

Toward Terminological, Conceptual, and Statistical Clarity in the Study of Mediators and Moderators: Examples From the Child-Clinical and Pediatric Psychology Literatures

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Numerous recent attempts to identify mediated and moderated effects in child-clinical and pediatric research on child adjustment have been characterized by terminological, conceptual, and statistical inconsistencies. To promote greater clarity, the terms *mediating* and *moderating* are defined and differentiated. Recommended statistical strategies that can be used to test for these effects are reviewed (i.e., multiple regression and structural equation modeling techniques). The distinction between mediated and indirect effects is also discussed. Examples of troublesome and appropriate uses of these terms in the child-clinical and pediatric psychology literatures are highlighted.

Despite the appearance of several useful discussions of differences between mediated and moderated effects (e.g., Aldwin, 1994; Baron & Kenny, 1986; James & Brett, 1984), there continue to be inconsistencies in the use of these terms. More specifically, several types of problems occur with some regularity: (a) vague or interchangeable use of the terms, (b) inconsistencies between terminology and the underlying conceptualization of the variables used, (c) use of data-analytic procedures that fail to test for mediated and moderated effects, and (d) a mismatch between written text and diagrammatic figures.

Frequently, terminological, conceptual, and statistical inconsistencies are all present in the same study, such as when investigators conceptualize a variable as a moderator (e.g., coping strategies are hypothesized to serve a protective or buffering function), use the term *mediator* (rather than *moderator*) to describe the impact of the variable, provide a figure where the variable is presented as a mediator (rather than as a moderator), and conduct statistical analyses that test neither mediation nor moderation. When such mismatches among terminology, theory, figures, and statistical analyses exist, findings become particularly difficult to interpret.

A lack of conceptual and statistical clarity in the study of mediated and moderated effects has become particularly prevalent in mental health literatures where investigators seek to examine factors that mediate or moderate associations between selected predictors and adjustment outcomes. In the child-clinical and pediatric psychology literatures, for example, models of predictor–adjustment relationships have become quite complex (e.g., Grych & Fincham, 1990; Thompson, Gil, Burbach,

Keith, & Kinney, 1993). Investigators working in these areas have found it necessary to invoke conceptual models that include mediated and moderated effects.

The purpose of this discussion is threefold: (a) the terms “mediator” and “moderator” are defined and differentiated, (b) statistical strategies for testing mediated and moderated effects are reviewed, and (c) examples of troublesome and appropriate uses of these terms in the child-clinical and pediatric psychology literatures are presented. Although examples have been drawn from only two literatures, the points made apply to any research area where mediated or moderated effects are of interest.

Definition of Mediated and Moderated Effects

According to Baron and Kenny, a moderator specifies the conditions under which a given effect occurs, as well as the conditions under which the direction or strength of an effect vary. They describe a moderator variable as the following:

a qualitative (e.g., sex, race, class) or quantitative . . . variable that affects the direction and/or strength of a relation between an independent or predictor variable and a dependent or criterion variable . . . a basic moderator effect can be represented as an interaction between a focal independent variable and a factor (the moderator) that specifies the appropriate conditions for its operation . . . Moderator variables are typically introduced when there is an unexpectedly weak or inconsistent relation between a predictor and a criterion variable. (Baron & Kenny, 1986, pp. 1174, 1178)

In other words, a moderator variable is one that affects the relationship between two variables, so that the nature of the impact of the predictor on the criterion varies according to the level or value of the moderator (also see Saunders, 1956; Zeideck, 1971). A moderator interacts with a predictor variable in such a way as to have an impact on the level of a dependent variable.

A mediator, on the other hand, specifies how (or the mechanism by which) a given effect occurs (Baron & Kenny, 1986; James & Brett, 1984). More specifically, Baron and Kenny (1986) describe a mediator variable as the following:

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the generative mechanism through which the focal independent variable is able to influence the dependent variable of interest . . . (and) Mediation . . . is best done in the case of a strong relation between the predictor and the criterion variable. (pp. 1173, 1178)

Stated more simply, "the independent variable causes the mediator which then causes the outcome" (Shadish & Sweeney, 1991, p. 883). Although one may argue that the relationships among independent variable, mediator, and outcome may not necessarily be "causal," the nature of the mediated relationship is such that the independent variable influences the mediator which, in turn, influences the outcome. Also critical is the prerequisite that there be a significant association between the independent variable and the dependent variable before testing for a mediated effect.

Mediators and moderators can also be differentiated diagrammatically (see Figure 1; see also Baron & Kenny, 1986; Cohen & Cohen, 1983). A mediator (B in the top model in Figure 1) falls in the causal pathway between two variables (A and C in the top model in Figure 1; James & Brett, 1984); that is, if A is significantly associated with C, and if A influences B and B influences C, then B is a mediating variable between A and C (more detailed criteria are discussed later). On the other hand, if A is expected to be related to C, but only under certain conditions of B, then B is a moderator variable (see the bottom model in Figure 1). The moderator (B) can be drawn to indicate that it has an impact on the relationship between A and C. Although some variables are more likely to be moderators than mediators (e.g., gender), some variables could serve either function, depending on the conceptual model under investigation (although not in the same analysis; see Lewis & Kliever, 1996; Quittner, Glueckauf & Jackson, 1990; Sandler, Tein, & West, 1994, for examples where coping strategies or social support were tested as both mediators and moderators in competing models). Moreover, both moderators and mediators can be specified within the same model (e.g., moderated mediation; James & Brett, 1984; for examples of this strategy, see Harnish, Dodge, & Valente, 1995; Holmbeck, 1996; Simons, Lorenz, Wu, & Conger, 1993).

An example illustrates the distinction between moderated and

mediated effects. This example is based on Fauber, Forehand, Thomas, and Wierson's (1990) study of marital conflict and adolescent adjustment in intact and divorced families. To examine the processes by which marital conflict has a negative influence on child adjustment, Fauber and his colleagues hypothesized that marital conflict has a negative impact on the quality of parenting to which a child is exposed which, in turn, has an impact on child adjustment. In this case, parenting quality is a potential mediator of the conflict → adjustment relationship and is predicted to account (at least partially) for this relationship. Alternatively, if one sought to test the hypothesis that the conflict → adjustment relationship would hold only for divorced families and would not hold for intact families, then one would be studying whether family structure (i.e., intact vs. divorced) moderates associations between marital conflict and child adjustment.

