the *ab* method of estimating the indirect effect systematically overestimates the true mediated effect (Li, Schneider, and Bennett, 2007). This is because the true value of the indirect effect in a logistic regression analysis is defined by the instantaneous indirect effect in Equation 4. As such, the second method for quantifying the indirect effect utilizes the mathematical definition of the instantaneous indirect effect (Equation 4). In this instance, where logistic regression is used to analyze a binary outcome *Y* and the relationship between *M* and *T* is modeled linearly as in Equation 2, Winship & Mare (1983) and Li et. al. derive the true indirect effect as the instantaneous indirect effect, θ. First, the partial derivative of *M* is taken with respect to *T*:

$$\frac{\partial M}{\partial T} = a. \tag{38}$$

Next, the partial derivative of *Y* is taken with respect to *M*:

$$\frac{\partial Y}{\partial M} = b\left\{ \frac{\exp(\beta_{0(Y^*.MT)} + bM + c'T)}{[1+\exp(\beta_{0(Y^*.MT)} + bM + c'T)]^2} \right\}. \tag{39}$$

Finally, Equations 38 and 39 are multiplied to obtain the instantaneous indirect effect:

$$\theta = \left(\frac{\partial M}{\partial T}\right)\left(\frac{\partial Y}{\partial M}\right) = ab\left\{ \frac{\exp(\beta_{0(Y^*.MT)} + bM + c'T)}{[1+\exp(\beta_{0(Y^*.MT)} + bM + c'T)]^2} \right\}. \tag{40}$$

Here, the true indirect effect varies as a function of both *M* and *T*. This provides a form of moderated mediation, whereby the true indirect effect depends on both the value of the mediator and the dosage of treatment received. Hayes and Preacher (2010) recommend examining the true indirect effect at the mean of *T* as well as at one standard deviation above and below the mean of T to investigate the instantaneous indirect effect at low, average, and high values of treatment dosage. Confidence intervals for the instantaneous indirect effect at each value of the treatment dosage can be estimated via the bias-corrected parametric percentile bootstrap resampling method described above. Additionally, although meditational analyses often include dummy coded treatment indicators, Equation 40 assumes that the treatment is measured continuously. As such, the current investigation is concerned only with continuous, dosage-like treatment variables, acknowledging that additional derivations are necessary for use with binary treatment indicators. See Li et al. (2007) for a discussion.

From the formulation of the instantaneous indirect effect in Equation 40, it is evident that the instantaneous indirect effect for continuous T is simply the product of the a and b coefficients adjusted by a factor that depends on the value of the mediator and the dosage of treatment received. Because this adjustment factor will always be less than one, the ab estimate of the indirect effect will always overstate the magnitude of the mediated effect (Li et al., 2007). The third method for calculating the indirect effect utilizes the $(c - c')$ approach outlined above. However, in cases where the outcome, Y, is dichotomous, this method is generally more biased than the already biased ab method (MacKinnon et al., 2007b).

Mediating relationships involving binary mediators also require the use of nonlinear models to appropriately handle estimation of indirect effects (Li et al., 2007; Winship & Mare, 1983; Jasti et al., 2008). This form of analysis is particularly useful when the mediator indicates the presence or absence of a specific mediating condition. As in the case with dichotomous outcomes, an observed dichotomous mediator, *M*, is specified as a threshold model whereby *M* is modeled as an observed indicator of an unobserved latent variable, *M**. *M* and *M** are then related through a nonlinear transformation defined by:

$$\begin{cases} M = 1 \, if \; M^* \geq L \\ M = 0 \, if \; M^* < L \end{cases}, \tag{41}$$

where *M** is assumed to have a mean of zero and a variance of one, and *L* is the threshold across which *M* changes from zero to one. This model specification directly parallels the threshold model specification for dichotomous *Y* (see Equation 33). As before, M* may be related to a set of observed continuous predictors, $X_1, ..., X_n$. This relationship may be modeled linearly as:

$$M^* = \beta_0 + \beta_1 X_1 + ... + \beta_n X_n + \varepsilon_{M^*} \tag{42}$$

Here, β_0 through β_n are parameters to be estimated, and ε_{M^*} is an error term that is assumed to be uncorrelated with all X_n. If ε_{M^*} is assumed to follow an extreme value distribution (Johnson & Kotz, 1970) then Equations 41 and 42 define a logit, or logistic regression, model (McFadden, 1974; Winship & Mare, 1983), though probit models provide viable alternatives. As previously mentioned, the current investigation will focus on the logistic regression conceptualization.

In mediation analysis with a binary mediator, only the *a* parameter is estimated using logistic regression:

$$\ln\left(\frac{p_i}{1-p_i}\right)=\beta_{0(M^*.T)}+aT_i \tag{43}$$

where p_i is the probability of subject i having a score of one on the mediator, and all other variables are defined as above. The c parameter is modeled linearly and estimated according to Equation 1. Similarly the b and c' prime parameters are modeled according to Equation 3. These estimates are then used in calculating the indirect effect as previously defined.

As in the case of dichotomous Y, the variance for the mediator model must be fixed in order for the model to be identified. Again, the residual variance is conventionally fixed at $\frac{\pi^2}{3}$. Now, with a binary mediator instead of a binary outcome, the a path and its standard error must be standardized by dividing both by the standard error for the logistic regression model with dichotomous outcome M (Jasti et al., 2008):

$$\sigma_{M^*}=\sqrt{a^2\sigma_T^2+\frac{\pi^2}{3}} \tag{44}$$

Equation 44 contains fewer terms than the standard error for dichotomous Y in Equation 37 because the logistic regression model of the mediator (Equation 43) contains only one predictor, the treatment, T. In contrast, the model for a dichotomous Y contains two predictors, the treatment, T, and the mediator, M. This explains the simplicity of the equation for the standard error of a dichotomous mediator (Equation 44) compared to the standard error for a dichotomous outcome (Equation 37).

Assuming a linear relationship between M and Y and between T and Y, the b and c' paths can be estimated using the traditional linear model, and the product of b and standardized a provide a measure of the indirect effect. However, as is the case with binary Y, the ab product is an overestimate of the true mediated effect (Li et al., 2007). Additionally, in the presence of a binary M, the $(c-c')$ method is sensitive to the skew of the distribution of the treatment dosage, and as such should be used cautiously (Li et al., 2007). The true indirect effect is again defined as the instantaneous indirect effect described in Equation 4. With logistic regression used to analyze the relationship between a binary mediator and the treatment variable, and all other relationships modeled linearly, Winship & Mare (1983) and Li et. al. derive the true indirect effect as the instantaneous indirect effect, θ. First, the derivative of M is taken with respect to T:

$$\frac{\partial M}{\partial T} = a\left\{\frac{\exp(\beta_{0(M^*.T)} + aT)}{[1+\exp(\beta_{0(M^*.T)} + aT)]^2}\right\}. \tag{45}$$

Next, the derivative of Y is taken with respect to M:

$$\frac{\partial Y}{\partial M} = b\,. \tag{46}$$

Finally, Equations 45 and 46 are multiplied to obtain the instantaneous indirect effect:

$$\theta = \left(\frac{\partial M}{\partial T}\right)\left(\frac{\partial Y}{\partial M}\right) = ab\left\{\frac{\exp(\beta_{0(M^*.T)} + aT)}{[1+\exp(\beta_{0(M^*.T)} + aT`)]^2}\right\}. \tag{47}$$

Here, the true indirect effect varies as a function of T. This again provides a form of moderated mediation, whereby the true indirect effect depends on the level of treatment received. Equation 47 assumes that the treatment is measured continuously. As before, the instantaneous indirect effect for continuous T is simply the product of the a and b coefficients adjusted by a factor that depends on the dosage of treatment received. Since this adjustment factor will always be less than one, the ab estimate of the indirect effect will always overestimate the magnitude of the mediated effect (Li et al., 2007). As is the case for binary Y, the instantaneous indirect effect may be evaluated at the mean of T and at one standard deviation above and below the mean of T to investigate the value of the instantaneous indirect effect at low, average, and high values of the treatment dosage (Hayes & Preacher, 2010). Additionally, as previously mentioned, the bias-corrected parametric percentile bootstrap resampling method may be used to provide confidence intervals around the instantaneous indirect effect at each dosage of the treatment.

In summary, binary outcomes in mediational analyses require special considerations in estimating and in testing the statistical significance of the indirect effect. When properly formulated, this indirect effect depends on the dosage of treatment received, providing a form of moderated mediation. As an additional consideration, many constructs in the social and behavioral sciences contain properties similar to binary outcomes, particularly with regard to survey scores used as measures of underlying mediators. These surveys frequently contain minimum and maximum possible scores (termed floor and ceiling effects, respectively) resulting in data with nonlinear relationships mimicking the pattern observed with binary outcomes. These constructs also require special treatment to properly model their lower and upper asymptotes and the resulting nonlinear relationships found between the score on the mediator and other variables included in the model. The following section addresses these concerns associated with measures of mediation frequently used in the social and behavioral sciences.

NONLINEAR MEDIATION FOR CONTINUOUS MEDIATORS WITH FLOOR AND CEILING EFFECTS

Hypothesized models in the social and behavioral sciences often posit nonlinear relationships amongst variables. For example, as cited in Singer and Willett (2003), Robertson (1909) theorized that the rate at which learning occurs is proportional to the amount of learning that has previously occurred times the amount of learning that will occur in the future. Mathematically, this can be expressed as a differential equation of the form:

$$\frac{dY}{dt} = kY(\alpha - Y), \tag{48}$$

where $\frac{dY}{dt}$ is the rate of learning, Y is the amount learned by time t, α is an upper limit to the amount that can be learned, and k is a proportionality constant. This first-order differential equation has a solution of the form:

$$Y_i = \frac{\alpha_i}{1 + \pi_{0i} \exp[-(\pi_{1i} TIME_i)]} \;, \tag{49}$$

which relates learning and time through an exponential (nonlinear) function for subject i. These functions are similar to the logistic function used in the analysis of dichotomous outcomes (see Equation 32). However, the function in Equation 49 differs from the logistic function in two important ways. First, Equation 49 allows for an upper asymptote value other than one, indicated by the α_i in the numerator. Second, the π_{0i}

coefficient allows for specification of a model whereby the function crosses the Y-axis at a non-zero value. Despite these differences between the logistic function and the function described in Equation 49, both stem from the same family of nonlinear distributions.

