Reconsidering Baron and Kenny: Myths and Truths about Mediation Analysis

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ABSTRACT

Baron and Kenny's (1986) framework for mediation analysis has become a standard part of the consumer researcher's toolkit: an independent variable X affects some mediator M that in turn affects some dependent variable Y. Baron and Kenny proposed a series of three regressions, showing: X affects M; X affects Y; and when X and M are included in the same regression, there is a significant partial effect of M and the partial (direct) effect of X on Y that is less than the effect of X on Y without controlling for M. Baron and Kenny argue that the strongest evidence for mediation is when this partial effect of X on Y is reduced to nonsignificance; they recommend using the Sobel test of the significance of the indirect effect of X on Y through M. Statistical literature has disputed these points, but this literature has not affected practice in consumer research. We argue that a) the outcome of "partial" rather than "full" mediation can be a positive rather than a negative. A "direct" effect of the X on Y when controlling for M can often reflect some omitted second mediator and the sign of the direct effect can give direction to future researchers. Moreover, b) there need not be a significant X-Y link in a proper mediation analysis; c) when the link between X and M is very strong, there may be no partial effect of M in the last equation predicting Y by X and M; d) under some conditions, the Sobel test is very low in power in comparison with newer bootstrap tests by Preacher and Hayes (2004). Worse, authors may report "supportive" Sobel tests that are actually opposite in sign to their theory. Finally, we argue that e) mediation analysis is often misapplied to cases in which the measured mediator M is the same construct as either X or Y. We present a typology for analyzing and interpreting whether mediation exists, classifying its form, and present a decision tree for translating the pattern of findings to interpret of the results and to plan research projects.

Brünhilde's 10-minute aria at the end of a Wagner opera gave rise to the expression "It ain't over 'til the fat lady sings." For those who seek to publish in the *Journal of Consumer Research*, it often seems that the journey from project conception to publication is not over until seeing the outcome of a mediation test of the effect of independent variable X on dependent variable Y through mediator M. Many a research project has stalled in the starting gate or staggered at the finish line of the review process because the data did not conform to Baron and Kenny's (1986) criteria for establishing mediation. Advisors tell their graduate students to start out a project establishing a basic effect. "Once you have the effect, then you can start looking for mediators and moderators." But after the first couple of tries, if the effect is not found the project is abandoned. Other researchers find the effects they are looking for and they propound a meditational account of those effects, but they struggle in the review process when it becomes apparent that that their data do not conform to one or more of Baron and Kenny's criteria.

Is the project not over until Baron and Kenny sing? Or can a project be declared over too soon because Baron and Kenny would not sing? We aim in this paper to show that misapplication of the Baron and Kenny procedure is causing projects to be dropped that may be promising, and papers to be rejected that may deserve publication. We will also suggest that papers are published that seem to conform perfectly to Baron and Kenny's criteria, though they actually reflect a meditational path opposite in sign to what the researchers suggest, or they simply reflect a lack of discriminant validity of the measured mediator, M, from the independent variable or dependent variable. Finally, we will show how misunderstanding of the implications of Baron and Kenny tests causes many authors to ignore important hints about directions for theory building in their next project.

Baron and Kenny's (1986) paper has been cited by 11,480 journal articles as of April 2009, according to *Social Sciences Citation Index*. The citations grew each year between 2002 and 2008 -- 689, 784, 831, 1046, 1266, 1439, and 1722. Ironically, while the popularity of the Baron and Kenny mediation analysis in the social sciences continues to grow, a small technical literature has grown alongside showing flaws in the original Baron and Kenny logic. Points that are now accepted in this technical literature have not diffused to workbench researchers in psychology or in consumer research.

We present a nontechnical tutorial in hope of correcting this deficit. Irwin and McClelland (2001) and Fitzsimons (2008) translated an existing technical literature on moderated regression to the audience of practicing consumer researchers; Baron and Kenny themselves translated a mediation test method suggested by Judd and Kenny (1981). In the same spirit, we aim in this article to explain to practicing users of Baron and Kenny's tests how to correctly interpret the analyses they are running. We add to the post-1986 literature on mediation by explaining the links between the pattern of statistical tests and implications for theory-building, presenting a typology of theoretical mediation models and a decision tree for interpreting results. We discuss the implications of our theses for the basic enterprise of how we conceive of studies to run and decide what to do next at the beginning of a project.