Statistical Strategies for Testing Moderated Effects

For both moderated and mediated effects, two types of statistical strategies are discussed: multiple regression (as reviewed by Baron & Kenny, 1986, and as used by several investigators) and structural equation modeling (SEM; see Tabachnick & Fidell, 1996, for a relatively straightforward discussion; also see Bollen, 1989; Byrne, 1994; Hoyle, 1995; Jaccard & Wan, 1996; Mueller, 1996). Although SEM is often considered the preferred method because of the information that it provides on the degree of "fit" for the entire model after controlling for measurement error (Peyrot, 1996), proper use of regression techniques can also provide meaningful tests of hypotheses. Moreover, for investigators working in the area of pediatric psychology, where sample *N*s are often relatively small, use of regression techniques (as opposed to SEM) may be necessary because of power considerations (see Tabachnick & Fidell, 1996, for a discussion of sample size and SEM). Although regression strategies may be more familiar to many readers of this journal, user-friendly versions of SEM software are now available (e.g., EQS; Bentler, 1995; although Jaccard & Wan, 1996, argue that LISREL 8, Jöreskog & Sörbom, 1993, is currently the preferred software when attempting to analyze the significance of interaction effects because EQS does not permit nonlinear constraints among parameters).

Regression Approach to Testing Moderated Effects

Although the manner in which moderators are tested statistically varies somewhat depending on whether the predictor and moderator are continuous or dichotomous (Baron & Kenny, 1986; Mason, Tu, & Cauce, 1996), the general strategy is the same regardless of the nature of the variables involved. As noted earlier, a moderator effect is an interaction effect. The preferred strategy is to use the variables in their continuous form (if they are not dichotomies) and to use multiple regression techniques (Cohen & Cohen, 1983; Cohen & Wills, 1985; Jaccard, Turrisi, & Wan, 1990; James & Brett, 1984; Mason et al., 1996).

The predictor and moderator main effects (and any covariates, if applicable) are entered into the regression equation first, followed by the interaction of the predictor and the moderator (e.g., Fuhrman & Holmbeck, 1995). Depending on the investigator's conceptual framework, the main effects can be entered in a hierarchical, stepwise, or simultaneous fashion (Cohen & Co-

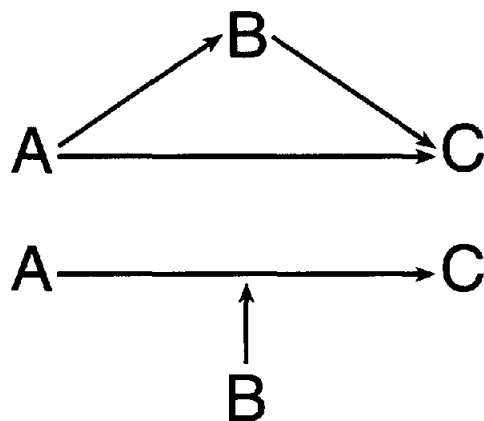


Figure 1. Models of mediated and moderated effects. In the top model, B mediates the relationship between A and C. In the bottom model, B moderates the relationship between A and C.

hen, 1983). For example, in analyses involving marital conflict as a predictor and family structure as a moderator, marital conflict and family structure could be entered in any order or simultaneously. The interaction term is represented by the product of the two main effects (e.g., Marital Conflict \times Family Structure) and "only becomes the interaction when its constituent elements are partialled" (Cohen & Cohen, 1983, p. 305; see also Aiken & West, 1991; Evans, 1991; Friedrich, 1982; Holmbeck, 1989). Thus, although the main effects may be entered in any order, they must be entered before the interaction term for the product of these two terms to represent the interaction when it enters the equation.

Given the manner in which the interaction is computed, the main effects (i.e., the predictor and the moderator) will be highly correlated with the interaction term, which can produce "ill-conditioning" error messages when using some statistical software packages. To eliminate problematic multicollinearity effects between first-order terms (i.e., the independent variable and the moderator) and the higher order terms (i.e., the interaction terms), Aiken and West (1991) have recommended that the independent variable and the moderator be "centered" before testing the significance of the interaction term. To center a variable, scores are put into deviation score form by simply subtracting the sample mean from all individuals' scores on the variable, thus producing a revised sample mean of zero. Such transformations have no impact on the level of significance of the interaction terms or the simple slopes of any plotted regression lines.

Statistically significant interactions are interpreted by plotting simple regression lines for high and low values of the moderator variable (Aiken & West, 1991; Cohen & Cohen, 1983; James & Brett, 1984; for recent examples with data tables or figures, see Brody, Stoneman, & Gauger, 1996; Colder, Lochman, & Wells, in press; Fuhrman & Holmbeck, 1995; Silverberg, Marczak, & Gondoli, 1996; Wagner, Cohen, & Brook, 1996). To plot regression lines, an equation is used that includes terms for the covariates (if applicable), the two main effects (e.g., marital conflict and family structure), and the interaction term (e.g., Marital Conflict \times Family Structure), along with the corresponding unstandardized regression coefficients and the y intercept (Aiken & West, 1991; Cohen & Cohen, 1983; Holmbeck, 1989). By substituting into this equation all possible combinations of high (e.g., $M + 1$ SD) and low (e.g., $M - 1$ SD) values of the predictor and the moderator (i.e., high-high, low-low, high-low, and low-high), two regression lines can be generated where predicted values of the dependent variable are plotted (e.g., Fuhrman & Holmbeck, 1995). Also, as carefully explained by Aiken and West (1991), investigators can test the significance of the slopes for these simple regression lines (e.g., Colder, Lochman, & Wells, in press; Silverberg et al., 1996; Wagner et al., 1996; although, in some cases, associations between the predictor and the dependent variable may be curvilinear; Molina & Chassin, 1996). In the case of categorical moderator variables, high and low values are represented by the two dichotomous dummy values for this variable. With respect to covariates, the means can be substituted for these terms in the equation (which are multiplied by their corresponding regression weights). This strategy for including covariates should only be used, however, if the investigator has tested for the presence of

significant interaction effects between the covariates and independent variables and found them to be nonsignificant.