These nonlinear relationships abound in the social and behavioral sciences (for examples, see Debreu, 1959; Yerkes & Dodson, 1908; Kahneman & Tversky, 1979; Knobloch, 2007). Thus, it is conceivable that mediation analyses might involve a nonlinear relationship between T and M stemming from use of a mediation measure, for example, that contains floor and ceiling effects. This is often the case when measures of psychological constructs are used as indicators of a mediating variable. For example, scores on criterion-referenced tests commonly include both floor and ceiling effects that should be accounted for when modeling the mediating effect. If the effect of T on M is modeled linearly, the resulting a parameter (Figure 2) can reflect the effect of the treatment (T) on unobtainable mediator values. This is analogous to modeling a binary outcome variable using a linear model specification. To address this, a nonlinear function with lower and upper asymptotes should be applied to the treatment's effect on the mediator. This requires a generalization of the nonlinear model used to analyze dichotomous mediating variables.

Mediators with floor and ceiling effects require a link function that contains lower and upper asymptotes corresponding to the mediating variables' minimum and maximum values. A generalized logistic trajectory can be used to model the effect of T on the mediator, M, as follows:

$$M_i = \alpha_1 + \frac{(\alpha_2 - \alpha_1)}{1 + \gamma_0 \exp[-(\gamma_1 T_i)]} + r_i, \qquad (50)$$

where M_i represents the value on the mediator for subject *i,* α_1 and α_2 respectively represent lower and upper mediator asymptotes, γ_0 is a pseudo-intercept parameter that influences the point at which the function crosses the ordinate axis, γ_1 is a pseudo-slope parameter that is related to the rate of change of the nonlinear function between its asymptotes, and r_i represents the residual for subject *i*. In contrast to instances involving dichotomous mediators, mediating variables containing upper and lower asymptotes are both continuous and observed. As such, the residual, r_i, is included in the model specification and its variance is directly estimable from the observed data. This precludes the need to utilize the threshold latent variable model discussed above. Figure 4 provides a path analysis diagram of the proposed nonlinear relationship between the treatment and the mediator.

Borrowing an exposition from Singer and Willett (2003), Figure 3 depicts graphs of Equation 50 for various values of γ_0 and γ_1 with α_1 and α_2 set at zero and 100, respectively. Although the γ_0 and γ_1 parameters do not have the same interpretations as in the linear model (see Equations 1 through 3), *M*-axis intercept values are clearly related to γ_0, and the rate at which the function reaches its upper asymptote is clearly related to γ_1. Thus, in keeping with the terminology introduced in Singer and Willett, γ_0 and γ_1 will be referred to as the pseudo-intercept and pseudo-slope parameters, respectively. As can be seen in the graphs in Figure 3, the larger the value of γ_1, the steeper the acceleration of the curve. The larger the value of γ_0, the larger the value of the M-axis intercept. This function simplifies to the standard logistic regression equation for binary outcomes given $\alpha_1 = 0$, $\alpha_2 = 1$, and $\gamma_0 = 1$.

The parameterization of the nonlinear model in Equation 50 is designed for use with single-level datasets. However, this parameterization is easily extended to multilevel mediation analyses. For example, in 1→1→1 mediation, the treatment's effect on the mediator may vary across level-two units. At level-one, this could be parameterized as follows:

$$M_{ij} = \alpha_1 + \frac{(\alpha_2 - \alpha_1)}{1 + \beta_{0(M.T)j} \exp[-(\beta_{1(M.T)j} T_{ij})]} + r_{ij}, \tag{51}$$

and at level-two:

$$\begin{cases} \beta_{0(M.T)j} = \gamma_{00} + u_{0j} \\ \beta_{1(M.T)j} = \gamma_{10} + u_{1j} \end{cases}, \tag{52}$$

resulting in the following combined equation:

$$M_{ij} = \alpha_1 + \frac{(\alpha_2 - \alpha_1)}{1 + (\gamma_{00} + u_{0j}) + \exp[-(\gamma_{10} + u_{1j}) T_{ij}]} + r_{ij}. \tag{53}$$

Here, γ_{00} and γ_{10}, respectively, represent the multilevel extensions of the parameters defined in Equation 50, and u_{0j} and u_{1j} represent their respective random effects. In this design, the model for the outcome could be modeled linearly as in Equations 22 and 23. The level-one and -two residuals for Equations 22, 23, and 53 are assumed to be distributed in the same manner as those described for the 1→1→1 mediation models in Equations 20 through 23.

Alternatively, Equation 50 could be extended for use with 2→1→1 mediation analyses according to the following specification at level-one:

$$M_{ij} = \alpha_1 + \frac{(\alpha_2 - \alpha_1)}{1 + \beta_{0(M.T)j} \exp[-(\beta_{1(M.T)j})]} + r_{ij}, \tag{54}$$

and at level-two:

$$\begin{cases} \beta_{0(M.T)j} = \gamma_{00} + u_{0j} \\ \beta_{1(M.T)j} = \gamma_{11} T_j + u_{1j} \end{cases}, \tag{55}$$

resulting in the following combined model:

$$M_{ij} = \alpha_1 \frac{(\alpha_2 - \alpha_1)}{1 + (\gamma_{00} + u_{0j})\{\exp[-(\gamma_{11} T_j + u_{1j})]\}} + r_{ij}. \tag{56}$$

Here, γ_{00} is the pseudo-intercept, γ_{11} is the pseudo-slope, and u_{0j} and u_{1j} respectively represent intercept and slope random effects. Level-one and -two residuals are distributed as those described for 2→1→1 mediation analyses in Equations 12 and 13. Though the asymptotes α_1 and α_2 may vary across level-two units, Equations 53 and 56 treat them as fixed effects. To obtain paths *b* and *c* in Figure 2, the model for the outcome could be modeled linearly as in Equations 14 and 15.

This specification may be further extended to include 2→2→1 mediation designs. Here, the treatment and the mediator are measured at level-two, while the outcome is measured at the level of the individual subject (level-one). Since the treatment and the mediator are both measured at the highest level in the data hierarchy, there is no clustering to model for the effect of the treatment on the mediator. Hence, the model for the mediator is:

$$M_j = \alpha_1 + \frac{(\alpha_2 - \alpha_1)}{1 + \gamma_0 \exp[-(\gamma_1 T_j)]} + r_{ij} \tag{57}$$

where γ_0 and γ_1 represent the pseudo-intercept and the pseudo-slope, respectively, and all other parameters are defined as above. The model for the outcome, *Y*, must address the nesting of individuals within level-two units by utilizing multilevel modeling techniques, and could be modeled linearly as in Equations 16 and 17. Again, level-one and -two residuals are distributed as those described for 2→2→1 mediation in Equations 16 and 17.

This series of multilevel logistic models can be used for calculating the indirect effect in one of three ways. First, the *ab* product may be used as an estimate of the indirect effect and tested for statistical significance. However, this conceptualization assumes that the treatment's effect on the mediator is constant across all dosages of the treatment. Furthermore, this conceptualization does not correspond to the mathematical definition of the instantaneous indirect effect (Equation 4). As such, as with binary mediators and outcomes, the *ab* estimate of the indirect effect tends to overestimate the true magnitude of the mediated effect (Li et al., 2007). Second, the $(c - c')$ estimate of the indirect effect may be utilized, although this estimate is sensitive to the distribution of the treatment dosage. As such, the $(c - c')$ estimate should be used cautiously if at all (Li et al., 2007). Finally, combining the mathematical definition of the instantaneous indirect effect (Equation 4) with the generalized equation for an asymptotic *M* (Equation 50) yields a third method of estimating the indirect effect. In this case, the instantaneous indirect effect provides an estimate of the indirect effect that depends on the dosage of the treatment variable. This conceptualization allows for a non-constant relationship between T and M corresponding to the nonlinear trajectory specified in Equation 50. Because the value of the instantaneous indirect effect depends on the level of the treatment, the instantaneous indirect effect is a form of moderated mediation. As such, it is recommended that the instantaneous indirect effect be calculated at the sample mean of T as well as at plus and minus one standard deviation from the mean of T to investigate the instantaneous indirect effect at low, moderate, and high dosages of the treatment variable (Hayes & Preacher, 2010). As previously mentioned, the current analysis focuses solely on models containing continuous treatment variables, with the understanding that formulations for binary treatment indicators must be derived separately (Li et al., 2007). Although the use of the instantaneous indirect effect applies to 1→1→1, 2→1→1, and 2→2→1 mediation models, the following derivation for the instantaneous indirect effect focuses only on 1→1→1 designs.