The basic concept of mediation and the Baron and Kenney procedure are now so well known that they are used by authors and requested by reviewers almost reflexively – even when other experimental rather than statistical approaches to theory testing might be more appropriate (Iacobucci, Saldanha, and Deng 2007; Lynch 2007; Spencer, Zanna, and Fong 2005). In the basic setup, an independent variable X is thought to "cause" a distal dependent variable Y through the mechanism of a mediating construct M, as shown in Figure 1, The causal chain X →

M → Y implies that there should be no partial effect of X on Y once one controls for the mediator M. If one allows for the possibility that X could affect Y "directly" (path c) as well as indirectly (path a * path b), then mediation is commonly understood to imply that the relationship between X and Y when one controls for M should be weaker than the relationship between X and Y when one does not control for M.

Figure 1 about here

BARON AND KENNY'S TESTS

Baron and Kenny's (1986) most cited lines refer to three tests:

> "A variable functions as a mediator when it meets the following conditions: (a) variations in levels of the independent variable significantly account for variations in the presumed mediator (i.e., Path *a*), (b) variations in the mediator significantly account for variations in the dependent variable (i.e., Path *b*), and (c) when Paths *a* and *b* are controlled, a previously significant relations between the independent and dependent variables is no longer significant, with the strongest demonstration of mediation occurring when Path *c* is zero." (p. 1176)

Note that Condition (c) requires a significance test for the "direct" Path *c*. Paths *a*, *b*, and *c* are tested and estimated by three regressions, Equations 1, 2, and 3 below.

$$M = i_1 + aX + e_1 \qquad \text{(Eq. 1)}$$
$$Y = i_2 + c'X + e_2 \qquad \text{(Eq. 2)}$$
$$Y = i_3 + cX + bM + e_3 \qquad \text{(Eq. 3)}$$

> "To test mediation, one should estimate the three following regression equations: first, regressing the mediator on the independent variable; second, regressing the dependent variable on the independent variable; and third, regressing the dependent variable on both the independent variable and on the mediator…To establish mediation, the following conditions must hold: First, the independent variable must affect the mediator in the first equation; second, the independent variable must be shown to affect the dependent variable in the second equation and third, the mediator must affect the dependent variable in the third equation." (p. 1177)

Baron and Kenny go on to recommend that the indirect path *a*b* in Figure 1 should be tested via the Sobel z test, which amounts to a test of whether the partial effect of X on Y controlling for M is significantly less than the zero order effect of X on Y.

$$z = \frac{a \times b}{sqrt[b^2 s_a^2 + a^2 s_b^2]} \qquad \text{(Eq. 4)}$$

Here *a*, *b*, and their squared standard errors come from Equations 1 and 3, respectively.

We will dispute the four conclusions from this analysis. First, Baron and Kenny advise that the strongest evidence of mediation exists when X is significant in Equation 2 but reduced to nonsignificance in Equation 3 – that is, when there is evidence of "full mediation." We show that when there is a significant "direct" effect of X on Y in Equation 3, this can inform theorizing about added mediators of the X-> Y relationship. Second, there need not be a significant "effect to be mediated" in Equation 2. The requirement that should supplant Baron and Kenny's "three tests" in the minds of researchers is simply that the indirect effect *a*b* should be significant. The other Baron and Kenny tests are useful primarily in establishing the *form* of the mediation. Third, in a true mediation chain, there need not be a significant partial effect of M in equation 3 when both X and M are included in the model. If the relationship between the independent variable X and the mediator M is very strong, it creates multicollinearity that inflates the standard error of *b* (and *c*) in equation 3 so that *b* is not significantly different from zero, unfortunately compromising the power of the test of the indirect effect. Fourth, the Sobel test is low in power compared to an alternative bootstrap test by Preacher and Hayes (2004), in some cases markedly so. We further note that a researcher testing for an expected positive indirect effect *a*b* may overlook that the indirect effect can be significant and negative despite positive correlations between X and Y, X and M, and Y and M. Finally, we argue that researchers sometimes claim mediation when they have not established that the measure M has discriminant validity from X and Y. We discuss these five points in turn.