As a caution to the reader, it is worth noting that significant moderator effects may be difficult to detect statistically. This difficulty is most likely to occur in studies where samples are relatively homogeneous because all high and low values of the moderator and predictor may not be adequately represented (see McClelland & Judd, 1993, for a complete discussion of this issue). Also, unreliability of measurement in the main effects is compounded once a multiplicative term is computed (see Jaccard & Wan, 1995, for suggestions on how to examine and take into account such unreliability when conducting statistical analyses).

SEM Approach to Testing Moderated Effects

Because of the problem of compounding of measurement error when computing interaction terms, several authors have maintained that SEM strategies provide a less biased assessment of the significance of moderator effects (e.g., Jaccard & Wan, 1996; Peyrot, 1996; Ping, 1996). In fact, regression strategies tend to underestimate the effect size of the interaction term, particularly as measurement error in the predictor and moderator variable increases (Jaccard & Wan, 1996; Peyrot, 1996). The SEM strategy is also preferred when the investigator has more than one measured variable for each of the constructs (or latent variables) assessed.

The logic behind testing the significance of interaction effects with SEM designs is relatively straightforward, particularly when the moderator is a dichotomous variable (Jaccard & Wan, 1996; Ping, 1996). Suppose that one is interested in whether the association between a latent predictor variable (which is assessed with more than one measured variable) and a latent criterion variable (which is also assessed with more than one measured variable) vary as a function of gender. To test for the presence of moderation, one assesses the overall fit of the model under two conditions: (a) when there are no constraints on the solution (i.e., when the relationship between the predictor and criterion variables can vary as a function of gender) and (b) when the association between the predictor and criterion variables is constrained to be equal (i.e., an equality constraint) for the two genders (see Farrell, 1994; Jaccard & Wan, 1996, for more in-depth treatments of this data-analytic technique; see Simons et al., 1993, for an empirical example). The effect of this constraint is to test a model where no Predictor \times Gender interaction is present. One can then calculate and test the significance of the difference between the goodness-of-fit chi-square values for the two models. Unlike other data-analytic strategies, nonsignificant (i.e., lower) chi-square values are indicative of a better fit. The magnitude of the difference between chi-square values determines the degree to which an interaction effect is present; that is, if there is a significant deterioration in model fit when evaluating the model under the constraint of the second condition (an assumption of no interaction), this would indicate that a significant interaction is present.

When the predictor, criterion, and moderator are all continuous, the analyses are more complex. On initial inspection, one may assume that all possible products of the measured indicators could be computed as indicators of a latent interaction variable (e.g., there would be 25 such interaction indicators if the moderator

and predictor latent variables were each assessed with 5 indicators). On the other hand, Jöreskog and Yang (1996) and Jaccard and Wan (1996) have maintained that fewer terms are needed but that several constraints must be imposed to test the significance of the interaction effect (a complete discussion of interactions involving continuous variables is beyond the scope of this article; see Jaccard & Wan, 1996, for a discussion of these issues, as well as programming examples using LISREL 8).

Statistical Strategies for Testing Mediated Effects

As was done for moderated effects, both regression and SEM strategies for testing mediated effects are discussed here.

Regression Approach to Testing Mediated Effects

According to Baron and Kenny (1986), four conditions must be met for a variable to be considered a mediator: (a) the predictor, A, must be significantly associated with the hypothesized mediator, B (letters refer to variables in Figure 1), (b) the predictor, A, must be significantly associated with the dependent measure, C, (c) the mediator, B, must be significantly associated with the dependent variable, C, and (d) the impact of the predictor, A, on the dependent measure, C, is less after controlling for the mediator, B.¹ A corollary of the second condition is that there first has to be a significant relationship between the predictor and the dependent variable for a mediator to serve its mediating role. In other words, if A and C are not significantly associated, there is no significant effect to mediate. Such a bivariate association between A and C is not required in the case of moderated effects (nor is it required in the case on an indirect effect, as discussed later).

The four conditions can be tested with three multiple regression analyses (see Eckenrode, Rowe, Laird, & Brathwaite, 1995, for an example that includes figures as well as a complete explanation of this data-analytic strategy). This strategy is similar to that used when conducting a path analysis (Cohen & Cohen, 1983; Nie, Hull, Jenkins, Steinbrenner, & Bent, 1975). The significance of the $A \rightarrow B$ path (in the direction predicted; Condition 1 above) is examined in the first regression, after controlling for any covariates. The significance of the $A \rightarrow C$ path (Condition 2) is examined in the second regression. Finally, A and B are used as predictors in the third equation where C is the dependent variable. Baron and Kenny (1986) have recommended using simultaneous entry (rather than hierarchical entry) in this third equation, so that the effect of B on C is examined after A is controlled and the effect of A on C is examined after B is controlled (borrowing from path-analytic methodology; Nie et al., 1975). The significance of the $B \rightarrow C$ path in this third equation is a test of Condition 3. The relative effect of A on C in this equation (when B is controlled), in comparison with the effect of A on C in the second equation (when B is not controlled), is the test of Condition 4. Specifically, A should be less highly associated with C in the third equation than was the case in the second equation. As Baron and Kenny (1986) discussed, it would be unusual in psychology for this $A \rightarrow C$ effect to be reduced from significance to zero. Thus, the degree to which the effect is reduced (e.g., the change in regression coefficients) is an indicator of the potency of the mediator. Moreover, the significance of the indirect effect can

be tested (Baron & Kenny, 1986). The reader should note, however, that Baron and Kenny's (1986) discussion of Sobel's (1982) significance test only includes an equation that determines the estimated standard error of the indirect effect. A recent article by Sobel (1988, p. 56) includes a more complete explanation of how to apply the significance test and compute confidence intervals for the indirect effect (see also Colder, Chassin, Stice, & Curran, in press; Ireys, Werthamer-Larsson, Kolodner, & Gross, 1994; Lustig, Ireys, Sills, & Walsh, 1996, for empirical examples).