In a 1→1→1 nonlinear multilevel mediation model, as defined in Equations 53, 22, and 23, the instantaneous indirect effect would be:

$$\theta = \left(\frac{\partial M}{\partial T}\right)\left(\frac{\partial Y}{\partial M}\right) = \left\{\frac{\gamma_{00}\gamma_{10}(\alpha_2 - \alpha_1)\exp[-(\gamma_{10}T)]}{\{1+\gamma_{00}\exp[-(\gamma_{10}T)]\}^2}\right\} b. \tag{58}$$

Using this conceptualization, Equation 58 shows that the indirect effect varies as a function of the value on the treatment variable, T. With nonlinear mediation as parameterized in Equation 53, the treatment eventually reaches a point of diminishing returns as the function representing the treatment's effect on the mediator approaches its upper asymptote. This point, defined, here, as the level of treatment dosage optimization, corresponds to the point of inflection on the logistic trajectory specified in Equation 53. The point of treatment dosage optimization holds important implications for optimizing the treatment effect through the mediator when the direct effect of the treatment on the outcome (*c'*) is close to zero. In these cases, the treatment's effect on the outcome is largely a result of the treatment's indirect effect on the outcome through the mediator. The point of treatment dosage optimization thus identifies a level of treatment beyond which the treatment's effect on the mediator (and, consequently, its effect on the outcome) is subject to diminishing returns. Optimization of treatment dosage can be defined mathematically as the value of T at which the rate of change of θ is zero, representing the point of inflection on the logistic trajectory. Mathematically, setting the derivative of θ with respect to T equal to zero identifies the point of inflection on the logistic trajectory. First, the derivative of θ is derived as follows:

$$\frac{d\theta}{dT} = \frac{2b(\alpha_2 - \alpha_1)(\gamma_{00}\gamma_{10}\exp[-(\gamma_{10}T)])^2}{\{1+\exp[-(\gamma_{10}T)]\}^3} - \frac{b(\alpha_2 - \alpha_1)\gamma_{10}^2\gamma_{00}\exp[-(\gamma_{10}T)]}{\{1+\gamma_{00}\exp[-(\gamma_{10}T)]\}^2}. \tag{59}$$

Next, setting $\frac{d\theta}{dT}$ equal to zero and solving for T provides the point of treatment dosage optimization, T_O:

$$T_O = \frac{\ln(\gamma_{00})}{\gamma_{10}} \tag{60}$$

The effect of the level of T on M is at a maximum at this point and starts decreasing for higher values on T.

In summary, specification of a nonlinear model for the relationship between a treatment and a proposed mediator has three primary advantages over conventional linear methods. First, mediating constructs often contain absolute maximum and minimum values that are ignored in linear parameterizations. Use of the nonlinear specification suggested here alleviates this concern. Second, utilization of the instantaneous indirect effect as defined by Stolzberg (1980; Equation 4) allows the assessment of the indirect effect as a function of the dosage of the treatment variable. Indirect effects that vary across the spectrum of treatment dosages allow investigators to specify the treatment's relationship with the mediator at all dosages of the treatment. This form of analysis is unavailable via linear mediation methods as the instantaneous indirect effect is assumed constant across all levels of the treatment. As such, nonlinear methods (when appropriate) provide additional information through their interpretation as moderated mediating relationships. Furthermore, this form of moderated mediation avoids issues with *ab* and $(c - c')$ estimates of the indirect effect commonly encountered when specifying nonlinear relationships. Finally, in instances of partial or complete mediation, nonlinear specification allows for investigation of an optimal amount of treatment dosage. This is particularly relevant to behavioral interventions designed to be administered in organizational settings. This ensures that organizations maximize the effectiveness of their interventions, thus conserving the organization's resources while ensuring that the intervention's participants are not subject to unnecessary and unbeneficial amounts of the intervention.

Although the nonlinear multilevel mediation model described in Equations 53 and 56 provides a mathematical model for a treatment-mediator relationship when the mediator has an upper- and lower-asymptote, the statistical properties and performance of the proposed model are yet to be empirically investigated. To examine use and estimation of the proposed nonlinear multilevel mediation model, a simulation study is proposed to assess the estimation of the nonlinear multilevel mediation model's parameters. The purpose of this study is to examine both the estimation of a nonlinear treatment-mediator relationship and to investigate the bias of the resulting parameter and instantaneous indirect effect estimates. The proposed simulation investigates only the $1 \rightarrow 1 \rightarrow 1$ design described in Equation 53, focusing on conditions common to the behavioral and social sciences as measures of mediating constructs with upper- and lower-asymptotes are commonly encountered in these contexts.

Chapter 3: Methods

GENERATING MODELS

To examine the estimation of the proposed nonlinear multilevel mediation model, a simulation study will be conducted for the 1→1→1 multisite experimental design in which participants are randomly assigned to a treatment dosage and are nested within data collection sites. The SAS programming environment (version 9.2) will be used to create simulated datasets according to two generating models. First, the following generating model will be used for the mediator at level-one:

$$M_{ij} = \frac{100}{1+\beta_{0(M.T)j}\exp[-(\beta_{1(M.T)j}T_{ij})]} + r_{ij}, \tag{61}$$

and at level-two:

$$\begin{cases} \beta_{0(M.T)j} = \gamma_{00} + u_{0j} \\ \beta_{1(M.T)j} = \gamma_{10} + u_{1j} \end{cases}, \tag{62}$$

Here, α_1 and α_2 are explicitly fixed at zero and 100, respectively, thereby defining a mediator that takes on values between zero and 100 along with a nonlinear relationship with the treatment dosage variable, *T*. Values for the treatment variable will be randomly selected from a normal distribution with a mean of zero and a unit variance (as in Li et al., 2007). The value of the pseudo-intercept parameter (γ_{00}) will be fixed at 150 (to match the pattern depicted in the relevant γ_{00} = 150 graph in Figure 3). Values for the pseudo-slope parameter (γ_{10}) will be specified as either zero, 0.14, 0.39, or 0.59 for use in calculating the indirect effect to parallel the simulation conditions used in Li et al.'s analysis of dichotomous mediators. The random effects (r_{ij} and u_{0j}) will be sampled from a random normal distribution with a mean of zero and standard deviation equal to one. The covariance of the level-two residuals will be set equal to zero, and the level-one residual will not be allowed to covary with either level-two residual. The outcome variable (*Y*) will be generated according to the following linear multilevel model at level-one:

$$Y_{ij} = \beta_{0(Y.MT)j} + \beta_{1j}M_{ij} + \beta_{2j}T_{ij} + r_{ij}, \qquad (63)$$

and at level-two:

$$\begin{cases} \beta_{0(Y.MT)j} = \gamma_{00} + u_{0j} \\ \beta_{1j} = \gamma_{10} \\ \beta_{2j} = \gamma_{20} \end{cases}, \qquad (64)$$

where M_{ij} comes from Equations 61 and 62. Here, γ_{10} and γ_{20} represent the *b* and *c'* parameters, respectively. Estimation of this random-intercept model with fixed treatment and mediating effects (Equations 63 and 64) is used to facilitate comparisons with similarly parameterized models from previous research regarding multisite mediation models (Pituch et al., 2005; Krull & MacKinnon, 1999, 2001; MacKinnon, Warsi, & Dwyer, 1995).

The *b* parameter for use in calculating the indirect effect (γ_{10} in Equation 64) will be specified to be zero, 0.14, 0.39, or 0.59, representing differing degrees for the relationship between the mediator and outcome variable. The intercept parameter in Equation 64 (γ_{00}) will be specified to be equal to zero, and the direct effect of the treatment on the outcome accounting for the mediator (*c'*; γ_{20} in Equation 64) will be held constant at 0.20. These parameter values stem directly from previous research on nonlinear treatment-mediator relationships (e.g., Li et al., 2007). The random effect (u_{0j}) will be sampled from a random normal distribution with a mean of zero and standard deviation equal to one.

Next, the number of sites and the number of participants within sites will be varied in a manner similar to those used in Pituch et al. (2005). More specifically, data for 10, 20, and 30 sites will be generated and completely crossed with the number of participants in each site being either 15 or 30 for a total of six combinations of sample size conditions. These values are consistent with values observed in applied multisite investigations (e.g., Plewis & Hurry, 1998; Pituch & Miller, 1999).

Finally, values for the residual ICCs will be set to either .05 or .15 in equations for both the mediator and the outcome. The covariance between level-two residuals will be set to zero. These values are consistent with those used by Krull and MacKinnon (2001) and are representative of observed values in educational research.

ESTIMATING MODELS

Once generated, two nonlinear multilevel models will be estimated to assess parameter bias for the estimated mediation model parameters and the instantaneous indirect effect at the mean treatment dosage as well as one standard deviation above and below the mean. These analyses are designed to examine both the estimation of a nonlinear treatment-mediator relationship and to investigate the bias of the resulting parameter and instantaneous indirect effect estimates. The first nonlinear multilevel model estimated will be identical to the model used to generate values on *M* as a function of *T* (see Equations 61 and 62) representing a fully parameterized model with all level-one parameters varying across level-two units. The proposed simulation will use SAS PROC NLMIXED to estimate this nonlinear relationship. The model for the outcome will also be identical to the generating model (Equations 63 and 64). In this model, the *b* parameter (γ_{10} in Equation 64) is modeled as fixed, resulting in a fixed instantaneous indirect effect and eliminating the need to simultaneously estimate all parameters in the model.

The second nonlinear model estimated will include only a random pseudo-intercept to address the clustering of participants within sites while assuming a constant treatment effect on the mediator across all level-two units. This random pseudo-intercept model results in the level-one specification described in Equation 61 and a level-two specification as follows:

$$\begin{cases} \beta_{0(M.T)j} = \gamma_{00} + u_{0j} \\ \beta_{1j} = \gamma_{10} \end{cases} . \tag{65}$$

Again, the estimated model for the *b* and *c* parameters will be identical to the generating model in Equations 63 and 64. In this model, both the pseudo-slope (γ_{10} Equation 65) and the *b* parameter will be modeled as fixed across level-two units, as indicated by the lack of a residual term in each level-two equation. This again results in a fixed instantaneous indirect effect, eliminating the need to simultaneously estimate all parameters.