PARTIAL MEDIATION AND OMITTED MEDIATORS THAT MASQUERADE AS MYSTERIOUS "DIRECT" EFFECTS: A BOON TO THEORY-BUILDING

Baron and Kenny (1986) asserted that the strongest evidence for mediation is when there is no partial effect of the independent variable X when Y is predicted by both X and the mediator M. When there is a significant indirect path by the Sobel test but the coefficient *c* in Equation 3 is significant, this is "partial mediation." Though "full" mediation is considered the gold standard, Iacobucci (2008, p. 12) notes that "When all tests are properly conducted and reported, the majority of articles conclude with 'partial mediation' as the result." Descriptively, then, evidence of mediation is usually accompanied by evidence of a significant "direct" effect.

What does this mean? The concept of a "direct" effect is clear statistically, but it is less clear what it means theoretically in the context of a mediation test. Is it a problem for the researcher? Sometimes there is an *a priori* theoretical reason to expect an indirect (mediated) effect and a direct effect. For example a researcher might posit that condom availability (X) has an indirect positive effect on sexually transmitted disease (Y) through the mediator of perceived risk of sex with multiple partners (M), similar to Bolton, Cohen, and Bloom's (2006) work on how marketing products as remedies creates "get out of jail free cards." Mapping to Figure 2, path *a* from X to M is negative, as condom availability reduces perceived risk of sex with multiple partners, and path *b* from M to Y is negative, as reduced perceived risk increases sex with multiple partners and hence sexually transmitted disease. Obviously, however, one should expect a direct negative effect of condom availability on sexually transmitted disease through the physical protection produced by condoms. Though the mediated path, *a*b* is positive, it would not be embarrassing to the "get out of jail free" theory that perceived risk does not perfectly mediate the effect of condom availability on sexually transmitted disease – because of the direct effect *c*.

More commonly, authors reporting Baron and Kenny analyses have no reason to hypothesize direct effects *a priori*. They report them offhand in the results section as evidence of "partial mediation," wherein the *a*b* path is significant by a Sobel test and the direct path *c* is also significant in Equation 3. The direct path is simply the "unexplained" part of the X-Y relationship.

Our assertion is that such "direct" paths are often evidence of the effects of one or more omitted mediators. It is quite common for theoretical independent variables to affect theoretical dependent variables by two (or more) mediators. In that case, we assert that the sign of the unexpected direct effect that emerges from a mediation test can provoke theoretical progress, by suggesting the sign of some as-yet-undiscovered second mediation mechanism.

A good example of this process at work comes from work in marketing on "relationship marketing." Morgan and Hunt (1994) introduced the "commitment-trust" theory of relationship marketing. This theory held that relationship-marketing activities led to positive business outcomes by increasing the mediators of trust and commitment. This was obviously a major contribution to theory, as this paper is the most cited in marketing for the past 15 years. But Palmatier et al. (2006) reported a meta analysis of commitment trust studies and found that 2/3 of the total effect of relationship marketing independent variables on business outcome dependent variables was direct, not mediated by commitment and trust. The sign of the direct effect was positive, leading Palmatier et al. (2009) to look for alternative mediators of the same sign. They conjectured that consumer gratitude for the relationship marketing investments might be a second mediator, and empirical work showed that this mediator explained seller performance outcomes over and above commitment and trust.[1]

We conclude, therefore, that there is a silver lining in "partial mediation:" it represents an opportunity for the authors and readers. The sign of the mysterious "direct" effect has heuristic value for theory building.

WHY THERE DOES NOT HAVE TO BE AN "EFFECT TO BE MEDIATED" IN A PROPER MEDIATION ANALYSIS

The starting point for Baron and Kenny's analysis is to test first that there is a significant zero-order effect of the independent variable X (often an experimental manipulation) on the dependent variable Y in Equation 2. This "X-Y test" has been labeled the "effect that may be mediated" (Collins et al, 1998; Judd and Kenny 1981; Kenny et al, 1998; Kenny, 2003; Preacher and Hayes, 2004). Without an "effect to be mediated," it seems intuitive that there is no point to further analysis to investigate whether the effect of X on Y is in fact mediated by M. It is for this