SEM Approach to Testing Mediated Effects

The logic for using SEM to test for mediated effects is similar to that discussed earlier for moderated effects involving a dichotomous moderator. Again, the SEM strategy is particularly useful when one has multiple indicators for the latent variables under investigation.

Assuming that there is a latent predictor variable (A), an hypothesized latent mediator variable (B), and a latent outcome variable (C), one would first assess the fit of the direct effect ($A \rightarrow C$) model (Hoyle & Smith, 1994). Assuming an adequate fit, the investigator then tests the fit of the overall $A \rightarrow B \rightarrow C$ model. Assuming that the overall model provides an adequate fit, the $A \rightarrow B$ and $B \rightarrow C$ path coefficients are examined. At this point, the $A \rightarrow C$, $A \rightarrow B$, and $B \rightarrow C$ paths (as well as the $A \rightarrow B \rightarrow C$ model) should all be significant in the directions predicted (which is analogous to the regression strategy discussed above).

The final step in assessing whether there is a mediational effect is to assess the fit of the $A \rightarrow B \rightarrow C$ model under two conditions: (a) when the $A \rightarrow C$ path is constrained to zero, and (b) when the $A \rightarrow C$ path is not constrained. One then examines whether the second model provides a significant improvement in fit over the first model. As noted earlier, improvement in fit is assessed with a significance test on the basis of the difference between the two model chi-squares. If there is a mediational effect, the addition of the $A \rightarrow C$ path to the constrained model should not improve the fit. In other words, the previously significant $A \rightarrow C$ path is reduced to nonsignificance (i.e., it does not improve the fit of the model) when the mediator is taken into account (which is, again, analogous to the regression approach). It is also useful at this point to report and compare the $A \rightarrow C$ path coefficients for when B is, versus when B is not, included in the model.

An additional consideration in using SEM to test for mediational effects is the important distinction between indirect and mediated effects. An example is used to highlight this distinction. Capaldi, Crosby, and Clark (1996) recently conducted an EQS-based longitudinal study, where they concluded that the effect of aggression in the family of origin on aggression in young adult intimate relationships was mediated by the level of boys' antisocial behaviors during adolescence. On the other hand, Capaldi and her colleagues appear to have found that the

¹ Although Baron and Kenny (1986) list three conditions of mediation (rather than four), their third condition actually contains two subconditions: the predictor (A) must be significantly associated with the outcome (C), and this association must be less after controlling for the mediator (B).

direct path between the predictor and criterion was not significant (i.e., all eight correlations between the measured variables for the predictor and the measured variables for the criterion were nonsignificant), despite the fact that the predictor \rightarrow mediator and mediator \rightarrow criterion paths were significant. Although there is evidence for an indirect effect between predictor and criterion, the findings suggest that the mediator does not (and cannot) significantly "account" for the predictor \rightarrow criterion relationship (because there was not a significant relationship between predictor and criterion in the first place; Hoyle & Smith, 1994). Thus, Capaldi et al.'s (1996) findings fit the criteria for an indirect effect but do not fit the criteria for a mediated effect (as defined here). In the case of such an indirect effect, one must be conservative when discussing interpretations of links between predictor and criterion because one cannot claim that the predictor and criterion are significantly associated.

It is relatively commonplace for investigators who use SEM to claim support for a mediational model, when they have only tested the significance of and found support for an indirect pathway. Statistical textbooks (e.g., Tabachnick & Fidell, 1996) also use "mediational pathway" and "indirect pathway" interchangeably. As noted earlier, it is critical to test whether the direct path between predictor and criterion is significant (Hoyle & Smith, 1994) and, if so, whether this previously significant direct pathway fails to improve the fit of the mediational model.

Inconsistencies in the Testing of Mediation and Moderation: Examples from the Pediatric and Child-Clinical Psychology Literatures

Researchers in the area of pediatric psychology have noted that most chronic illnesses and physical disabilities require ongoing medical management and place considerable physical, psychological, and social demands on the individuals and families involved (e.g., Quittner, 1992). It is also the case, however, that there is considerable variability in the degree to which children and their families exhibit higher levels of adjustment difficulties (Thompson et al., 1993). The fact that there is such variability has led several investigators to suggest mechanisms that buffer (or exacerbate) the impact of illness on adjustment outcomes (e.g., coping resources, family functioning, illness appraisal; Thompson et al., 1993; Thompson & Gustafson, 1996; Wallander & Thompson, 1995). Similarly, child-clinical psychologists have long been interested in the child adjustment outcomes of various stressors as well as factors which account for such stressor \rightarrow outcome associations. One such stressor, marital conflict, has received considerable theoretical and empirical attention (e.g., Cummings, Davies, & Simpson, 1994; Grych & Fincham, 1990; O'Brien, Margolin, & John, 1995).

Although the literatures on adjustment to illness and adjustment to marital conflict during childhood have advanced to the point where model development is now possible, several recent attempts to identify factors which are associated with adjustment have not taken full advantage of the terminological, conceptual, or statistical advances that would facilitate progress in the field. Many of these conceptual and statistical issues relate to use of the terms "mediating" and "moderating." This section will highlight the following types of problems that have begun to emerge in these literatures as investigators have embarked on

the study of moderated and mediated effects (see Appendix): (a) terminological inconsistencies, (b) inconsistencies between terminology and conceptualization, (c) inconsistencies between terminology and statistical analyses, (d) lack of diagrammatic clarity, and (e) lack of conceptual clarity when a proposed mediator represents a "response" to a predictor.

Terminological Inconsistencies

In this section, the following types of terminological inconsistencies will be discussed: (a) idiosyncratic definitions of terms, (b) lack of clarity in the labeling of variables, and (c) interchangeable use of terms.

