David A. Kenny
October 3, 2014
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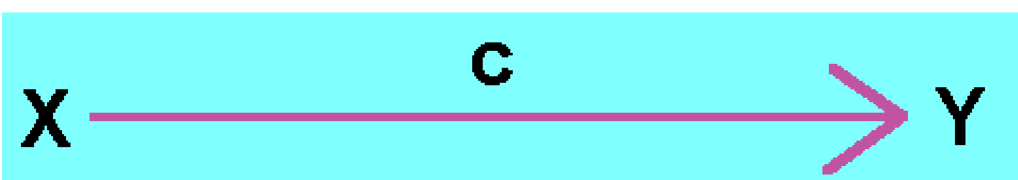
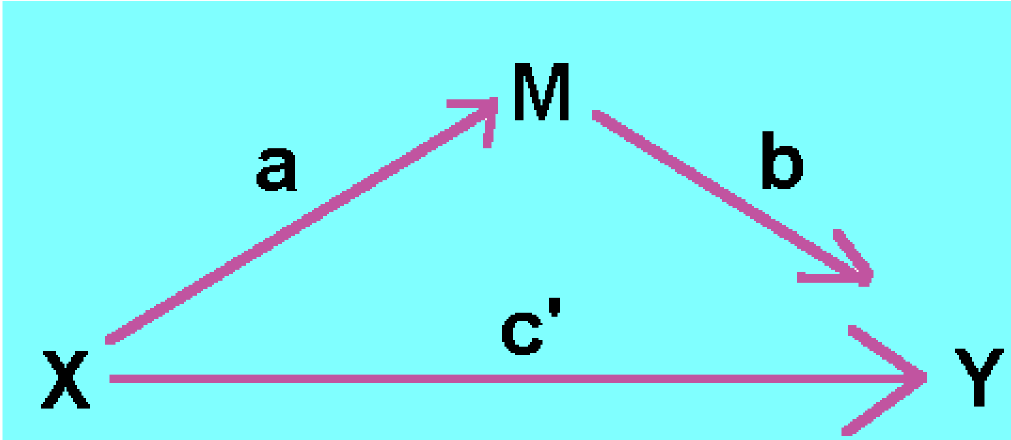
MEDIATION

Introduction

Consider a variable X that is assumed to cause another variable Y. The variable X is called the *causal variable* and the variable that it causes or Y is called the *outcome*. In diagrammatic form, the unmediated model is



Path *c* in the above model is called the *total effect*. The effect of X on Y may be mediated by a process or mediating variable M, and the variable X may still affect Y. The mediated model is



(These two diagrams are essential to the understanding of this page. Please study them carefully!) Path *c'* is called the *direct effect*. The mediator has been called an *intervening* or *process* variable. *Complete mediation* is the case in which variable X no longer affects Y after M has been controlled and so path *c'* is zero. Partial mediation is the case in which the path from X to Y is reduced in absolute size but is still different from zero when the mediator is introduced.

Note that a mediational model is a causal model. For example, the mediator is presumed to cause the outcome and not vice versa. If the presumed model is not correct, the results from the mediational analysis are likely of little value. Mediation is not defined statistically; rather statistics can be used to evaluate a presumed mediational model. The specific causal assumptions are detailed below in the section on [Specification Error](#).

There is a long history in the study of mediation (Hyman, 1955; MacCorquodale & Meehl, 1948; Wright, 1934). Mediation is a very popular topic. (This page averages over 200 visitors a day and Baron and Kenny (1986) has over 40,000 citations, according to Google Scholar, and there are three books on the topic (Hayes, 2013; Jose, 2012; MacKinnon, 2008).) There are several reasons for the intense interest in this topic: One reason for testing mediation is trying to understand the mechanism through which the causal variable affects the outcome. Mediation and moderation analyses are a key part of what has been called *process analysis,* but mediation analyses tend to be more powerful than moderation analyses. Moreover, when most causal or structural models are examined, the mediational part of the model is the most interesting part of that model.

The Four Steps

If the mediational model (see above) is correctly specified, the paths of *c, a, b,* and *c'* can be estimated by multiple regression, sometimes call ordinary least squares or OLS. In some cases, other methods of estimation (e.g., logistic regression, multilevel modeling, and structural equal modeling) must be used instead of multiple regression. Regardless of which data analytic method is used, the steps necessary for testing mediation are the same. This section describes the analyses required for testing mediational hypotheses [previously presented by Baron and Kenny (1986), Judd and Kenny (1981), and James and Brett (1984)]. See also Frazier, Tix, and Barron (2004) for a more contemporary introduction. We note that these steps are at best a starting point in a mediational analysis. More contemporary analyses focus on the indirect effect.

The Steps

Baron and Kenny (1986), Judd and Kenny (1981), and James and Brett (1984) discussed four steps in establishing mediation:

Step 1: Show that the causal variable is correlated with the outcome. Use Y as the criterion variable in a regression equation and X as a predictor (estimate and test path *c* in the above figure). This step establishes that there is an effect that may be mediated.

Step 2: Show that the causal variable is correlated with the mediator. Use M as the criterion variable in the regression equation and X as a predictor (estimate and test path *a*). This step essentially involves treating the mediator as if it were an outcome variable.

Step 3: Show that the mediator affects the outcome variable. Use Y as the criterion variable in a regression equation and X and M as predictors (estimate and test path *b*). It is not sufficient just to correlate the mediator with the outcome because the mediator and the outcome may be correlated because they are both caused by the causal variable X. Thus, the causal variable must be controlled in establishing the effect of the mediator on the outcome.

Step 4: To establish that M completely mediates the X-Y relationship, the effect of X on Y controlling for M (path *c'*) should be zero (see discussion below on significance testing). The effects in both Steps 3 and 4 are estimated in the same equation.