[1] The original "direct" effect can reflect more than one omitted mediator, In such cases, the sign of the direct effect points only to the sign of the combined effects of those omitted mediators, which may be mixed in sign. Indeed Palmatier (2008) shows evidence for alternatives to gratitude, commitment and trust in a second paper. It is of course conceivable that any link in a path analysis can reflect some combination of the modeled effect and an omitted effect or even multiple omitted effects. For instance, when the data seem to support a hypothesized "indirect only" mediation, an apparent zero direct effect could, hypothetically, derive from two underlying omitted mediators of that effect of opposite sign. Parsimony, however, does not favor such complex explanations of data when a simpler account accords with theory.

reason that advisors think they are helping their students by telling them to wait until they have established a basic effect of X on Y before beginning to hunt for mediators.

We aim to show that this intuition can be wrong, and consequently, practicing consumer researchers may be misinterpreting their data. There need not be a significant zero-order effect of X on Y for a theoretically meaningful mediation analysis. What Baron and Kenny (1986) and most users of their tests thereafter have missed is that the zero-order effect of X on Y is in fact mathematically equivalent to the "total effect" of X on Y in Figure 1 above.

$$c' = ab + c \qquad \text{(Eq. 5)}$$

That is, it exactly equals the sum of the "indirect path" (path *a* * path *b*, usually hypothesized) and the "direct path" (path *c*, usually not hypothesized).

If *c* and *a*b* are of the same sign, *c'* will have the same sign. We call this the case of "complementary mediation." In such a situation, Baron and Kenny's test for the significance of the X-Y relation will be satisfied any time that a Sobel test of the indirect *a*b* test would be significant. Consequently, the "X-Y" test is superfluous in this case.

But if *c* and *a*b* are of opposite signs – what we will call "competitive mediation" -- then *c'* can be close to zero, and Baron and Kenny's second condition will fail. As shown in Figure 2 (corresponding to our condom example), the mediated path *a*b* has sign opposite to the "direct" effect *c*. "Competitive" and "complementary" mediations are equally likely and of equal theoretical interest *a priori*. Both point to a theoretically interesting indirect effect. Both identify an unexplained direct effect and guide future research to look for alternative mediators that match the sign of the revealed direct effect. It is nonsensical that only "complementary" mediations should be judged to be publishable, yet this is the net effect of consumer researchers' reliance on Baron and Kenny's (1986) second test of the significance of the X-Y relation.[2]

Figure 2 about here

The problems we just identified with the X-Y test have been recognized in the technical literature on mediation in psychology (Cliff and Earleywine, 1994; Collins et al.1998; Davis, 1985; Judd and Kenny forthcoming; Kenny et al. 1998; Kenny 2003; MacKinnon, 2000, 2008, 2009; MacKinnon et al. 2000, 2002; McFatter, 1979; Shrout and Bolger, 2002; Tzelgov and Henik, 1991). The discussion has been couched in relatively arcane language about "suppressor effects" in multiple regression and has not migrated to consumer research. Up to April 2009, in *Journal of Consumer Research*, *Journal of Marketing*, and *Journal of Marketing Research*, 240 articles cited Baron and Kenny (1986), while 5 cited any of the dissenting or later revisions by Kenny and colleagues. We speculate that the "suppressor" framing of the issues in the technical literature has obscured for practicing consumer researchers the relevance of this debate for more widely shared interests in theory building. Consider a specific example from our literature.

<u>An Example of "Competitive" Mediation: Indirect and Direct Effects Have Opposite Sign</u>

If X affects Y through two mediated paths of opposite signs, but only one is modeled, we will have a case of "competitive" mediation. Figure 3 shows such an example. Mitra and Lynch (1995) resolved a paradox in the literature about when advertising increases and when advertising decreases price sensitivity. "Information" theories in economics held that advertising should increase price sensitivity by making consumers more aware of the existence of

[2] Structural equation model alternatives to Baron and Kenny do not rely on an X-Y test to conclude that an indirect path $a*b$ is significant. We agree with Iacobucci (2008) that the SEM approach is superior to Baron and Kenny because it estimates everything simultaneously instead of assuming Eqs. 1-3 are independent. However, the greater technical complexity of SEM makes it seem unlikely that SEM will supplant Baron and Kenny's approach soon.