An example of an idiosyncratic definition of the term mediator comes from the work of Thompson and his colleagues (Thompson, Kronenberger, Johnson, & Whiting, 1989), who have recently presented a transactional stress and coping model of psychological adjustment in children with chronic illness (e.g., Thompson et al., 1993; Thompson & Gustafson, 1996). In an earlier report on the role of central nervous system functioning and family relationships in the adjustment of children with myelodysplasia, Thompson et al. (1989) hypothesized the following:

illness factors (e.g., type, age of onset, and severity), demographic factors (e.g., socioeconomic status), cognitive processes, and social support mediate the relationship between the stress of chronic illness and psychosocial outcome. "Mediate" means that these factors and processes contribute to the variability in psychosocial outcome. In particular, there is theoretical and empirical evidence that family functioning is one type of social support that can lower the risk of poor psychosocial outcome in the face of the stress associated with chronic illness. (p. 243)

Thompson's definition of mediation is clearly at odds with that offered by several authors (e.g., Baron & Kenny, 1986; James & Brett, 1984), as well as standard dictionary definitions (e.g., to mediate is "to serve as a vehicle for bringing about a result . . . to occupy an intermediate or middle position"; *The American Heritage Dictionary of the American Language*, 1969, p. 814). Specifically, there is no specification of an $A \rightarrow B \rightarrow C$ relationship. Although it is not the case that all investigators must adhere to the same definitions of all terms, it is likely that progress in the field will be hampered if the same term is used in different ways by different scholars.

The quote from Thompson et al. (1989) also demonstrates the second form of terminological inconsistency: a lack of clarity in the labeling of variables. Some of the variables that Thompson et al. (1989) list as potential mediators in the quote (e.g., age of onset, socioeconomic status) should probably have been listed as moderators. "Moderation" (rather than "mediation") appears to be what Thompson and his colleagues had in mind, given the last sentence in the quote (which is a clear description of a moderated effect). Variables such as age of onset and socioeconomic status presumably dictate conditions under which the stress of a chronic illness is (or is not) associated with problematic outcomes (i.e., these variables are more likely to serve a moderational than a mediational role; see Hackworth & McMahon, 1991, for a similar lack of clarity in the use of the term "mediating").

Finally, some investigators have, inappropriately, used the

terms "mediating" and "moderating" interchangeably. Baron and Kenny (1986) provide some examples of this problem from the social psychology literature. More recently, Davies and Cummings (1995) appear to be using the terms interchangeably when discussing Grych and Fincham's (1990) model of child adjustment to marital conflict. On the one hand, Davies and Cummings (1995) maintain that Grych and Fincham (1990) "have proposed that intraindividual factors, including cognitive processes . . . and emotional states, interact with the characteristics of marital conflict to shape its impact" (p. 677), which implies that Grych and Fincham have proposed a moderational model. Later in the same article, Davies and Cummings (1995) argued that Grych and Fincham have emphasized "the delineation of cognitive processes mediating the impact of marital conflict on children . . ." (p. 678). Thus, Davies and Cummings have identified the same process as both a moderated effect and a mediated effect (see Hanson, Henggeler, & Burghen, 1987, and Mullins et al., 1991, for similar examples).

Inconsistencies Between Terminology and Conceptualization

Two types of inconsistencies are highlighted in this section: (a) the term "mediator" is used, but the variable in question is not conceptualized as a mediator or a moderator, and (b) the term "mediator" is used, but the variable is conceptualized as a moderator.

As an example of the first type of inconsistency, Thompson and colleagues present a diagrammatic model that includes "mediational processes" (e.g., cognitive processes, methods of coping, family functioning; Thompson, Gil, Abrams, & Phillips, 1992; Thompson et al., 1993; Thompson, Gustafson, & Gil, 1995; Thompson, Gustafson, Hamlett, & Spock, 1992), but the variables contained within these components of the model are not conceptualized as mediators, at least as the authors have described them in their published work.² Instead, Thompson et al. (1993) argued that "child cognitive processes, child pain-coping strategies, and maternal psychological adjustment will account for independent and significant increments in the variance in child adjustment over and above that accounted for by illness and demographic parameters" (p. 469). This is a statement of neither mediation nor moderation; rather, this hypothesis is a statement of relative predictive utility. In an example from the child-clinical literature, Cummings et al. (1994) used children's appraisals of marital conflict and perceived coping efficacy as mediators between marital conflict and child adjustment. Despite the use of the term "mediation," these investigators have not made a clear case for how their variables could serve a mediational function (i.e., they do not present a model in the $A \rightarrow B \rightarrow C$ format, either in written or diagrammatic form).

As an example of the second type of inconsistency (i.e., the term "mediation" is used but the variable appears to be conceptualized as a moderator), Ireys et al. (1994) examined "perceived impact" as a variable that mediates associations between several illness parameters and psychological symptoms. Although the analyses appear to provide accurate tests of mediational effects, Ireys et al. (1994) have implied that perceived impact may serve a moderating function:

Some young adults with a chronic health condition, for example,

view their disorder as negatively affecting most aspects of their lives and may therefore report high levels of psychological symptomatology; others, with similar conditions, may view their condition in a less burdensome light . . . How a young adult perceives that a condition has influenced a developmentally important task . . . appears to alter significantly some of the associations between specific condition characteristics and mental health status. (pp. 206, 219)

These statements appear to describe a moderated effect (rather than the intended mediational effect); an individual's condition is more likely to have a negative effect on outcome when the illness is perceived in a certain way (see Barakat & Linney, 1992, for a similar example).

In an example from the child-clinical literature, Grych and Fincham (1990) have provided a cognitive-contextual framework for understanding children's responses to marital conflict and emphasize "the role of cognitive factors in mediating the relationship between marital conflict and maladjustment" (p. 277). On the other hand, the examples they provide suggest that they are discussing a moderated effect. For example, in discussing causal attributions, they suggest that "a child who views him or herself as a cause of parental conflict is likely to experience more distress than a child who attributes the cause of conflict to one or both parents or to outside circumstances" (p. 282). This statement implies that the effect of marital conflict on adjustment is moderated by the child's attributions insofar as marital conflict is expected to have an impact on adjustment only under certain conditions (see O'Brien et al., 1995; Rudolph, Dennig, & Weisz, 1995, for similar examples).