ANALYSIS

Considering all combinations of conditions, this study will entail a 4x4x3x2x2 fully factorial design, resulting in 192 unique conditions. 1,000 replication datasets will be generated for each condition. The instantaneous indirect effect for each replication will be calculated according to Equation 58 at the mean of T and at one standard deviation above and below the mean of T. Confidence intervals for the instantaneous indirect effect will be established according to the bias-corrected parametric percentile bootstrapping procedure outlined by Pituch et al. (2006) for average treatment values as well as at treatment values one standard deviation above and below the average. Coverage rates for the proportion of replications containing the true value of the instantaneous indirect effect for each condition will be reported, as will the proportion of times the true instantaneous indirect effect lies to the left or to the right of the 95% bias-corrected parametric percentile bootstrapped confidence interval.

The relative parameter bias will be assessed for the pseudo-slope parameter (γ_{10} in Equations 62 and 65) and for the instantaneous indirect effect (Equation 58) at the mean value of T and at one standard deviation above and below the mean value of T. These bias estimates will be used to assess estimation of the nonlinear model for the mediator. The relative parameter bias for the b parameter (γ_{10} in Equation 64) will be compared to previous bias estimates (i.e, Pituch et al., 2006) to ensure proper model estimation. Relative parameter bias will be estimated according to the following equation:

$$B(\hat{\theta}) = \frac{\bar{\hat{\theta}}_i - \theta}{\theta}, \tag{66}$$

where $\bar{\hat{\theta}}_i$ equals the represents the average parameter value across all replications for one study condition(Hoogland & Boomsma, 1998). Hoogland and Boomsma's recommended cutoff for substantial parameter bias ($\left|B(\hat{\theta})\right| \geq 0.05$) will be used to assess the severity of the observed bias.

Chapter 4: Discussion

The proposed simulation study will assess the statistical performance of the aforementioned nonlinear multilevel mediation model. The ability of nonlinear parameterizations to provide both a point of treatment dosage optimization and an instantaneous indirect effect for each value of the treatment provides an additional advantage over the type of information available from conventional methods.

Estimation issues will likely arise when using the nonlinear model. More specifically, default estimation procedures in SAS 9.2 will likely require modification in order to facilitate model estimation. Future research should explore the relationship between the optimization and integration techniques used to achieve model convergence and how well the ensuing parameter estimates recover the true parameters' values.

Although the current investigation provides nonlinear specifications for 1→1→1, 2→1→1, and 2→2→1 designs, the proposed simulation study will only examine the statistical performance of the 1→1→1 specification. Future research should focus on the 2→1→1 and 2→2→1 designs that are commonly encountered in cluster randomized trials. In addition, the proposed study will only examine multilevel designs containing at most two-levels consisting of a nonlinear relationship between the treatment and the mediating variables. Nonlinear parameterizations could easily be extended to 3-level designs (e.g., Pituch, Murphy, & Tate, 2010) or to designs specifying nonlinearity between the mediating and the outcome variables. Future research should examine these possibilities. Nonlinear multilevel mediation may also exist in the context of repeated measures or longitudinal data analysis. In these instances, outcomes scores are measured within individuals over time. Singer and Willett (2003) suggest that these kinds of designs may lend themselves to nonlinear model specifications. Future research should examine these nonlinear longitudinal designs in the context of mediational analysis.

In the absence of an analytical solution that provides the standard error for the instantaneous indirect effect, bootstrap resampling methods offer an empirical alternative. These methods have been used to test the instantaneous indirect effect for statistical significance at a specific value of the treatment variable (Hayes & Preacher, 2010). Although several methods exist, the current investigation utilized only the bias-corrected parametric percentile bootstrap procedure. Future research should investigate the utility of other bootstrapping procedures with nonlinear models, including consideration of nonparametric alternatives.

Finally, although the proposed nonlinear multilevel mediation parameterization has roots in latent variable analysis, the current investigation did not utilize structural equation modeling methods to investigate the instantaneous indirect effect. Previous research has examined structural equation modeling as a means of providing confidence intervals around the mediated effect (Cheung, 2007; Bollen & Stine, 1990), and as a means of providing alternate specifications for meditational models (Winship & Mare, 1983). The threshold model described in the current study is one of many specifications that may be used to provide an estimate of the instantaneous indirect effect. As an alternative, multiple mediating measures may be employed as observed indicators of an unobserved latent mediating variable. This latent mediator may also be nonlinearly related to the treatment variable, resulting in a nonlinear mediation model similar to the model proposed in the current investigation. This would then necessitate estimation of a multilevel structural equation model to properly address the clustering of individuals within level-two units. Again, this is only one example of many possible alternative parameterizations. Future research should investigate the performance of nonlinear multilevel latent variable models in the context of meditational analysis.

Chapter 5: Evaluation Addendum

This addendum extends meditational analyses for use in program evaluation by placing meditational analysis within the context of quantitative evaluation. First, evaluation frameworks are discussed as a means of providing an impetus for quantitative program evaluation. Next, evaluation outcomes are placed in a meditational context as portray the similarities between quantitative program evaluation and meditational analyses. Conventional meditational analyses are then suggested for a real-world evaluation. Advantages and disadvantages of meditational methods in program evaluation are then discussed, as are implications for the field of program evaluation.

EVALUATION FRAMEWORKS

Program evaluation often takes place within the context of a specific analytic framework. Borich and Jemelka (1981) outline four specific approaches to evaluation: the decision-oriented approach, the applied research approach, the value-oriented approach, and the systems-oriented approach. Though other approaches are utilized successfully and frequently, Borich and Jemelka identify these four frameworks as prominent approaches to program evaluation.

The decision-oriented approach casts evaluators in the role of providing decision makers with information (Alkin, 1972). In this context, evaluation is the process of collecting and analyzing information and reporting summary information to decision-makers for use in the process of selecting between alternatives. The CIPP evaluation model is representative of this paradigm, whereby evaluators focus on the context, inputs, process and products of the intervention in question. In context evaluation, the evaluator's objective is to specify the scope and intent of the program to be evaluated. This requires a clear statement of the program's objectives. Input evaluation attempts to assess the capabilities and requirements of the intervention. Here, the evaluator must determine if the program is capable of accomplishing its stated objectives, and, if so, what substantive requirements are needed. Process evaluation aims to identify potential issues regarding program logistics; procedures and program implementation are assessed in an effort to streamline and optimize the intervention of interest. Finally, product evaluation relates the outcomes of interest to the context, inputs, and process of the invention in question. Here, the evaluator must determine if the program's outcomes align with its stated objectives. In this model, outcomes and program elements must be both clearly stated and measureable to be used in any ensuing analysis. As such, quantitative modes of analysis, especially those stemming from classical statistical inference and experimental design, are particularly relevant within this framework. The utility of qualitative data in this framework is limited, as this framework relies heavily on the evaluator's ability to quantify constructs of interest.

The applied research orientation aims to establish a causal relationship between the intervention in question and the outcome of interest (Borich & Jemelka, 1981). This framework borrows heavily from classical experimental design methodology to establish the necessary preconditions for causal inference. This conventionally involves random assignment of participants to conditions and contrasting treatment and control groups. As in academic research, this framework utilizes well-established statistical methodology to quantify intervention efficacy to aid in the decision making process. Though this approach gains its strength from its reliance on statistical and methodological rigor, as with the decision-oriented approach this framework precludes the use of qualitative data decision makers often desire to aid in the decision making process (Stufflebeam et al., 1971).

The value-added orientation, in contrast to the quantitative frameworks mentioned above, places evaluation in the context of subjective value judgments (Scriven, 1974). In this framework, the rigorous quantitative methods used in the decision-oriented and applied research approaches are considered to be only a portion of the data necessary to effectively evaluate an intervention. Equally important in this framework are the values held by the program's primary stakeholders. Quantitative methodology may provide some information to the evaluator, but the program itself must ultimately be justified in terms of its subjective value to the program's constituents. This places a high degree of importance on qualitative data and subjective determinations of overall program efficacy.

Finally, the systems-oriented approach takes a systems theoretical perspective on program evaluation (Borich & Jemelka, 1981). In this framework, individual elements can only be understood in the context of the complex systems they inhabit. As such, thorough and effective program evaluation requires examining not only the specific units of interest, but also the higher-level structures that subsume the lower-level units. This holistic approach to evaluation typically utilizes qualitative data, as this framework is less concerned with experimental design than it is with holistic system functionality.

In practice, evaluators typically combine frameworks in developing an evaluation strategy. Combining the quantitative aspects of the decision-oriented and applied research approaches with the qualitative aspects of the systems-oriented and value added approaches allows evaluators to utilize the strengths of each framework in the process of determining a program's efficacy. Though qualitative evaluation provides valuable information to both evaluators and decision makers, the current discussion focuses on quantitative program evaluation. As mentioned above, quantitative modes of evaluation are particularly relevant in the context of the decision-oriented and applied research frameworks. These approaches to evaluation borrow heavily from both statistical and experimental methodology, rendering them particularly useful as quantitative methods of program evaluation. Statistical mediation models fall squarely within the realm of framework-appropriate methods for the decision-oriented and applied research approaches, as both focus on evaluating objectively measured outcomes through well-established statistical methods. Though statistical mediation models may be incorporated into the value-added and systems-oriented frameworks, these approaches focus primarily on subjective value and holistic evaluations (respectively) as obtained through primarily qualitative means. As such, meditational methods are likely more relevant to evaluators operating within a decision-oriented or applied research paradigm.

For statistical mediation models to be utilized as described in the preceding document, evaluation outcomes must be clearly defined, measureable, causally related to the intervention, and chronologically related to each other. The following section describes the means by which program outcomes can be conceptualized as statistical mediators, thereby establishing the framework within which statistical mediation models may be utilized in evaluation settings.

PROGRAM OUTCOMES AS MEDIATORS

Interventions are frequently designed to affect distal outcomes through changes in mediating constructs (MacKinnon & Dwyer, 1993). Because distal processes may be harder to detect than proximal effects, evaluators must be careful to both specify the proper mediation mechanism and to analyze it appropriately. In this context, meditational analysis provides a viable tool for analyzing intervention efficacy by decomposing a program's effects into a series of mediating constructs. In this manner, program evaluators can quantitatively analyze the effect of a program on each outcome of interest by specifying the causal mechanisms by which a program is effective. This is particularly relevant to programs with higher order outcomes, as distal results may be difficult to detect with conventional analyses.