substitutes. “Market power” theories held the opposite: advertising differentiates brands that would otherwise be seen as close substitutes. Mitra and Lynch (1995) reconciled conflicting theories and data, positing that advertising affected price sensitivity through its effects on two mediators, consideration set size and relative strength of preference. Increasing advertising by all firms in a market increases consideration set size by “reminding” consumers (Link 1) and increases relative strength of preference by providing differentiating information (Link 2). Larger consideration sets are associated with greater price sensitivity (Link 3). Greater relative strength of preference reduces price sensitivity (Link 4).[3]

Figure 3 about here

Mitra and Lynch did not rely on Baron and Kenny types of regression tests to establish mediation. Rather, they experimentally manipulated factors that should affect the strength of paths 1 and 2 (as in the kinds of approaches to mediation advocated by Spencer et al. 2005) and showed that advertising could lead to either positive, negative, or zero “total” effect on price sensitivity depending on the environment.[4] Dual mediator models like that in Mitra and Lynch are common (e.g., Ahearne et al. forthcoming; Karson and Fischer 1995; Mittal, Kumar, and Tsiros 1999; Palmatier et al. 2009; Preacher and Hayes 2008, Scholten and Sherman 2006). But before some researchers have the insight to propose a dual mediator model, it is likely that several – even hundreds – of articles are published earlier investigating only one of those mediators, as in Palmatier et al. (2009).

[3] Mitra and Lynch also showed evidence for a fifth link between mediators: Stronger preferences for favorites cause consumers to drop less-liked brands from their consideration sets. We ignore this link for the present discussion.

[4] They experimentally varied whether consumers saw no advertising, purely informative “reminder” advertising, or “differentiating” advertising and whether consumers chose in “stimulus-based” environments where memory was unimportant to consideration set formation or “memory-based” environments in which they must remember brands to consider them. Mitra and Lynch were able to show that the same differentiating advertising that made consumers less price sensitive under some conditions (when path a went to zero) made them more price sensitive under other conditions (when path a was positive).

Imagine that Mitra and Lynch had the insight only to anticipate one of the two mediators (say, consideration set size) and that they had run a conventional Baron and Kenny mediation test, omitting the second mediator (strength of preferences). The outcome would have been that the indirect path (link 1 * link 3) would be positive, and the direct path (reflecting an implicit link 2 * link 4) would be negative.

It would not be surprising in such an investigation if the total effect of advertising on price sensitivity– that is, the zero-order X-Y effect – was nonsignificant. In this scenario, it would be easy to imagine the authors giving the project up after failing to find an "effect to be mediated." The authors in this case should be persistent and submit their paper, and the particular pattern of results should have an important effect on what the authors say in their Discussion section. It would be useful for a researcher to report findings showing the positive indirect path and the negative direct path. That finding allows the authors to tell future researchers that a) despite the progress in the paper, there is room for future authors to explain the unexplained portion of the effect of advertising on price elasticity, and b) in searching for added mediators, researchers should focus on those that would produce a negative and not a positive indirect path. If the direct path had been significant and positive, then future researchers would know that their efforts should be to identify secondary mediators producing a positive indirect path.

Thus far we have considered certain possible outcomes of a mediation analysis: complementary mediation when the indirect effect and direct effect have the same sign, competitive mediation when they have opposite signs. In the next section, we present a typology of all possible patterns that a researcher might observe. We then link these patterns to their implications for theory testing and future theory building, attempting to present unified criteria for establishing mediation, understanding the particular type of mediation revealed by the data, and translating the data patterns uncovered into theoretical statements.

A Typology of Mediations and Non-Mediations

It should be evident by now that the Baron and Kenny classification of "full", "partial", and "no" mediation is somewhat coarse and misleading, due to its reliance on a test of the total effect of X on Y. The Table lays out five possible scenarios of a non-recursive three-variable causal model, with a column for each of five types of mediation or non-mediation:

Table about Here
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1. *Complementary Mediation*: Mediated effect (*a*b*) and direct effect *c* both exist and point at the same direction;
2. *Competitive Mediation*: Mediated effect (*a*b*) and direct effect *c* both exist and point in opposite directions;
3. *Indirect-Only Mediation*: Mediated effect (*a*b*) exists, but not direct effect;
4. *Direct-Only Non-Mediation*. Direct effect *c* exists, but no significant indirect effect *a*b*;
5. *No-Effect Non-Mediation.* Neither direct nor indirect effect exists.