If all four of these steps are met, then the data are consistent with the hypothesis that variable M *completely* mediates the X-Y relationship, and if the first three steps are met but the Step 4 is not, then *partial* mediation is indicated. Meeting these steps does not, however, conclusively establish that mediation has occurred because there are other (perhaps less plausible) models that are consistent with the data. Some of these models are considered later in the Specification Error section.

James and Brett (1984) have argued that Step 3 should be modified by not controlling for the causal variable. Their rationale is that if there were complete mediation, there would be no need to control for the causal variable. However, because complete mediation does not always occur, it would seem sensible to control for X in Step 3.

Note that the steps are stated in terms of zero and nonzero coefficients, not in terms of statistical significance, as they were in Baron and Kenny (1986). Because trivially small coefficients can be statistically significant with large sample sizes and very large coefficients can be nonsignificant with small sample sizes, the steps should not be defined in terms of statistical significance. Statistical significance is informative, but other information should be part of statistical decision making. For instance, consider the case in which path *a* is large and *b* is zero. In this case, *c* = *c'* (the reason for this is shown later). It is very possible that the statistical test of *c'* is not significant (due to the collinearity between X and M), whereas *c* is statistically significant. Using just significance testing would make it appear that there is complete mediation when in fact there is no mediation at all.

Inconsistent Mediation

Following, Kenny, Kashy, and Bolger (1998), one might ask whether all of the steps have to be met for there to be mediation. Most contemporary analysts believe that the essential steps in establishing mediation are Steps 2 and 3. Certainly, Step 4 does not have to be met unless the expectation is for complete mediation. In the opinion of most though not all analysts, Step 1 is not required. (See the Power section below why the test of *c* can be low power, even if paths *a* and *b* are non-trivial.)

If *c′* were opposite in sign to *ab* something that MacKinnon, Fairchild, and Fritz (2007) refer to as *inconsistent mediation*, then it could be the case that Step 1 would not be met, but there is still mediation. In this case the mediator acts like a suppressor variable. One example of inconsistent mediation is the relationship between stress and mood as mediated by coping. Presumably, the direct effect is negative: more stress, the worse the mood. However, likely the effect of stress on coping is positive (more stress, more coping) and the effect of coping on mood is positive (more coping, better mood), making the indirect effect positive. The total effect of stress on mood then is likely to be very small because the direct and indirect effects will tend to cancel each other out. Note too that with inconsistent mediation that typically the direct effect is even larger than the total effect.

The Indirect Effect

The amount of mediation is called the *indirect effect*. Note that the

total effect = direct effect + indirect effect

or using symbols

$$c = c' + ab$$

Note also that the indirect effect equals the reduction of the effect of the causal variable on the outcome or $ab = c - c'$. In contemporary mediational analyses, the indirect effect or *ab* is the measure of the amount of mediation.

The equation of $c = c' + ab$ exactly holds when a) multiple regression (or structural equation modeling without latent variables) is used, b) the same cases are used in all the analyses, c) and the same covariates are in all the equations. However, the two are only approximately equal for multilevel models, logistic analysis and structural equation modeling with latent variables. For such models, it is probably inadvisable to compute *c* from Step 1, but rather *c* or the total effect should be inferred to be $c' + ab$ and not directly computed.

Note also that the amount of reduction in the effect of X on Y due to M is not equivalent to either the change in variance explained or the change in an inferential statistic such as *F* or a *p* value. It is possible for the *F* from the causal variable to the outcome to decrease dramatically even when the mediator has no effect on the outcome! It is also not equivalent to a change in partial correlations. The way to measure mediation is the indirect effect.

Another measure of mediation is the proportion of the effect that is mediated, or the indirect effect divided by the total effect or ab/c or equivalently $1 - c'/c$. Such a measure though theoretically informative is very unstable and should not be computed if

c is small. Note that this measure can be greater than one or even negative when there is inconsistent mediation. I would advise only computing this measure if standardized c is at least ±.2. The measure can be informative, especially when c' is not statistically significant. See the example in Kenny et al. (1998) where c' is not statistically significant but only 56% of c is explained. One rule of thumb is that if one wants to claim complete mediation ab/c should be at least .80.

Most often the indirect effect is computed directly as the product of a and b. Below are discussed three different ways to test the product of the two coefficients. Recently Imai, Keele, and Tingly (2010) have re-proposed the use of $c - c'$ as the measure of the indirect effect. They make the claim that difference in coefficients is more robust to certain forms of specification error. It is unclear at this point if the difference in coefficients approach will replace the product in coefficients approach. It is also noted here that the Causal Inference Approach (Pearl, 2011) has developed a very general approach to measuring the indirect effect.

Below are described four tests of the indirect effect or ab. Read carefully as some of the tests have key drawback that should be noted. One key issue concerns whether paths a and b are correlated; i.e., if path a is over-estimated, is path b also over-estimated? Paths a and b are uncorrelated when multiple regression is used to estimate them, but are not for most other methods. The different tests make different assumptions about this correlation.

Joint Significance of Paths *a* and *b*

If Step 2 (the test of a) and Step 3 (the test of b) are met, it follows that the indirect effect is likely nonzero. Thus, one way to test the null hypothesis that $ab = 0$ is to test that both paths a and b are zero (Steps 2 and 3). This simple approach, called the *joint test of significance*, appears to work rather well (Fritz & MacKinnon, 2007), but is rarely used as the definitive test of the indirect effect. (Joint significance presumes that a and b are uncorrelated.) However, Fritz, Taylor, and MacKinnon (2012) have strongly urged that researchers use this test in conjunction with other tests. Also recent simulation results by Hayes and Scharkow (2013) have shown that this test performs about as well as a bootstrap test. Moreover, this test is often used to determine the power of the test of the indirect effect. The major drawback with this approach is that it does not provide a confidence interval for the indirect effect.