Evaluation outcomes can be framed as mediators between the intervention and the most distal outcome. Within this framework, an intervention affects a distal outcome through a series of mediating constructs. In the simplest case with one mediator and one distal outcome, the path diagram for the analysis resembles Figure 2. Here, *T* represents the intervention to be evaluated, *M* represents the mediator, and *Y* represents the distal outcome of interest. In many evaluation settings, the distal outcome is both chronologically and psychologically separated from the intervention of interest. As a result, a mediating construct is measured in close proximity to the intervention in an effort to measure the intervention's effect on a proximal outcome of interest. This proximal outcome (i.e., the mediator), is then hypothesized to affect some necessarily distal outcome that is measured sometime after the intervention is completed. Because of the lapse in time between intervention completion and distal outcome measurement, the intervention effect is likely muted and its direct effect on the distal outcome may be understated. Put differently, a behavioral intervention is most effective directly after its completion. As time passes, intervention efficacy may deteriorate due to lack of reinforcement. If the intervention is designed to affect some distal outcome that chronologically succeeds the intervention by some appreciable period of time, then the measured effectiveness of the intervention may be diluted simply because of the distal nature of the outcome. This can result in underestimated effect sizes.

In addressing this issue, properly parameterized meditational methods may provide more statistical power to detect distal effects than do conventional bivariate analyses (Shrout & Bolger, 2002). For example, in the scenario with only first and second order outcomes, the first order outcome can be operationalized as a mediator in a single mediator model as depicted in Figure 2 and analyzed accordingly. This type of analysis is described in the *Traditional Mediation* section of the main document. In the more complicated scenario, an intervention designed to affect first, second, third, and fourth order outcomes may not reveal significant effects for a fourth order outcome when analyzed via conventional bivariate methods. In this case, the first, second and third order outcomes may be conceptualized as mediators, and a multiple mediator analysis may be employed to estimate the indirect effect of the intervention on the fourth order outcome. (See Preacher & Hayes, 2008, for an exposition of multiple mediator models.) This example highlights the flexibility of meditational analysis to accommodate designs including multiple outcomes of interest, as is commonly the case in evaluation settings.

To summarize, outcomes of interest in evaluation settings can be conceptualized as either proximal mediators or distal outcomes in a statistical meditation model. This framework allows for a more powerful test of intervention efficacy, as the inclusion of a mediator ameliorates the deterioration of the treatment effect as often occurs with distal outcomes. This provides evaluators a viable quantitative tool for analyzing mediated interventions. To demonstrate this, the next section describes a real-world evaluation conducive to meditational analysis. Because the evaluation presented was terminated prior to completion, only a description of the intended analysis is provided; no data was actually analyzed in the following evaluation. Nonetheless, the conditions established in this example provide an exemplar evaluation setting in which statistical meditational analysis may be used.

EVALUATION EXAMPLE

Thomas Concept, LLC, provides healthcare organizations with consulting services and seminars designed to a) reduce conflict within the organization, b) increase employee productivity, and c) increase controllable retention rates. The program itself is based on the Power of Opposite Strengths (Thomas & Thomas, 2006), a psychological theory of individual differences that increases individuals' self-awareness and provides instruction to help manage conflict-ridden professional relationships. As organizations expand, inter-employee conflicts inevitably surface. The Power of Opposite Strengths program is designed specifically to reduce these tensions by providing both awareness of one's pattern of personality strengths and an understanding of how those strengths lead to professional conflict. With this information, employees gain the ability to effectively manage inter-office conflict, thereby increasing both worker productivity and controllable retention rates.

Mediational methods may be utilized in this context to evaluate the effectiveness of the Opposite Strengths program as it was implemented within the Baylor University Medical Gastroenterology leadership team in the fall of 2009. Though the actual evaluation was terminated prior to completion, this example provides a blueprint for implementing a meditational model to aid in program evaluation. The team of professionals evaluated in the Gastroenterology department at Baylor University Medical Center was plagued by widespread inefficiency stemming from, as the administration believed, ineffective communication and a lack of teamwork. The Opposite Strengths program was implemented in this context in two specific phases. First, Dr. Tommy Thomas, CEO of Thomas Concept, LLC, led three 3-hour (9 hours total) group educational sessions (referred to as "the Opposite Strengths seminar") with all 10 members of the Baylor University Medical Gastroenterology leadership team. Second, each team member also received 10 additional 1-hour individual sessions (10 hours total) to assist in applying material presented in the seminar. The entire program took place between October 1 and December 15, 2009. As stated above, the formal evaluation was terminated prior to the completion of the data collection process.

OUTCOMES EXAMINED

The first order outcome of interest for the Opposite Strengths program consisted of participants' overall satisfaction with the program. This was measured with a simple questionnaire at the completion of the final seminar.

The second order outcome of interest pertained to participants' retention of information presented throughout the program. At the termination of the intervention, the participants should have had a clear understanding of the theory of Opposite Strengths and how it applies to workplace conflict. This evaluation occurred as an observational report, with Dr. Tommy Thomas rating each participant's ability to internalize the information presented to them. However, for the sake of the current exposition, a hypothetical score on a post-intervention knowledge assessment will be used as a measure of participants' information retention.

Third order outcomes of interest were primarily two-fold. First, as the program was designed to reduce conflict, time spent engaged in conflict resolution should have decreased substantially. Second, the time spent by the team leader resolving conflict between team members should have decreased. These third order constructs of conflict-resolution were explicitly measured in a pre-test/post-test fashion via surveys completed by Deborah Upshaw, Director, BUMC GI. This third order evaluation is intended to determine the extent of behavior change as observed by the leader of the department.

Finally, the fourth order outcome of interest consisted of patient satisfaction scores for participated in the intervention; it is believed that these individuals had a positive effect on the remaining GI team staff, resulting in an overall increase in patient satisfaction. Figure 5 provides a schematic decomposition of the Opposite Strengths seminar.

Given the quantitative nature of outcomes of interest, this evaluation combined elements of the decision-oriented and applied research as described below.

Decision Oriented Approach

The following objectives were used to evaluate the success of the program:

- A documented decrease in the amount of executive time spent in managing the relationships within the BUMC Gastroenterology leadership team.
- A documented increase in the productive interaction of the leadership team.
- A documented increase in the satisfaction scores of the patients ultimately served by the BUMC Gastroenterology team.

Applied Research Approach

With four orders of outcomes to investigate, this evaluation was particularly susceptible to low-powered tests of intervention efficacy for the most distal (i.e., fourth-order) outcome. This, combined with the quantitative nature of the data collected, suggested that meditational models were appropriate in assessing all effects of interest.

MEDIATIONAL MODEL FOR PROGRAM EVALUATION

The Appendix describes the questions of interest and the measures used to evaluate them. Though several orders of outcomes are relevant to current evaluation, this exposition will focus on evaluating only the fourth order outcome, as this outcome represents the most distal effect of interest. In this context, the fourth-order outcome represents the effect of the Opposite Strengths program on overall patient satisfaction. As evident from Figure 5, knowledge of the Opposite Strengths principles chronologically precedes ensuing patient satisfaction. As such, knowledge of Opposite Strengths principles may be conceptualized as a mediator between the Opposite Strengths intervention and overall patient satisfaction. In this framework, the Opposite Strengths seminar serves as the treatment (*T*), outcome knowledge serves as the mediator (*M*), and subsequent patient satisfaction serves as the outcome of interest (*Y*). Given the distal nature of patient satisfaction measures, this methodology provides a more powerful design for detecting the effect of the intervention on the outcome of interest.

As an additional consideration, the evaluation design consists of patients nested within doctors who receive a specific amount of the intervention. Since not all doctors were able to attend every Opposite Strengths seminar, the treatment variable utilized in the mediation model can be conceptualized as an intervention dosage, with the number of seminars attended representing the amount of treatment received. This results in a 2→2→1 design as described in the section on *Multilevel Mediation*. Furthermore, since the mediating variable represents the amount of material learned from the Opposite Strengths program, the treatment-mediator relationship could be modeled nonlinearly according to Robertson's (1909) equation for the rate at which learning occurs (Equation 49). Combining these concepts results in the following specification for effects of the treatment on the mediator at level-one:

$$M_i = \frac{\alpha}{1 + \pi_0 \exp[-(\pi_1 T_i)]} + r_{ij} \tag{67}$$

where M_i represents the score on the retention test for doctor *i*, α represents the highest possible score on the retention test, π_0 and π_1 represent the pseudo-intercept and pseudo-slope parameters (respectively) as described above, T_i represents the number of seminars attended for doctor *i*, and r_{ij} represents the level-one residual. Here, both the mediator and the outcomes are measured at the same level (level-two; the level of the doctors who participated in the evaluation), and as such a single-level model is appropriate. The outcome (patient satisfaction; *Y*), however, is nested within doctors, requiring a multilevel specification to account for the inherent clustering. As mentioned in the primary document, the outcome, *Y*, in a 2→2→1 design may be modeled linearly according to the following equation at level-one:

$$Y_{ij} = \beta_{0(Y.MT)j} + r_{ij}, \tag{68}$$

and at level-two:

$$\beta_{0(Y.MT)j} = \gamma_{00} + bM_j + c'T_j + u_{0j}, \tag{69}$$

where Y_{ij} represents the patient satisfaction score for patient *i* seeing doctor *j*, γ_{00} represents the overall intercept, M_j represents the score on the mediator for doctor *j*, T_j represents the number of seminars attended by doctor *j*, *b* and *c'* represent the paths depicted in Figure 2, and r_{ij} and u_{0j} represent the level-one and level-two residuals, respectively. As exposited in the main document, the indirect effect stemming from a nonlinear treatment-mediator relationship must be estimated according to the formula given by Stolzenburg (1980) for the instantaneous indirect effect. Given equations 67 through 69, the indirect effect for the current evaluation model is calculated as:

$$\left\{ \frac{\pi_0 \pi_1 \alpha \exp[-(\pi_1 T_j)]}{\{1 + \pi_0 \exp[-(\pi_1 T_j)]\}^2} \right\} b, \tag{70}$$

where all parameters are estimated in equations 67 through 69. This model conceptualizes the effect of the Opposite Strengths program on patient satisfaction as operating indirectly through doctors' ability to acquire the information. As such, indirect effect of the treatment on the outcome is a function of the number of seminars a doctor attends. This is intuitively appealing, as attending additional seminars should result in the acquisition of additional information, which, in turn, should result in increased patient satisfaction.