Our "complementary" mediation corresponds to Baron and Kenny's "partial mediation"; our "indirect only mediation" corresponds to their "full mediation." Our other three categories of competitive mediation, direct-only non-mediation, and no-effect non-mediation were clubbed together as "no mediation" by Baron and Kenny – a ticket to the file drawer. Other authors (Breslow and Day, 1980; Cliff and Earleywine, 1994; Collins et al. 1998; Davis 1985; MacKinnon et al. 2000; McFatter 1979; Shrout and Bolger 2002; Tzelgov and Henik 1991) have referred to complementary mediations as "consistent" models or "positive confounding," and competitive mediations as "inconsistent" models or "negative confounding." Our last two types have rarely been discussed in this literature, because the "full-partial-no scale assumes one dimension. We contend that proper interpretation of one's data requires two dimensions for the indirect and the direct paths.

In our approach to mediation analysis, *c'* now represents only "total effect" – not the "effect to be mediated." A significant *c'* does not necessarily indicate mediation, while a non significant *c'* does not necessarily indicate lack of mediation. One can see this in the Table; all but one of our five categories of mediation or non-mediation have instances both in the top row of the Table where c' is significant and in the bottom row where it is not. In the one case where a (complementary) mediation type is uniquely associated with a significant c', the c' test adds nothing beyond the more informative *a*b* test. Some authors argued for waiving the XY test in some situations (e.g., Collins et al. 1998; MacKinnon et al. 2000; Shrout and Bolger 2002; Kenny, 2003). It is never relevant when the indirect path is significant and "properly" signed, except insofar as the Sobel test of the indirect effect *a*b* is equivalently understood as the difference between *c'* and *c* – i.e. the difference between the total (or zero order) effect of X on Y and the partial effect of X on Y, controlling for M. Researchers should not give up on a project when they fail to find an "effect to be mediated." It may well be possible to establish an indirect effect despite no total effect.

Figure 4 shows a decision tree to conceptualize five types of mediation and non-mediation introduced in the Table and to convey to readers what really matters in a mediation analysis. The top of the figure shows the statistical path to establishing mediation. The bottom of the figure shows the interpretation of that pattern for conclusions about theory.

Figure 4 about Here

First let us consider the conditions for establishing mediation. In the top part of Figure 4, at the first node, is the indirect path *a*b* significant by a Sobel or bootstrap test? If the answer is "yes", then we have some form of mediation as shown on the left of Figure 4. Thus none of Baron and Kenny's steps really matter to *establishing* mediation. The one and only requirement to establish mediation is to find a significant indirect effect *a*b* by the Sobel test, or, as we will argue later, by a superior alternative bootstrap test (Preacher and Hayes 2004). The results from Baron and Kenny's three equations obviously feed into the parameters of the test of the indirect effect, but they are otherwise secondary for establishing mediation.

The main role we see for their equations is in establishing the *type* of mediation, as explained in Figure 4. Consider the three leftmost paths where the indirect path *a*b* is significant, If the direct effect *c* is not significant in Equation 3, we have "indirect only" mediation, corresponding to Baron and Kenny's (1986) "full mediation." If *c* is significant, then is the product *a*b*c* positive? The answer to this question will be "yes" if the indirect path *a*b* and direct path *c* are of the same sign, signaling "complementary mediation" (corresponding to Baron and Kenny's partial mediation). The answer will be "no" if the indirect path *a*b* and the direct path *c* are of opposite sign, signaling "competitive mediation."

The bottom half of Figure 4 shows the implications of these patterns for theory. First, in all three cases on the left hand side, the data support the hypothesized mediation story X → M → Y. This includes "competitive mediation" where it is likely that there will be no significant treatment effect of X on Y as measured by r_{XY}. Second, for both complementary and competitive mediations, the significant direct effect *c* points to the possible existence of some omitted second mediator that can be pursued in future research. The sign of this direct effect gives guidance for the sign of the omitted indirect path.