Sobel Test

A test, first proposed by Sobel (1982), was initially often used. Some sources refer to this test as the *delta method*. It requires the standard error of a or s_a (which equals a/t_a where t_a is the t test of coefficient a) and the standard error of b or s_b. The Sobel test provides an approximate estimate of the standard error of ab which equals to the square root of

$$b^2 s_a^2 + a^2 s_b^2$$

Other approximate estimates of the standard error of ab standard errors have been proposed, but the Sobel test is by far the most commonly used estimate. (As discussed below, bootstrapping has replaced the more conservative Sobel test.) The test of the

indirect effect is given by dividing *ab* by the square root of the above variance and treating the ratio as a *Z* test (i.e., larger than 1.96 in absolute value is significant at the .05 level). Kristopher J. Preacher and Geoffrey J. Leonardelli have an excellent webpage that can help you calculate these test (go to the Sobel test). Measures and tests of indirect effects are also available within many structural equation modeling programs. These programs appear to use the Sobel formula.

The derivation of the Sobel standard error presumes that the estimates of paths *a* and *b* are independent, something that is true when the tests are from multiple regression but not true when other tests are used (e.g., logistic regression, structural equation modeling, and multilevel modeling). In such cases, the researcher ideally provides evidence for approximate independence. The Sobel test can be conducted using the standardized or unstandardized coefficients. Care must be taken to use the appropriate standard errors if standardized coefficients are used.

The Sobel test is very conservative (MacKinnon, Warsi, & Dwyer, 1995), and so it has very low power. The main reason for the test being conservative is that the sampling distribution of *ab* is highly skewed. If *ab* is positive, there is positive skew with many small estimates of *ab* and few very large ones. Because the Sobel test uses a normal approximation which presumes a symmetric distribution, it falsely presumes symmetry which leads to a conservative test.

Bootstrapping

An increasingly popular method of testing the indirect effect is bootstrapping (Bollen & Stine, 1990; Shrout & Bolger, 2002). Bootstrapping is a non-parametric method based on resampling with replacement which is done many times, e.g., 5000 times. From each of these samples the indirect effect is computed and a sampling distribution can be empirically generated. Because the mean of the bootstrapped distribution will not exactly equal the indirect effect a correction for bias is usually made. With the distribution, a confidence interval, a *p* value, or a standard error can be determined. Very typically a confidence interval is computed and it is checked to determine if zero is in the interval. If zero is not in the interval, then the researcher can be confident that the indirect effect is different from zero. Also a *p* value can determined, but standard errors suffer the same problem as the Sobel standard errors and are not recommended. (Bootstrapping does require the assumption that *a* and *b* are uncorrelated.

Recently, Fritz, Taylor, and MacKinnon (2012) have raised concerns that bias-corrected bootstrapping test is too liberal with alpha being around .07. Actually not doing the bias correction seems to improve the Type I error rate. Hayes and Scharkow (2013) recommended using the bias corrected bootstrap if power is the major concern, but if Type I error rate is the major concern, then the percentile bootstrap should be used.

Hayes and Preacher have written SPSS and SAS macros that can be downloaded for tests of indirect effects (click here to get the Hayes and Preacher macro). Also Mplus and Amos can be used to bootstrap (click here for an Amos tutorial). If one has more than one mediator and is using Amos, one should consult for details Macho and Ledermann (2011) on how to compute separate confidence intervals for each indirect effect.

Monte Carlo Method

MacKinnon, Lockwood, and Williams (2004) have proposed a computer simulation test of the indirect effect. One starts with *a, b* s_a, and s_b (not the raw data). Using this information, random normal variables for *a* and *b* are generated to create a distribution of *ab* values. With these values, confidence intervals and a *p* value can be created. Selig and Preacher have created a helpful [website] that will perform the necessary calculations using R. The test is useful in situations in which there is not easy to bootstrap (e.g., the raw data are unavailable). If *a* and *b* are correlated, then that correlation can be included in the Monte Carlo simulation of *a* and *b* values.

Power

Effect Size of the Indirect Effect and the Computation of Power

The indirect effect is the product of two effects. One simple way, but not the only way, to determine the effect size is to measure the product of the two effects, each turned into an effect size. The standard effect size for paths *a* and *b* is a partial correlation; that is, for path *a*, it is the correlation between X and M, controlling for the covariates and any other Xs and for path *b*, it is the correlation between M and Y, controlling for covariates and other Ms and Xs. One possible effect size for the indirect effect would be the product of the two partial correlations. (Preacher and Kelley (2011) discuss a similar measure of effect size which they refer to as the *completely standardized indirect effect*, which uses betas, not partial correlations.)

There are two different strategies for determining small, medium, and large effect sizes. (Any designation of small, medium, or larger is fundamentally arbitrary and depends on the particular application.) First, following Shrout and Bolger (2002), the usual Cohen (1988) standards of .1 for small, .3 for medium, and .5 for large could be used. Alternatively and I think more appropriately because an indirect effect is a product of two effects, these values should be squared or *rr*. Thus, a small effect size would be .01, medium would .09, and large would be .25. Note that if X is a dichotomy, it makes sense to replace the correlation for path *a* with Cohen's *d*. In this case the effect size would be *dr* and a small effect size would be .02, medium would .15, and large would be .40.

One strategy to compute the power of the test of the indirect effect is to use the joint test of significance . Thus, one computes the power of test of paths *a* and *b* and then multiply their power to obtain the power of the test of the indirect effect. (One can download the program [PowMed.R] and view the webinars on [power] and the [PowMedR] program.) Alternatively and more generally, one could use a structural equation modeling to run a simulation.

Distal and Proximal Mediation

To demonstrate mediation both paths *a* and *b* need to be present. Generally, the maximum size of the product *ab* equals a value near *c*, and so as path *a* increases, path *b*

must decrease and vice versa. Hoyle and Kenny (1999) define a proximal mediator as *a* being greater than *b* (all variables standardized) and a distal mediator as *b* being greater than *a*.