Inconsistencies Between Terminology and Statistical Analyses

Three types of inconsistencies are highlighted in this section: (a) The term "mediation" is used, but the analyses test neither mediation nor moderation, (b) the term "moderation" is used, but the analyses test neither mediation nor moderation, and (c) a lack of clarity in discussing implications of statistical results.

Although Thompson and his colleagues use the term "mediator," their data analyses do not test for the presence of mediational effects (see also Varni, Wilcox, & Hanson, 1988). Consistent with the predictive utility hypothesis discussed above, Thompson et al. typically use hierarchical regression strategies to assess differential predictive utility rather than mediational effects (e.g., Thompson et al., 1993). On the other hand, Thompson et al. would probably not advance mediational hypotheses (as defined here), given their *a priori* expectation that disease parameters are not likely to be significantly associated with adjustment outcomes (Thompson et al., 1993). Similar inconsistencies have emerged in the child-clinical literature (Cummings et al., 1994; O'Brien et al., 1995).

A related statistical concern is that some investigators have

² Thompson and his colleagues have not used the phrase "mediational processes" in recent diagrammatic versions of their model (e.g., Thompson, Gil, Gustafson, et al., 1994; Thompson & Gustafson, 1996; Thompson, Gustafson, George, & Spock, 1994; Wallander & Thompson, 1995). On the other hand, the figure that they use continues to represent a mediational model, and they continue to use the term *mediate* in their writings (e.g., Thompson & Gustafson, 1996).

not provided complete tests of moderated effects. To explain variability in the adjustment levels of children (and the parents of children) with chronic illnesses and handicapping conditions, Wallander proposed a disability-stress-coping model (Wallander & Varni, 1992). Risk factors are differentiated from resistance factors; the latter "are thought to influence the risk-adjustment relationship, both through a moderation process and via direct influence on adjustment" (Wallander & Varni, 1992, p. 282). Despite the clarity of this conceptualization, Wallander and Varni (1992) apparently have not examined whether their "resistance" factors serve a moderating function, even though it appears that they have the data to test this aspect of their model (although see Wallander & Bachanas, 1997, for an unpublished report). Wallander's strategy to date has been to examine direct (main) effects with hierarchical regressions (e.g., Wallander, Varni, Babani, Banis, & Wilcox, 1989; Wallander, Varni, Babani, DeHaan, et al., 1989), which is similar to Thompson's predictive utility approach (see Barakat & Linney, 1992; Hamlett, Pellegrini, & Katz, 1992; Mullins et al., 1991; for other examples of this approach).

Finally, some investigators demonstrate a lack of clarity when discussing the implications of statistical findings. In Wallander, Pitt, and Mellins's (1990) study of the relationship between child functional independence and maternal adaptation, they argue that "the lack of even a weak relationship in this study suggests there is relatively little to be *moderated* [emphasis added] in this sample" (p. 823). Contrary to this statement, the strongest moderation effects occur (in a statistical sense) when there are no main effects present (i.e., when both independent variables are not associated with the dependent measure; see Baron & Kenny, 1986, Footnote 1). When no main effects are present, a significant interaction would indicate that a pure moderated effect had emerged (i.e., a crossover interaction; Baron & Kenny, 1986).

Lack of Diagrammatic Clarity

Although moderational hypotheses are discussed in the text of Wallander's articles, it is not clear from his figures that moderated effects are proposed. Referring to the diagram of the model (which appears in several of Wallander's articles; Wallander & Varni, 1992; Wallander, Varni, Babani, Banis, & Wilcox, 1989; Wallander, Varni, Babani, DeHaan, et al., 1989), most variable clusters appear to have direct effects on other variable clusters. Moreover, some of the hypothesized moderating variable clusters (i.e., resistance factors) directly affect the predictors (e.g., psychosocial stressors) and the outcomes (e.g., adaptation). The connection between psychosocial stressors and adaptation appears to "pass through" the resistance factors, which appears to be Wallander's diagrammatic strategy for indicating a moderated effect. As it is, however, the model appears to be more mediational than moderational; the figure obscures the "moderating" aspects of the model.³ As evidence of this lack of clarity, other investigators have had differing interpretations of this model. Lustig et al. (1996), for example, maintain that Wallander, Varni, Babini, DeHaan, et al.'s (1989) model suggests that associations between the functional severity of a child's medical condition and maternal adaptation are "mediated" by maternal appraisals and coping. Contrary to this statement, Wallander, Varni, Babini, DeHaan et al. (1989) suggest

that "the impact of these risk factors on adaptation is . . . hypothesized to be moderated by social-ecological factors, intrapersonal factors, and coping" (p. 372; see Brown, Ievers, & Donegan, 1997; Mullins et al., 1991, for similar examples).

Lack of Conceptual Clarity When a Proposed Mediator (e.g., Coping) Represents a "Response" to a Predictor (e.g., Marital Conflict)

In many of the examples discussed thus far, a variable is included in a model that represents a response to another variable in the model. Variables such as coping strategies, cognitive appraisals, and causal attributions cannot exist in isolation; they only exist in relation to variables that have preceded them (e.g., marital conflict, a chronic illness). One cannot exhibit a coping strategy in response to marital conflict, for example, if there is no marital conflict in the first place. Some have also argued that such "response" variables are the mechanism through which the independent variable influences the dependent variable and are, therefore, best thought of as mediators (e.g., Folkman & Lazarus, 1991). In many investigations, such variables are included in a "box" that is placed, connected by arrows, between antecedent (e.g., stress) and outcome (e.g., adjustment) variables (e.g., Barakat & Linney, 1992; Thompson et al., 1993).