DISCUSSION

The nonlinear multilevel mediation framework utilized in the current investigation allows for a more powerful evaluation of a distal outcome in the presence of an intervention of interest. As patient satisfaction scores would likely take some time to acquire, the use of a mediating variable will enable an evaluator to accurately measure the distal fourth-order outcome. In addition, the use of a nonlinear multilevel meditational framework suggests that the effect of the program on the distal outcome is a function of the amount of the program doctors completed. This dosage-model aspect of the current evaluation is directly relevant to both program constituents and decision makers, as it lends itself to a cost-benefit analysis based on the effect of each additional unit of treatment received. On the whole, this analysis fits within the general context of statistical mediation modeling, providing a general framework within which evaluators may choose to operate.

The current evaluation utilized nonlinear meditational methods in the context of a nested data. With patients nested within doctors, conventional methods may provide inaccurate results, which could result in premature program termination. The multilevel methods utilized in this investigation guard against this type of error. However, this particular set of models is not exhaustive of all methodologies available to an evaluator. Under certain conditions, latent variable models may provide equally viable modes of investigating program efficacy. The evaluator should be aware of these alternative methods, choosing the methodology that most closely aligns with the program's objectives.

Statistical mediation models depend on the availability of clearly defined objectives and psychometrically sound measures. Well-researched surveys with sound reliability and validity coefficients or measures scored with item response theory (IRT) models are best suited towards this end. In the absence of well-established measures, the methods exposited above may fail to provide accurate estimates of intervention efficacy. Evaluators should we weary of this particular limitation, utilizing well-understood surveys whenever possible. When well-established surveys are unavailable, latent variable models may provide a more viable mode of analysis. See Winship & Mare (1983) for a discussion of latent variable mediation models.

Meditational methods are especially useful for evaluators operating within the decision-oriented or applied research frameworks. However, as mentioned above, these frameworks are not exhaustive of all possible paradigms, and decision makers frequently solicit qualitative input in a formal program evaluation. This is especially true for evaluations taking place in the context of value-added or systems oriented approaches. While meditational methods may be utilized in tandem with qualitative approaches, meditational analysis provides no utility in evaluation settings relying solely on subjective data. As mediation methods require the implementation of a sound experimental design, evaluators should discuss explicitly the goals of the evaluation with the relevant stakeholders prior implementing the evaluation. This will maximize the validity of the inferences resulting from a meditational analysis.

Overall, meditational methods provide a valuable tool for use in quantitative program evaluation. When combined with qualitative assessments, these methods provide evaluators, stakeholders, and decision makers with an abundance of data on which to base their ultimate decisions. Mediational models should not be considered in a vacuum, but rather as part of larger evaluation framework that solicits both qualitative and quantitative input. Utilized in this manner, meditational models can provide valuable information for any evaluation.

Figure 1: Total effect, c, of treatment T on outcome Y.

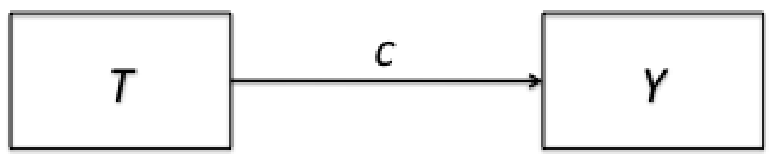


Figure 2: Single-level linear mediation model of treatment *T*'s effect on outcome *Y* through mediator *M*.

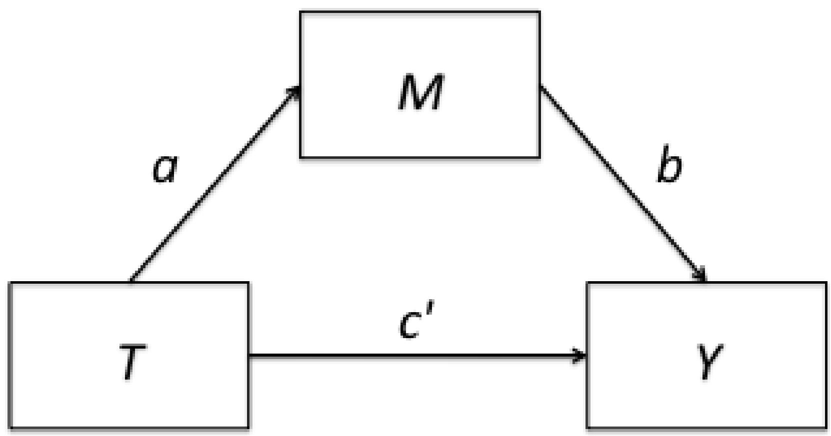


Figure 3: Nonlinear parameters and their effects on the logistic change trajectory.

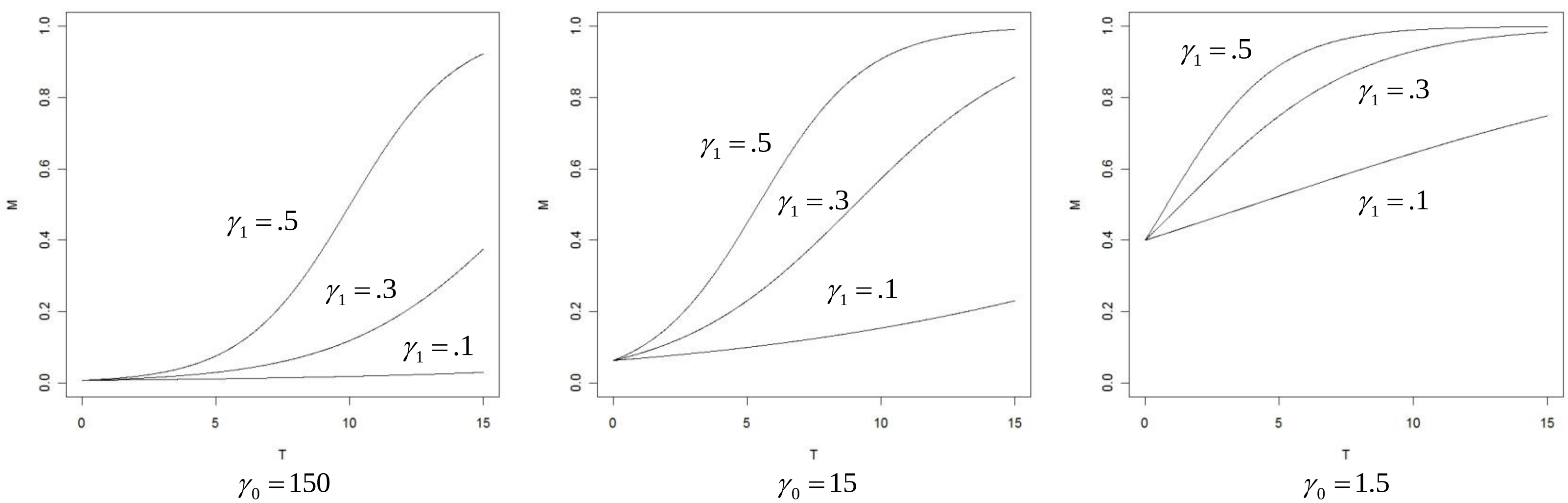


Figure 4: A mediation model in which the relationship between the treatment and the mediator is modeled nonlinearly.