A mediator can be too close in time or in the process to the causal variable and so path *a* would be relatively large and path *b* relatively small. An example of a proximal mediator is a manipulation check. The use of a very proximal mediator creates strong multicollinearity which lowers power as is discussed in the next section.

Alternatively, the mediator can be chosen too close to the outcome and with a distal mediator path *b* is large and path a is small. Ideally in terms of power, standardized *a* and *b* should be comparable in size. However, work by Hoyle and Kenny (1999) shows that the power of the test of *ab* is maximal when *b* is somewhat larger than *a* in absolute value. So slightly distal mediators result in somewhat greater power than proximal mediators. Note that if there is proximal mediation (*a* > *b*), sometimes power actually declines as *a* (and so *ab*) increases.

Multicollinearity

If M is a successful mediator, it is necessarily correlated with X due to path a. This correlation, called collinearity, affects the precision of the estimates of the last regression equation. If X were to explain all of the variance in M, then there would be no unique variance in M to explain Y. Given that path *a* is nonzero, the power of the tests of the coefficients *b* and *c'* is lowered. The effective sample size for the tests of coefficients *b* and *c'* is approximately $N(1 - r^2)$ where *N* is the total sample size and *r* is the correlation between the causal variable and the mediator, which is equal to standardized *a*. So if M is *a* strong mediator (path *a* is large), to achieve equivalent power, the sample size to test coefficients *b* and *c'* would have to be larger than what it would be if M were a weak mediator. Multicollinearity is to be expected in *a* mediational analysis and it cannot be avoided.

Low Power for Steps 1 and 4

As described by Kenny and Judd (2014), as well as others, the tests of c and *c'* have relatively low power, especially in comparison to the indirect effect. It can easily happen, that *ab* can be statistically significant but *c* is not. For instance, if *a* = *b* = .4 and *c'* = 0, making *c* = .16, and *N* = 100, the power of the test of path *a* is .99, the power of the test of path *b* is .97 which makes the power of *ab* equal to about .96, but the power of the test that *c* is only .36. Surprisingly, it is very easy to have complete mediation, a statistically significant indirect effect, but no statistical evidence that X causes Y.

Because of the low power in the test of *c'*, one needs to be very careful about any claim of complete mediation based on the non-significance of *c'*. In fact, several sources (e.g., Hayes, 2013) have argued that one should never make any claim of complete or partial mediation. It seems more sensible to be careful about claims of complete mediation. One idea is establish first that is sufficient power to test for partial mediation: The power of the test of *c'*. Also if the sample size is very large, then finding a significant value for *c'* and so "partial" mediation is not very informative. More informative, in the case of large *N,* is knowing the proportion of the total effect that is mediated or *ab*/*c*.

Specification Error

Mediation is a hypothesis about a causal network. (See Kraemer, Wilson, Fairburn, and Agras (2002) who attempt to define mediation without making causal assumptions.) The conclusions from a mediation analysis are valid only if the causal assumptions are valid (Judd & Kenny, 2010). In this section, the three major assumptions of mediation are discussed. Mediation analysis also makes all of the standard assumptions of the general linear model (i.e., linearity, normality, homogeneity of error variance, and independence of errors). It is strongly advised to check these assumptions before conducting a mediational analysis. Clustering effects are discussed in the Extensions section. What follows are sufficient conditions. That is, if the assumptions are met, the mediational model is identified. However, there are sometimes special cases in which an assumption can be violated, yet the model is identified (Pearl, 2013).

Reverse Causal Effects

The mediator may be caused by the outcome variable (Y would cause M in the above diagram), what is commonly called a *feedback model*. When the causal variable is a manipulated variable, it cannot be caused by either the mediator or the outcome. But because both the mediator and the outcome variables are not manipulated variables, they may cause each other.

Often it is advisable to interchange the mediator and the outcome variable and have the outcome "cause" the mediator. If the results look similar to the specified mediational pattern (i.e., the *c'* and *b* are about the same in the two models), one would be less confident in the specified model. However, it should be realized that the direction of causation between M and Y cannot be determined by statistical analyses.

Sometimes reverse causal effects can be ruled out theoretically. That is, a causal effect in one direction does not make sense. Design considerations may also weaken the plausibility of reverse causation. Ideally, the mediator should be measured temporally before the outcome variable.

If it can be assumed that *c'* is zero, then reverse causal effects can be estimated. That is, if it can be assumed that there is complete mediation (X does not directly cause Y and so *c'* is zero), the mediator may cause the outcome and the outcome may cause the mediator and the model can be estimated using instrumental variable estimation.

Smith (1982) has developed another method for the estimation of reverse causal effects. Both the mediator and the outcome variables are treated as outcome variables, and they each may mediate the effect of the other. To be able to employ the Smith approach, for both the mediator and the outcome, there must be a different variable that is known to cause each of them but not the other. So a variable must be found that is known to cause the mediator but not the outcome and another variable that is known to cause the outcome but not the mediator. These variables are called instrumental variables. For such a model, mediation can be estimated and tested with feedback.

Measurement Error in the Mediator

If the mediator is measured with less than perfect reliability, then the effects (*b* and *c'*) are likely biased. The effect of the mediator on the outcome (path *b*) is likely underestimated and the effect of the causal variable on the outcome (path *c'*) is likely over-estimated if *ab* is positive (which is typical). The over-estimation of *c'* is exacerbated to the extent to which path *a* is large. In a parallel fashion, if X is measured with less than perfect reliability, then the effects (*b* and *c'*) are likely biased. The effect of the M on Y mediator on the outcome (path *b*) is likely over-estimated and the effect of the causal variable on the outcome (path *c'*) is likely under-estimated. Moreover, measurement error in X attenuates the estimate of path *a* and *c*. Measurement error in Y does not bias unstandardized estimates, but it does bias standardized estimates, attenuating them.