On the other hand, investigators who use variables such as coping strategies and cognitive appraisals as mediators rarely provide a complete rationale for how these variables could serve a mediational function (as defined in this article). For example, Thompson and his colleagues (e.g., Thompson & Gustafson, 1996) have not articulated how a child's illness parameters could influence the coping strategies used by the mother (the A → B portion of the model; top of Figure 1). To do so, they would need to select a specific coping strategy (e.g., denial) and propose how such a coping strategy is expected to be used with greater (or lesser) frequency when there are higher (or lower) levels of some illness parameter (e.g., severity of illness; Frese, 1986). They would also need to propose that higher (or lower) rates of certain maternal adjustment outcomes are expected when this particular coping strategy is used with greater (or lesser) frequency (B → C in Figure 1). Finally, they would need to propose that the illness parameters are expected to be associated with the maternal adjustment outcomes (A → C in Figure 1). Although Thompson and his colleagues do not provide this type of conceptualization (nor is such a conceptualization consistent with the types of predictions that are typically advanced by these investigators), it is possible to advance such hypotheses. One might predict, for example, that the higher the severity of childhood illness, the smaller a parent's family support network, which would, in turn, be associated with higher levels of maladjustment. Several investigators have conceptual-

³ To their credit, Wallander and his colleagues have recently provided a revised model that more clearly represents the hypothesized moderated effects (see Wallander & Thompson, 1995; Wallander & Varni, 1995). On the other hand, the moderational effects of the intrapersonal factors and social-ecological factors are still not clearly indicated. It appears that the moderational influence of these two factors is mediated by "stress processing," despite Wallander, Varni, Babani, DeHaan, et al.'s (1989) statements that all three of these resistance factors serve a moderational role.

ized variables such as coping, appraisal, and social support as "mediational," although the degree to which a conceptual rationale is provided varies considerably (e.g., Blankfeld & Holahan, 1996; Holahan, Valentiner, & Moos, 1995; Jose, Cafasso, & D'Anna, 1994; Lewis & Kliever, 1996; Quittner, 1992; Quittner et al., 1990; Sandler et al., 1994).

Contrary to this "mediational" perspective, coping strategies (and other "response" variables) can also be viewed as buffers or protective factors (i.e., moderators) of the stress \rightarrow adjustment relationship (Aldwin, 1994; Cohen & Wills, 1985; Conrad & Hammen, 1993; Holmbeck, 1996; Jessor, Van Den Bos, Vanderlyn, Costa, & Turbin, 1995; Rutter, 1990; see Frese, 1986, for a discussion of the mediational vs. moderational roles of coping strategies). From this perspective, high levels of stress are expected to produce poor outcomes only when the level of the protective factor is low. To examine such protective effects, one would test the significance of Stress \times Protective Factor interactions after entering the main effects.

Why are variables such as coping and appraisal so frequently referred to as mediators and so often represented as mediators in diagrammatic versions of prediction or causal models, without the requisite rationale? Although it is probably impossible to trace the actual roots of this practice, some of the early work on coping and appraisal has been influential (see Thompson & Gustafson, 1996, for a review). Lazarus and Folkman (1984), for example, maintain the following:

Under comparable conditions . . . one person responds with anger, another with depression, yet another with anxiety or guilt; and still others feel challenged rather than threatened . . . In order to understand variations among individuals under comparable conditions, we must take into account the cognitive processes that intervene between the encounter and the reaction, and the factors that affect the nature of this mediation. (pp. 22–23)

Although they use the term "mediation," Lazarus and Folkman (1984) appear to be describing a moderational process. In fact, they clearly endorse an individual differences perspective on coping and appraisal when they argue that there is considerable variability across individuals with respect to how they cope with and appraise stressors and that these individual differences influence the impact of the stressor on the outcome. Despite this perspective, their use of the term "mediation" and diagrams that include mediational causal pathways (e.g., Folkman & Lazarus, 1991) appear to have been more influential than the conceptualization, as is evidenced by the frequent references to Lazarus and Folkman's (1984) theory as an example of a mediational model (e.g., Thompson et al., 1993).

A key distinction in this lack of clarity seems to involve the difference between temporal antecedents and causal antecedents. From a temporal perspective, many of the diagrammatic versions of mediational models make sense. In a recent article by La Greca and her colleagues (La Greca, Vernberg, Silverman, & Prinstein, 1996), for example, a diagram of a mediational model is presented where "exposure to traumatic events" precedes "efforts to process and cope with events," which precedes "posttraumatic stress disorder symptomatology." From a temporal perspective, this figure is understandable insofar as the traumatic event precedes (temporally) the coping efforts which precede (temporally) the adjustment outcome.

On the other hand, a figure such as this lacks clarity as a

causal model (also see Barakat & Linney, 1992; Folkman & Lazarus, 1991; Thompson et al., 1993; Thompson & Gustafson, 1996). Although the occurrence of a traumatic event will precede coping temporally and may (or may not) stimulate the individual to begin coping, whether the level of a stressor is high or low does not necessarily dictate what specific coping strategy will be chosen by a given individual or the degree to which this specific coping strategy will be used (i.e., coping strategies are individual differences variables; Lazarus & Folkman, 1984). As discussed above, a "response variable" model only becomes mediational when the investigator provides predictions that certain specific mediational "responses" (e.g., coping strategies) are expected to be more (or less) likely to be used when the level of a stressor is higher (or lower; see Quittner et al., 1990, for an example of such predictions where social support is used as a mediator). A corollary of this statement is that such models also require that the level of the stressor (e.g., marital conflict, severity of illness) vary across individuals in the study (i.e., one cannot assess the impact of a mediator when the predictor has no variability). Moreover, all individuals in the study should have been exposed to the stressor to some degree (otherwise coping and appraisal strategies are not necessary and become irrelevant for those individuals not exposed to the stressor; Rogers & Holmbeck, 1997).