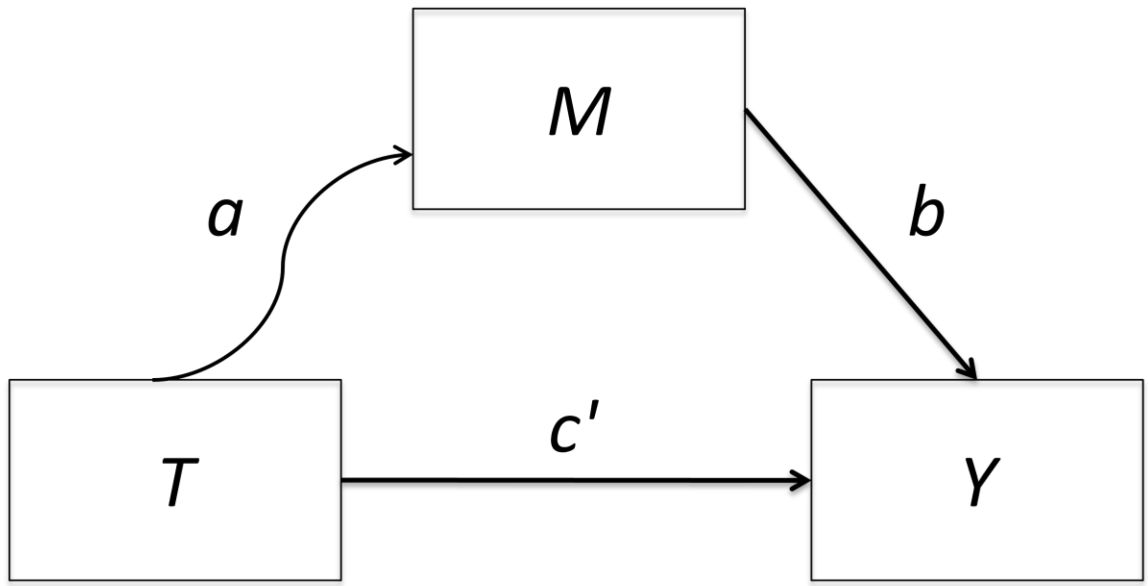
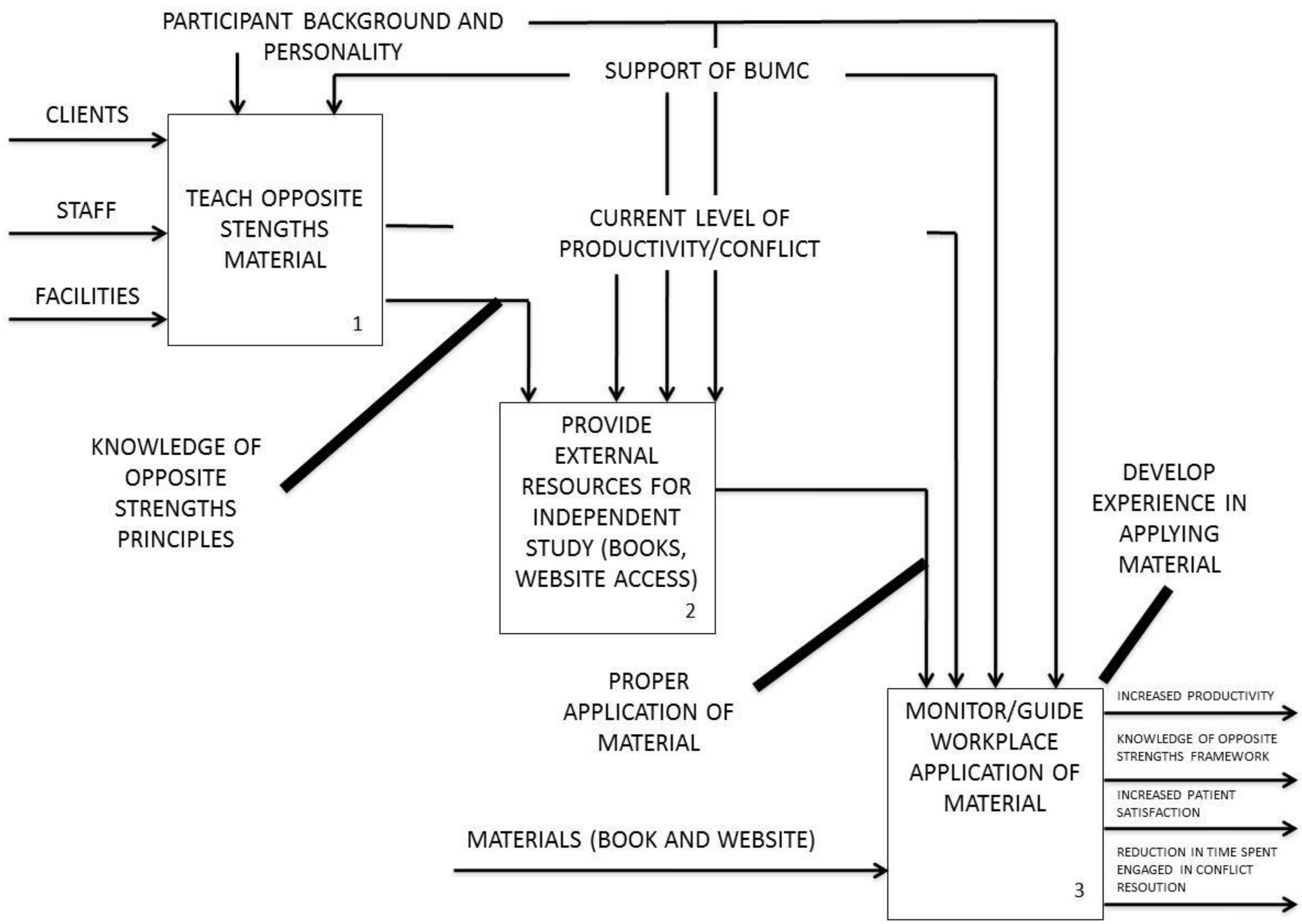


Figure 5: Schematic decomposition of the Opposite Strengths Program.



Appendix

A. Are the participants satisfied with the Opposite Strengths seminar?
   a. Variable to be measured: Participant satisfaction.
   b. Instrumentation: Online survey/questionnaire developed by Thomas Concepts, LLC.
   c. Analysis procedures: compute summary statistics for each individual, provide summary data in evaluation report indicating overall satisfaction scores, provide analyses of individual questions to determine which specific aspects of the program are exemplary or need substantial improvement.

B. Are the participants retaining the information presented in the seminar?
   a. Variable to me measured: knowledge of Opposite Strengths material.
   b. Instrumentation: Observer report. Dr. Tommy Thomas, leader of the Opposite Strengths seminars, will provide an observational write-up based on his interactions with the seminar's participants.
   c. Analysis procedures: this observational report will be included as part of the larger evaluation study. A formal assessment of knowledge may take place at a later date.

C. After presentation of the Opposite Strengths material, does the GI team leader spend less time engaged in conflict management?
   a. Variable to measured: time spent by the GI team leader (Deborah Upshaw) engaged in conflict management.
   b. Instrumentation: Self-reported log of time spent per day engaged in conflict resolution, kept over the duration of the intervention.
   c. Analysis procedures: Descriptive statistics, graph of log's entries.

D. After presentation of the Opposite Strengths material, do members of the GI team spend less time engaged in conflict management?
   a. Variable to be measured: time spent in conflict management by members of the GI team.
   b. Instrumentation: Self-reported logs of time spent per day engaged in conflict resolution, kept over the duration of the intervention.
   c. Analysis procedures: Descriptive statistics, graph of log's entries, dependent samples t-test of time spent in conflict management pre- and post-intervention.

E. Are patient satisfaction scores higher as a result of the Opposite Strengths seminar?
   a. Variable to be measured: patient satisfaction.
   b. Instrumentation: Patient satisfaction survey administered by BUMC staff and analyzed by Press Ganey Associates, Inc.
   c. Analysis procedures: The evaluation team will select specific questions from the patient satisfaction survey to represent constructs on which the

Opposite Strengths seminar will have a positive influence. The evaluation staff will be given access to summary statistics on these questions. The analysis itself will include these summary statistics, as well as visual displays of response trends both pre- and post-intervention. If the nature of the summary statistics are conducive to further analysis, dependent samples t-test will be conducted on patient satisfaction scores pre- and post-intervention.

F. Does the Opposite Strengths seminar increase the hospital's ability to retain key personnel?
    a. Variable to be measured: retention rate.
    b. Instrumentation: Number of key personnel leaving BUMC within a specified timeframe, presumably provided by BUMC's Human Resources department.
    c. Analysis procedures: summary statistics both pre- and post- intervention, t-test for independent samples, visual display of retention rate pre- and post-intervention.

G. How much money does BUMC save on new employee training as a result of the Opposite Strengths seminar?
    a. Variable to be measured: Long-term savings.
    b. Instrumentation: balance sheets regarding the cost of new employee training and the cost of the Opposite Strengths seminar, retention rates.
    c. Analysis procedures: Money saved as a higher retention rates result in fewer resources spent on new employee training will be compared to the cost of the Opposite Strengths seminar.

H. How effective is the Opposite Strengths seminar?
    a. Variable to be measured: patient satisfaction.
    b. Instrumentation: Overall patient satisfaction scores from the Press Ganey questionnaire.
    c. Analysis procedures: nonlinear multilevel mediation model.

References

Alkin, M. C. (1972). Evaluation theory development. In C. H. Weiss (Ed.), *Evaluating Action Programs: Readings in Social Action and Education*. Boston: Allyn and Bacon, Inc.

Arojan, L. A. (1944). The probability function of the product of two normally distributed variables. *Annals of Mathematical Statistics, 18*, 265-271.

Barcikowski, R. S. (1981). Statistical power with group mean as the unit of analysis. *Journal of Education Statistics, 6*, 267-285.

Baron, R. M., & Kenny, D. A. (1986). The moderator-mediator variable distinction in social psychological research: conceptual, strategic, and statistical considerations. *Journal of Personality and Social Psychology, 51*, 1173-1182.

Bauer, D. J., Preacher, K. J., & Gil, K. M. (2006). Conceptualizing and testing random indirect effects and moderated mediation in multilevel models: new procedures and recommendations. *Psychological Methods, 11*(2), 142-163.

Bollen, K. A., & Stine, R. (1990). Direct and indirect effects: classical and bootstrap estimates of variability. *Sociological Methodology, 20*, 115-140.

Borich, G. and Jemelka, R. (1981). Definitions of program evaluation and their relation to instructional design*. Educational Technology, August,* 31-38.

Bruner, J. S. (1957). Going beyond the information given. In H. Gruber, G. Terrell, & M. Wertheimer (Eds.), *Contemporary approaches to cognition* (pp. 258-290). Cambridge, MA: Harvard University Press.

Collins, L. M., Graham, J. W., & Flaherty, B. P. (1998). An alternative framework for defining mediation. *Multivariate Behavioral Research, 33*, 95-312.

Cheung, M. W.-L. (2007). Comparison of approaches to constructing confidence intervals for mediating effects using structural equation models. *Structural Equation Modeling,14(2)*, 227-246.

Craig, C. C. (1936). On the frequency function of xy. *Annals of Mathematical Statistics, 7*, 1-15.

Debreu, G. (1959). *Theory of Value: An Axiomatic Analysis of Economic Equilibrium.* New Haven, Conn.: Yale University Press.

Frazier, P. A., Tix, A. P., & Barron, K. E. (2004). Testing moderator and mediator effects in counseling psychology research. *Journal of Counseling Psychology, 51*, 115-134.