To remove the biasing effect of measurement error, multiple indicators of the variable can be used to tap a latent variable. Alternatively for M, instrumental variable estimation can be used, but as before, it must be assumed that *c'* is zero. Also possible is to fix the error variance at the value or one minus the reliability quantity times the variance of the measure. If none of these approaches is used, the researcher needs to demonstrate that the reliability of the mediator is very high so that the bias is fairly minimal.

Omitted Variables

In this case, there is a variable that causes both variables in the equation. (These variables are called *confounders* in some literatures and the assumption can be stated more formally and generally, see below.) For example, there is a variable that causes both the mediator and the outcome. This is the most likely source of specification error and is difficult to find solutions to circumvent it. (Unfortunately, this key assumption is not directly discussed in Baron and Kenny (1986), but is discussed in Judd and Kenny (1981).) Although there has been some work on the omitted variable problem, the only complete solution is to specify and measure such variables and control for their effects.

Note that if the causal variable, X, is randomized, then omitted variables do not bias the estimates of *a* and *c*. However, in this case, paths b and *c'* might be biased if there is an omitted variable that causes both M and Y. Assuming that this omitted variable has paths in the same direction on M and Y and that *ab* is positive, then path b is over-estimated and path *c'* is underestimated. In this case, if the true *c'* were zero, then it would appear that there was inconsistent mediation when in fact there is complete mediation.

Sometimes the source of correlation between the mediator and the outcome is a common method effect. For instance, the measuring scale of the two variables is the same. Ideally, efforts should be made to ensure that the two variables do not share method effects (e.g., both are self-reports from the same person). A latent variable analysis might be used to remove the effects of correlated measurement error.

To remove the biasing effect of omitted variables, the classical strategy is measure and control for them. Alternatively, an instrumental variable estimation can be used. One possibility is that *c'* is zero, which makes X the instrumental variable. Alternatively, *c'* is estimated and another variable or variables are used as instrument(s). The instrument must cause M but not Y. Note that M serves as a perfect mediator of the instrument to Y relationship. Instruments must be chosen on the basis of theory not

empirical relationships. Alternative strategies for dealing with omitted variables are being developed within the Causal Inference Approach.

Brewer, Campbell, and Crano (1970) argued that in some cases when X is not manipulated, it might be that single unmeasured variable can explain the covariation between variables. In fact, unless there is inconsistent mediation, a single latent variable can always explain the covariation between X, M, and Y.

As stated above, the absence of omitted variables is not a necessary condition (Pearl, 2013). For instance, consider the case that $M \leftarrow Z_1 \leftarrow Z_2 \rightarrow Y$ but Z_1 and not Z_2 is measured and included in the model. The mediational paths would be correctly estimated because Z_1 can be used as a "proxy" for Z_2.

The Combined Effects of Measurement Error and an Omitted Variable

Fritz, Kenny, and MacKinnon (2014) point out that in the case in which X is manipulated and M has measurement error and there is a confounder for M and Y, it can happen that the two biases can to some degree offset each other. That is, assuming *a* and *b* being positive, measurement error in M in this case leads to under-estimation of *b* and over-estimation of *c′* whereas typically (but not always) a MY confounder leads to over-estimation of b and under-estimation of *c′*. See the paper for details and an example.

The Mediator as also a Moderator

Baron and Kenny (1986) and Kraemer et al. (2002) discuss the possibility that M might interact with X to cause Y. Baron and Kenny (1986) refer to this as M being both a mediator and a moderator and Kraemer et al. (2002) as a form of mediation. The X with M interaction should be estimated and tested and added to the model if present. Judd and Kenny (1981) discuss how the meaning of path *b* changes when this interaction is present. Also the Causal Inference Approach begin with the assumption that X and M interact.

Design Considerations

One of the best ways to increase the internal validity of mediational analysis is by the design of study. Key considerations are randomizing X (i.e., randomly assigning units to levels of X), the timing of measurement of M and Y, and obtaining prior values of M and Y. By randomizing X, it is known that both M and Y do not cause X. By measuring M after X, and Y after M, it is known that M does not cause X and that Y does not cause X or M. Finally by obtaining prior measures of M and Y and control for them, we can reduce and perhaps eliminate the effects of omitted variables. The reader should consult Cole and Maxwell (2003) about the difficulties of estimating mediational effect using a cross-sectional design. Also as mentioned earlier, it is possible to randomize X, M, and Y (Smith, 1982).

Sensitivity Analyses

As discussed above, it is usually assumed for mediation that there is perfect reliability for X and M, no omitted variables for the X to M, X to Y, and M to Y relationships, and no causal effects from Y to X and M and from M to X. It is possible to determine what would happen to the mediational paths one or more of these assumptions is violated by conducting sensitivity analyses. For instance, one might find that allowing for measurement error in M (reliability of .8) that path *b* would be larger by 0.15 and that *c'* would be less by 0.10. Alternatively, one might determine what was the value of reliability that would make *c'* equal zero.