It is my contention that many of the diagrammatic versions of these "response variable" models should probably be drawn as moderator models (see Figure 1) and should be analyzed as such. "Moderator" modeling and data-analytic strategies would probably be more consistent with the conceptualizations provided by most investigators (e.g., Aldwin, 1994). Moreover, those who advance predictive utility hypotheses should probably not provide figures that include mediated or moderated causal pathways. On the other hand, it is not my contention that variables such as coping, social support, and cognitive processes can never serve a mediational function (although, as discussed above, it is incumbent on the investigator to explain carefully how such mediation can occur; see Lewis & Kliever, 1996; Quittner et al., 1990).

Exemplary Uses of Mediation and Moderation

There are several instances in the pediatric and child-clinical literatures in which moderating or mediating effects have been hypothesized and tested in a manner consistent with the recommendations provided in this review.

Pediatric Psychology

Murch and Cohen (1989) were interested in buffers and exacerbators (i.e., moderators) of the relationship between life stress and psychological distress in adolescents with spina bifida. In this study, the conceptualization and statistical strategy are appropriate and clearly presented (also see Kager & Holden, 1992; Walker, Garber, & Greene, 1994; for other examples of moderator analyses with a pediatric sample). Varni and Setoguchi (1996) conducted a study of associations between perceived physical appearance and adjustment as mediated by general self-esteem in adolescents with congenital or acquired limb deficiencies. Their analyses were conducted in line with Baron and Kenny's (1986) recommendations and their mediational

model was supported. Although their figure was useful, it may have been helpful for the reader if the results of the mediational analyses had been tabled (see Melnyk, 1995, for another example of mediator analyses with a pediatric sample).

Finally, Quittner (1992; Quittner et al., 1990) tested for the presence of moderated and mediated effects within the same study. Mediated effects of social support in a study of families with deaf offspring, are presented diagrammatically and are fully tested. Similarly, moderated effects are discussed and tested as well. In another study that tested the significance of both mediated and moderated effects, Lewis and Kliever (1996) examined associations between hope and adjustment as mediated or moderated by coping strategies in children with sickle cell disease. These investigators found support for the moderational model but not the mediational model. This study serves as a model for maintaining consistency between figures, written text, and tabled data. The tabled data for moderated effects (i.e., Lewis & Kliever, 1996, Table 3) are particularly informative and well organized.

Child-Clinical Psychology

Allen, Leadbeater, and Aber (1994) examined moderators of associations between psychological factors (as well as other predictors) and problem behaviors in at-risk adolescents. In addition to testing for the significance of interaction terms in their data analyses, they also tested for the significance of multiple moderated effects within a longitudinal design. By first controlling for earlier levels of the outcome when predicting later levels of the outcome, they were able to determine whether moderated effects were predictive of stability in the outcome (see Sandler et al., 1994, for another study of moderated effects within a longitudinal context; also see Colder, Lochman, & Wells, in press; Frank & Jackson, 1996; Fuhrman & Holmbeck, 1995; Jessor et al., 1995; Molina & Chassin, 1996; Rogers & Holmbeck, 1997; Silverberg et al., 1996; Wagner et al., 1996, for other examples of moderated effects within the child and adolescent adjustment literature).

Several studies of mediated effects in the literature on child adjustment have used the multiple regression strategy (e.g., Boivin, Hymel, & Bukowski, 1995; Campbell, Pierce, Moore, Marakovitz, & Newby, 1996; Eckenrode et al., 1995; Feldman & Weinberger, 1994; Felner et al., 1995; Lenhart & Rabiner, 1995; Taylor, 1996; Taylor, Casten, & Flickinger, 1993; Taylor & Roberts, 1995). Feldman and Weinberger (1994), for example, examined whether child self-restraint was a mediator of associations between parenting behaviors and child delinquent behavior. Baron and Kenny's (1986) strategy for assessing mediated effects was used. As was the case in the Allen et al. (1994) study, Feldman and Weinberger (1994) used a longitudinal design to assess associations among their variables. Although the study of mediated relationships has received relatively little attention in the child treatment literature (see Treadwell & Kendall, 1996, for an exception), some have suggested that such relationships could be incorporated into meta-analyses of treatment studies (Shadish & Sweeney, 1991).

Other mediational studies have used SEM (e.g., Blankfeld & Holahan, 1996; Colder, Chassin, et al., in press; Conger, Patterson, & Ge, 1995; Harnish et al., 1995; Holahan et al., 1995; Reynolds, Mavrogenes, Bezruczko, & Hagemann, 1996; Simons

et al., 1993). Additionally, the Harnish et al. (1995) and Simons et al. (1993) studies examined both moderated and mediated effects, with Harnish et al. (1995) clearly assessing whether there was a mediational effect by examining the degree to which the direct effect between predictor and criterion was reduced after accounting for the mediator.

Conclusion

This discussion highlighted the need for consistency in the use of the terms "mediating" and "moderating" in the child-clinical and pediatric psychology literatures. It was recommended that care be taken in discussing these processes and that investigators be clear about what statistical approaches are appropriate for a given hypothesis. Because research in pediatric and child-clinical psychology has important treatment, prevention, and public policy implications, and given that the relationship between stress and adjustment is a complex one, appropriate modeling and statistical techniques are needed to move the field toward greater understanding.

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Appendix

Examples of Terminological, Conceptual, and Statistical Inconsistencies in the Research Literature

- A. Terminological inconsistencies
 - 1. Idiosyncratic definitions of terms
 - 2. Lack of clarity in the labeling of variables
 - 3. Interchangeable use of terms
- B. Inconsistencies between terminology and conceptualization
 - 1. The term *mediator* is used, but the variable in question is not conceptualized as a mediator or a moderator
 - 2. The term *mediator* is used, but the variable is conceptualized as a moderator
- C. Inconsistencies between terminology and statistical analyses
 - 1. The term *mediation* is used, but the analyses test neither mediation nor moderation
 - 2. The term *moderation* is used, but the analyses test neither mediation nor moderation
 - 3. Lack of clarity in discussing implications of statistical results
- D. Lack of diagrammatic clarity
- E. Lack of conceptual clarity when a proposed mediator (e.g., coping) represents a “response” to a predictor (e.g., marital conflict)

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