Goodman, L. A. (1960). On the exact variance of products. *Journal of the American Statistical Association, 55*, 708-713.

Hanushek, E. A., & Jackson, J. E. (1977). *Statistical Methods for Social Scientists.* New York: Academic Press.

Hayes, A. F., & Preacher, K. J. (2010). Quantifying and testing indirect effects in simple mediation models when the constituent paths are nonlinear. *Multivariate Behavioral Statistics, 45*, 627-660.

Homer, D. W., & Lemeshow, S. (2000). *Applied Logistic Regression. 2nd Ed.* New York: John Wiley and Sons.

Hoogland, J.J., & Boomsma, A. (1998). Robustness studies in covariance structure modeling: An overview and a meta-analysis. *Sociological Methods & Research, 26*, 329–367.

Howell, D. S. (2002). *Statistical Methods for Psychology. 5th Ed.* Belmont, CA: Wadsworth.

Jasti, S., Dudley, W. N., & Goldwater, E. (2008). SAS macros for testing statistical mediation in data with binary mediators or outcomes. *Nursing Research, 57*(2), 118-122.

Judd, C. M., & Kenny, D. A. (1981). Process analysis: estimating mediation in treatment evaluations. *Evaluation Review, 5*, 602-619.

Johnson, N., & Kotz, S. (1995). *Continuous Univariate Distributions (Volume 1). 2nd Ed.* New York: Wiley.

Kahneman, D., & Tversky, A. (1979).Prospect theory: An analysis of decision under risk. *Econometrica, 47*, 263-291.

Kenny, D. A., Kashy, D. A., & Bolger, N. (1998). Data analysis in social psychology. In D. Gilbert, S. T. Fiske, & G. Lindzey (Eds.). *The handbook of social psychology* (4th ed., Vol. 1, pp. 223-265). New York: McGraw-Hill.

Kenny, D. A., Korchmaros, J. D., & Bolger, N. (2003). Lower level mediation in multilevel models. *Psychological Methods, 8*(2), 115-128.

Klein, R. B. (2005). *Principles and Practice of Structural Equation Modeling*. New York: The Guilford Press.

Knobloch,, L. K. (2007). Perceptions of turmoil within courtship: Associations with intimacy, relational uncertainty, and interference from partners. *Personal Relationships, 14*, 91-112.

Kreft, I. G. G. (1996). *Are multilevel techniques necessary? An overview, including simulation studies.* Retrieved December 5, 2010, from http://www.calstatela.edu/faculty/ikreft/quarterly/quarterly.html.

Krull, J. L., & MacKinnon, D. P. (1999). Multilevel modeling in group-based intervention studies. *Evaluation Review, 23*, 418-444.

Krull, J. L., & MacKinnon, D. P. (2001). Multilevel modeling of individual and group level mediated effects. *Multivariate Behavioral Research, 36*(2), 249-277.

Li, Y., Schneider, J. A., & Bennett, D. A. (2007). Estimation of the mediation effect with a binary mediator. *Statistics in Medicine, 26*, 3398-3414.

Lomnicki, Z. A. (1967). On the distribution of products of random variables. *Journal of the Royal Statistical Society, 29*, 513-524.

MacKinnon, D. P., & Dwyer, J. H. (1993). Estimating mediated effects in prevention studies. *Evaluation Review, 17*(2), 144-158.

MacKinnon, D. P., Fritz, M. S., Williams, J., & Lockwood, C. M. (2007a). Distribution of the product confidence limits for the indirect effect: Program PRODCLIN. *Behavior Research Methods, 39*(3), 384-389.

MacKinnon, D. P., Lockwood, C. M., Brown, C. H., Wang, W., & Hoffman, J. M. (2007b). The intermediate endpoint effect in logistic and probit regression. *Clinical Trials, 4*(5), 499-513.

MacKinnon, D. P., Lockwood, J. L., Hoffman, J. M., West, S. G., & Sheets, V. (2002). A comparison of methods to test mediation and other intervening variable effects. *Psychological Methods, 7*(1), 83-104.

MacKinnon, D. P., Lockwood, C. M., & Williams, J. (2004). Confidence Limits for the indirect effect: Distribution of the product and resampling methods. *Multivariate Behavioral Research, 39*(1), 99-128.

MacKinnon, D. P., Krull, J. L., & Lockwood, C. M. (2000). Equivalence of the mediation, confounding and suppression effects. *Prevention Science, 1*, 173-181.

MacKinnon, D. P. (2008). *Introduction to Statistical Mediation Analysis*. New York: Lawrence Erlbaum Associates.

MacKinnon, D. P., Warsi, G., & Dwyer, J. H. (1995). A simulation study of mediated effect measures. *Multivariate Behavioral Research, 30*, 41-62.

Moerbeek, M. (2004). The consequence of ignoring a level of nesting in multilevel analysis. *Multivariate Behavioral Research, 39*(1), 129-149.

Moulton, B. R. (1986). Random group effects and the precision of regression estimates. *Journal of Econometrics, 32*, 385-397.

Muthén, B. O., & Satorra, A. (1995). Complex sample data in structural equation modeling. *Sociological Methodology, 25*, 267-316.

Pituch, K. A., & Miller, J. W. (1999). Strengthening multisite educational interventions: An illustration with multilevel modeling. *Educational Research and Evaluation, 5*, 62-75.

Pituch, K. A., Murphy, D. L., & Tate, R. L. (2010). Three-level models for indirect effects in school- and class-randomized experiments in education. *The Journal of Experimental Education, 78*, 60-95.

Pituch, K. A., Stapleton, L. M., & Kang, J. Y. (2006). A comparison of single sample and bootstrap methods to assess mediation in cluster randomized trials. *Multivariate Behavioral Research, 41*(3), 367-400.

Pituch, K. A., & Stapleton, L. M. (2008). The performance of methods to test upper-level mediation in the presence of nonnormal data. *Multivariate Behavioral Research, 43*, 237-267.

Pituch, K. A., Whittaker, T. A., & Stapleton, L. M. (2005). A comparison of methods to test for mediation in multisite experiments. *Multivariate Behavioral Research, 40*(1), 1-23.

Plewis, I., & Hurry, J. (1998). A multilevel perspective on the design and analysis of intervention studies. *Educational Research and Evaluation, 4*, 13-26.

Preacher, K. J., & Hayes, A. F. (2008). Asymptotic and resampling strategies for assessing and comparing indirect effects in multiple mediator models. *Behavior Research Methods, 40*(3), 879-891.

Raudenbush, S. W., & Bryk, A. S. (2002). *Hierarchical Linear Models: Applications and Data Analysis Methods. 2nd Ed.* Newbury Park, CA: Sage.

Robertson, T. B. (1909). A biochemical conception of memory and sensation. *The Monist, 19*, 367-386.

SAS Institute (2009). *User's Guide: Statistics, Version 9.2. 2nd Ed.* Cary, NC: Author.

Scariano, S. M., & Davenport, J. M. (1987). The effects of violations of independence assumptions in one-way ANOVA. *The American Statistician, 41*, 123-129.

Scott, A. J., & Holt, D. (1982). The effect of two-stage sampling on ordinary least squares methods. *Journal of the American Statistical Association, 77*, 848-854.

Scriven, M. (1974). The concept of evaluation. In M. Apple, M Subkoviak, and H. Lufler, Jr. (Eds.), *Educational Evaluation: Analysis and Responsibility.* Berkeley, CA: McCutchan.

Shrout, P. E., & Bolger, N. (2002). Mediation in experimental and nonexperimental studies: New procedures and recommendations. *Psychological Methods, 7*(4), 422-445.

Singer, J. D., & Willett, J. B. (2003). *Applied Longitudinal Data Analysis: Modeling Change and Event Occurrence.* New York: Oxford University Press.

Sobel, M. E. (1982). Asymptotic confidence intervals for indirect effects in structural equation models. In S. Leinhardt (Ed.), *Sociological methodology* (pp. 290-312). San Francisco: Jossey-Bass.

Sobel, M. E. (1986). Some new results on indirect effects and their standard errors in covariance structure models. In N. Tuma (Ed.), *Sociological methodology* (pp. 159-186). Washington, DC: American Sociological Association.

Spencer, S. J., Zanna, M. P., & Fong, G. T. (2005). Establishing a causal chain: why experiments are often more effective than meditational analyses in examining psychological processes. *Journal of Personality and Social Psychology, 89*, 845-851.

Springer, M. D., & Thompson, W. E. (1966). The distribution of independent random variables. *SIAM Journal on Applied Mathematics, 14*, 511-526.

Stolzenberg, R. M. (1980). The measurement and decomposition of causal effects in nonlinear and nonadditive models. *Sociological Methodology, 11*, 459-488.

Stone, C. A., & Sobel, M. E. (1990). The robustness of estimates of total indirect effects in covariance structure models estimated by maximum likelihood. *Psychometrika, 55*, 337-352.

Stufflebeam, D. L., Foley, W. J., Gephart, W. J., Guba, E. G., Hammond, H. D., Marriman, H. O., & Provus, M. M. (1971). *Educational Evaluation and Decision Making.* Itsca, Ill: Peacock.

Thomas, J. W., & Thomas, T. J. (2006). *The Power of Opposite Strengths: Making Relationships Work.* Austin, TX: Thomas Concept.

Walsh, J. E. (1947). Concerning the effect of the intraclass correlation on certain significance tests. *Annals of Mathematical Statistics, 18*, 88-96.

Winship, C., & Mare, R. D. (1983). Structural equations and path analysis for discrete data. *American Journal of Sociology, 89*, 54-110.

Yerkes, R. M., & Dodson, J. D. (1908). The relation of strength of stimulus to rapidity of habit formation. *Journal of Comparative Neurology and Psychology, 18*, 459-482.