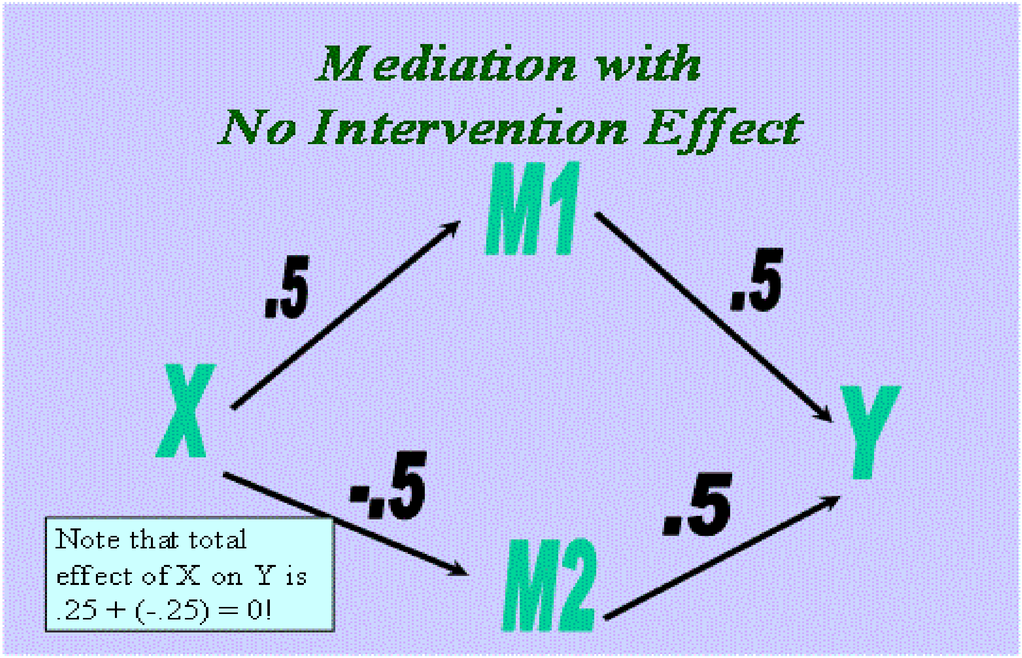
One way to conduct a sensitivity analysis is to estimate the mediational model using Structural Equation Modeling. One fixes the additional parameter to the value of interest. For example, an omitted variable is added that has a moderate effect on M and Y. One can then use the estimates from this analysis in the sensitivity analysis. See the Sensitivity Analyses webinar (small charge) that I have created which has more details. A convincing mediation analyses should be accompanied by a sensitivity analysis.

Additional Variables

Rarely in mediation are there just the three variables of X, M, and Y. Discussed in this section is how to handle additional variables in a mediational model.

Multiple Mediators

If there are multiple mediators, they can be tested simultaneously or separately. The advantage of doing them simultaneously is that one learns if the mediation is independent of the effect of the other mediators. One should make sure that the different mediators are conceptually distinct and not too highly correlated. (Kenny et al. (1998) consider an example with two mediators.) There is an interesting case of two mediators (see below) in which *ab* is opposite sign. The sum of indirect effects for M1 and M2 would be zero. It might then be possible that *c* is near zero, because there are two indirect effects that work in the opposite direction. In this case "no effect" would be mediated. I suggest that the case in which there are two indirect effects of the same effect that are approximately equal in size but opposite in sign be called *opposing mediation*.



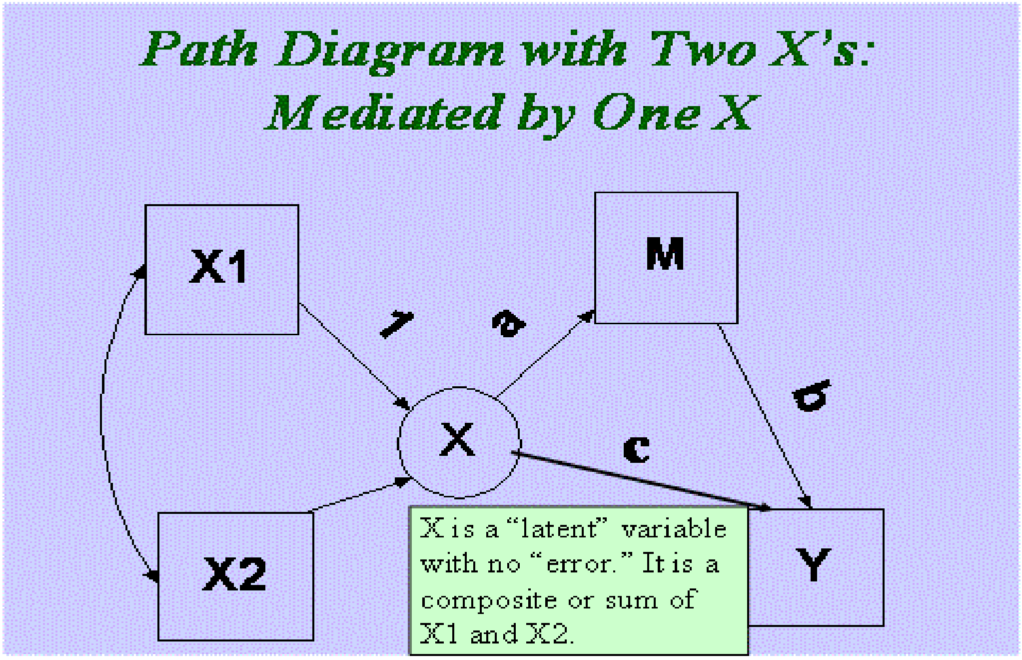
The Hayes and Preacher bootstrapping macro can be used to test hypotheses about the linear combinations of indirect effects: For example, it can be if they are equal or if they sum to zero.

Multiple Outcomes

If there are multiple outcomes, they can be tested simultaneously or separately. If tested simultaneously, the entire model can be estimated by structural equation modeling. One might want to consider combining the multiple outcomes into one or more [latent variables](http://davidakenny.net/cm/mediate.htm#COMP).

Multiple Causal Variables

In this case there are multiple X variables and each has an indirect effect on Y. The Hayes and Preacher bootstrapping macro can be used to test hypotheses about the linear combinations of indirect effects: For example, are they equal? Do they sum to zero? One can alternatively treat the multiple X variables as a *formative* variable and so if a single "super variable" can be used to summarize the indirect effect. As seen below, the formative variable X "mediates" the effect of X on M and Y. The model can be tested and it has *k* - 1 degrees of freedom where *k* is the number of X variables. Thus, the degrees of freedom for the example would be 1.



Covariates

There are often variables that do not change that can cause or be correlated with the causal variable, mediator, and outcome (e.g., age, gender, and ethnicity); these variables are commonly called *covariates*. They would generally be included in the M and Y equations, A covariate would not be trimmed from one equations unless it is dropped from all of the other equation. If a covariates interacts with X or M, it would be called a moderator variable.

Extensions

Mediated Moderation and Moderated Mediation

Moderation means that the effect of a variable on an outcome is altered (i.e., moderated) by a covariate. (To read about moderation click here.) Moderation is usually captured by an interaction between the causal variable and the covariate. If this moderation is mediated, then we have the usual pattern of mediation but the X variable is an interaction and the pattern would be referred to as *mediated moderation*. All the Baron and Kenny steps would be repeated with the causal variable or X being an interaction, and the two main effects would be treated as "covariates." We could compute the total effect or the original moderation effect, the direct effect or how much moderation exists after introducing the moderator, and the indirect effect or how much of the total effect of the moderator is due to the mediator.

Sometimes, mediation can be stronger for one group (e.g., males) than for another (e.g., females), something called *moderated mediation*. There are two major different forms of moderated mediation. The effect of the causal variable on the mediator may differ as a function of the moderator (i.e., path *a* varies) or the mediator may interact with the moderator to cause the outcome (i.e., path *b* varies). It is also possible that the direct effect or *c'* might change as a function of the moderator.

Papers by Muller, Judd, and Yzerbyt (2005) and Edwards and Lambert (2007) discuss mediated moderation and moderated mediation and examples of each. Also Preacher, Rucker, and Hayes have developed a macro for estimating moderated mediation (click here).

Latent Variables

Some or all of the mediational variables might be latent variables. Estimation would be accomplished using a structural equation modeling (SEM) program (e.g., LISREL, Amos, Eqs, or MPlus). Some programs provide measures and tests of indirect effects. Also such programs are quite flexible in handling multiple mediators and outcomes. The one complication is how to handle Step 1. That is, if two models are estimated, one with the mediator and one without, the paths *c* and *c'* are not directly comparable because the factor loadings would be different. It is then inadvisable to test the relative fit of two structural models, one with the mediator and one without. Rather *c*, the total effect, can be estimated using the formula of *c'* + *ab*. Most SEM programs give this estimate.

If there are multiple mediators, Amos does not compute indirect effects for each mediator. The reader should consult Macho and Ledermann (2011) for a method that does decompose the total indirect effect into separate effects.

One advantage of a latent variable model is that correlated measurement error in X, M, and Y might be modeled. For instance, if some of the measures are self-report, their errors might be correlated.

Dichotomous Variables

If either the mediator or the outcome is a dichotomy, standard methods of estimation should not be used. (Having the causal variable or X be a dichotomy is not problematic.) If either the mediator or the outcome is a dichotomy, the analysis would likely be conducted using logistic regression. One can still use the Baron and Kenny steps. The Sobel test is problematic in that it assumes that *a* and *b* are independent, which may not be the case. The one complication is the computation of indirect effect the degree of mediation because the coefficients need to be transformed. (To read about the computation of indirect effects using logistic or probit regression click here.) With dichotomous outcomes, it is advisable to use a program like Mplus that can handle such variables.

Clustered Data

Traditional mediation analyses presume that the data are at just one level. However, sometimes the data are clustered in that persons are in classrooms or groups, or the same person is measured over time. With clustered data, multilevel modeling should be used. Estimation of mediation within multilevel models can be very complicated, especially when the mediation occurs at level one and when that mediation is allowed to be random, i.e., vary across level two units. The reader is referred to Krull and MacKinnon (1999), Kenny, Korchmaros, and Bolger (2003), and Bauer and Preacher (2006) for a discussion of this topic. Recently, Preacher, Zyphur, and Zhang (2010) have proposed that multilevel structural equation methods or MSEM can be used to estimate

these models. Ledermann, Macho, and Kenny (2011) discuss mediational models for dyadic data.

Over-Time Data

Over-time data can be treated as clustered data, but there are further complications due to temporal nature of the data. Among such issues are correlated errors, lagged effects, and the outcome causing the mediator. One might consult Bolger and Laurenceau (2013) for guidance. Also, Judd, Kenny, and McClelland, (2001) discuss a generalization of repeated measures analysis of variance test of mediation.

Causal Inference Approach to Mediation

(I want to thank Tom Loeys and Haeike Josephy who reviewed an early version of this section, Pierre-Olivier Bédard who spotted a typographical error, and especially Judea Pearl who made several very helpful suggestions. Go to Pearl's blog discussion of this section.)

A group of researchers have developed an approach that has several emphases that are different from the traditional SEM approach, the approach that is emphasized on this page. The approach is commonly called the *Causal Inference* approach, and I provide here a brief and relatively non-technical summary in which I attempt to explain the approach to those more familiar with Structural Equation Modeling. Robins and Greenland (1992) conceptualized the approach and more recent papers within this tradition are Pearl (2001; 2011) and Imai et al. (2010). Somewhat more accessible is the paper by Valeri and VanderWeele (2013). Unfortunately, SEMers know relatively little about this approach and, I believe also that Causal Inference researchers fail to appreciate the insights of SEM.

The Causal Inference Approach uses the same basic causal structure (see diagram) as the SEM approach, albeit usually with different symbols for variables and paths. The two key differences are that the relationships between variables need not be linear and the variables need not be interval. In fact, typically the variables of X, Y, and M are presumed to be binary and that X and M are presumed to interact to cause Y.

Similar to SEM, the Causal Inference approach attempts to develop a formal basis for causal inference in general and mediation in particular. Typically counterfactuals or potential outcomes are used. The potential outcome for person i on Y for whom X = 1 would be denoted as $Y_i(1)$. The potential outcome of $Y_i(0)$ can be defined even though person i did not score 0 on X. Thus, it is a potential outcome or a counterfactual. The averages of these potential outcomes across persons are denoted as E[Y(0)] and E[Y(1)]. To an SEM modeler, potential outcomes can be viewed as predicted values of a structural equation. Consider the "Step 1" structural equation:

$$Y_i = d + cX_i + e_i$$

If for individual i for whom X_i equals 1, then $Y_i(1) = d + c + e_i$ equals his or her score on Y. We can determine what the score of person i would have been had his or her score on X_i been equal to 0, i.e., the potential outcome for person i, by taking the structural

equation and setting X_i to zero to yield d + e_i. Although the term is new, potential outcomes are not really new to SEMers. They simply equal the predicted value for endogenous variable, once we fix the values of its causal variables.

The Causal Inference approach also employs [directed acyclic graphs] or DAGs, which are similar to, though not identical to, path diagrams. DAGs typically do not include disturbances but they are implicit. The curved lines of path diagrams between exogenous variables are also not drawn but are implicit.

Assumptions

Earlier, the assumptions necessary for mediation were stated using structural equation modeling terms. Within the Causal Inference approach, there are essentially the same assumptions, but they are stated somewhat differently. Note that the term *confounder* is used where earlier the term [omitted variable] was used.

Condition 1: No unmeasured confounding of the XY relationship; that is, any variable that causes both X and Y must be included in the model.

Condition 2: No unmeasured confounding of the MY relationship.

Condition 3: No unmeasured confounding of the XM relationship.

Condition 4: Variable X must not cause any confounder of the MY relationship.

Note that if Condition 2 is met, then Condition 4 must be met. However, this fourth condition is added because certain effects can be estimated without making this assumption and other effects require this assumption. Note also that these assumptions are sufficient but not necessary. That is, if these conditions are met the mediational paths are identified, but there are some special cases where mediational paths are identified even if the assumptions are violated (Pearl, 2013). For instance, consider the case that $M \leftarrow Z_1 \leftarrow Z_2 \rightarrow Y$ but Z_1 and not Z_2 is measured and included in the model. Note that Z_2 is a MY confounder and thus violates Condition 2, but it is sufficient to control for only Z_1.

The Causal Inference approach emphasizes [sensitivity analyses]: These are analyses that ask the question such as, "What would happen to the results if there was a MY confounder that had both a moderate effect on M and Y?" SEMers would benefit by considering these analyses more often.

Definitions of the Direct, Indirect, and Total Effects

Because effects involve variables not necessarily at the interval level and because interactions are allowed, the direct, indirect, and total effects need to be redefined. These effects are defined using counterfactuals, not using structural equations. Recall from above that for person i, it can be asked: What would i's score on Y be if i had scored 0 on X? That value, called the *potential outcome*, is denoted $Y_i(0)$. The population average of these potential outcomes across persons is denoted as E[Y(0)]. We can then define the effect of X on Y as

$$E[Y(1)] - E[Y(0)]$$

This looks strange to an SEMer, but it is useful to remember effects can be viewed as a difference between what the outcome would be when the causal variable differs by one unit. Consider path *c* in mediation. We can view *c* as the difference between what it would be expected that Y would equal when X was 1 and equal to 0, the difference between the two potential outcomes, E[Y(1)] - E[Y(0)].

In the Causal Inference approach, there is the Controlled Direct Effect or CDE for the mediator equal to a particular value, denoted as M (not to be confused with the variable M):

$$CDE(M) = E[Y(1,M)] - E[Y(0,M)]$$

where M is a particular value of the mediator. (Note that it is E[Y(1,M)] and not E[Y(1|M)], the expected value of Y given that X equals 1 "controlling for M." The variable M is not "fixed" or "conditioned" in this approach.) If X and X interact, the CDE(M) changes for different values of M. To obtain a single measure of the direct effect, several different suggestions have been made. Although the suggestions are different, all of these measures are called "Natural." One idea is to determine the Natural Direct Effect as follows

$$NDE = E[Y(1,M_0)] - E[Y(0,M_0)]$$

where M_0 is M(0) which is the expected value on the mediator if X were to equal 0 (i.e., the potential outcome of M given X = 0). Thus, within this approach, there needs to be a meaningful "baseline" value for X which becomes its zero value. For instance, if X is the variable experimental group versus control group, then the control group would have a score of 0. However, if X is level of self-esteem, it might be more arbitrary to define the zero value. The parallel Natural Indirect Effect is defined as

$$NIE = E[Y(1,M_1)] - E[Y(1,M_0)]$$

where M_1 is M(1) or the potential outcome for M when X equals 1. The Total Effect becomes the sum of the two:

$$TE = NIE + NDE = E[Y(1,M_1)] - E[Y(1,M_0)] = E[Y(1)] - E[Y(0)]$$

Some might benefit from Muthén's discussion of these measures of mediation effects in his paper [Applications of Causally Defined Direct and Indirect Effects in Mediation Analysis using SEM in Mplus](#)."

Note that both the CDE and the NDE would equal the regression slope or what was earlier called path *c′* if the model is linear, assumptions are met, and there is no XM interaction affecting Y, the NIE would equal *ab*, and the TE would equal *ab* + *c′*. In the case in which the specifications made by traditional mediation approach (e.g., linearity, no omitted variables, no XM interaction), the estimates would be the same.

Links

Doing a mediation analysis and output a text description of the results using SPSS.
A description of an R mediation program by Tingley, Yamamoto, and Kosuke Imai that is especially useful for non-normal variables.
A conference on mediation with links to talks.
Doing a mediation analysis and output a text description of the results using R.
To find out why computing partial correlations to test mediation is problematic.
Power analyses for a mediation analysis using R.
Go to my moderation page.
A paper I have written called "Reflections on Mediation."
Dave MacKinnon's mediation website.
Kris Preacher's papers and programs.
Andrew Hayes' papers and programs.
Mediation Facebook page.
View my webinars on mediation. (small charge is requested)
Please suggest new links!

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