Now let us consider the two cases on the right hand side of Figure 4 – when the indirect path *a*b* is not significant. Only the rightmost path, “no effect nonmediation”, should be viewed as a failure. This pattern of non-significant indirect effect *a*b* and nonsignificant direct effect *c can* occur despite is a significant total effect of X on Y (Lynch and Shin 2009), but are no hints about the mechanism for the “effect to be mediated. In the case of “direct only mediation”, there is no indirect effect but a significant direct effect *c*. This pattern is likely to be viewed as disappointing by authors, but the sign of the direct effect can point to as-yet-undiscovered mediators.

Our typology and figure have no separate test for the significance of *b*, contrary to Baron and Kenny’s (1986) claim that *b* must be significant to claim mediation. We turn next to this issue.

WHY TRUE MEDIATION MAY NOT MANIFEST AS A SIGNIFICANT PARTIAL EFFECT OF M IN EQUATION 3 WHEN X IS IN THE MODEL

Baron and Kenny suggested that when X and M are both used to predict the dependent variable Y in Equation 3, the coefficient *b* (on M) should be significant and the coefficient *c* (on X) should not. But consider what occurs when the relationship between X and M gets stronger and stronger in Equation 1. This produces multicollinearity that inflates the standard errors of all variables in a model. This can create the classic situation in which the overall model is significant in Equation 3 but neither coefficient *b* nor *c* is significant.

This can be seen by examining the equation for the standard error of *b* in Eq. 3:

$$\text{SE(b)} = \frac{SD(Y)}{SD(M)} \times \frac{1 - R^2(Y \mid X, M_{Eq.3})}{df_{Error\ Eq.\ 3}} \times \sqrt{\frac{1}{(1 - r_{XM}^2)}} \qquad \text{(Eq. 6)}$$

Equation 6 shows that the standard error of the regression coefficient as the product of three terms. As the fit of Equation 1 goes to 1, the last term in Equation 6 (sometimes called the Variance Inflation Factor or VIF) goes to infinity. See the excellent mediation primer by Iacobucci (2008, pp. 15-16) for references to papers addressing consequences of multicollinarity in mediation analysis.

Finding no significant *b* is not *per se* embarrassing to a mediation story, but it prevents the researcher from being able to persuade others. If *b* is not significant due to this multicollinearity, the indirect effect *a*b* will likely not be significant. We asserted that a significant indirect effect is necessary for establishing mediation. This implies that researchers with correct mediation hypotheses may be disappointed by low power of the test if the indirect effect when there is a close connection between X and M. We now consider the proper statistical procedure for establishing the indirect effect that we claim is the key test for mediation.

SOBEL'S NOT NOBLE

Baron and Kenny (1986) recommended testing the significance of the indirect path *a*b* by the Sobel test shown in Equation 4.

$$z = \frac{a \times b}{sqrt[b^2 s_a^2 + a^2 s_b^2]} \qquad \text{(Eq. 4)}$$

Preacher and Hayes (2004) note that this admonition was largely ignored in published mediation tests, perhaps because Baron and Kenny did not list this as one of their three "steps." But after Preacher and Hayes, the Sobel test has become *de rigueur* in consumer research and psychology, and we will show below how consumer researchers have misapplied and misunderstood that test.

Preacher and Hayes made a strong case that it is insufficient to show the effect of X on Y is reduced in size when M was added to the model. Finding that X has a significant total (zero order) effect on Y in Equation 2 and no significant partial effect in Equation 3 does not mean that there is a *significant* difference between the two. Equivalently, this does not mean that there is a significant indirect effect *a*b* in the numerator of Equation 4, when measured against the standard error of the indirect path in the denominator. Preacher and Hayes developed and shared easy-to-use SPSS and SAS macros for calculating Sobel's z statistic.

<u>Use Bootstrap, Not Sobel</u>

It is ironic (and somewhat unfortunate) that the Preacher and Hayes paper popularized the Sobel test, because that paper argued that more powerful tests of the indirect effect are given by a *bootstrap* procedure; they also provided easy-to use SPSS and SAS macros for the bootstrap test. Bootstrap tests use the sample data to estimate the sampling distribution of *a*b* estimates from re-sampling of the researcher's data, rather than relying on normal distribution theory assumed by the Sobel test (usually improperly). Because the indirect effect is the product of two parameters, the sampling distribution of products is not normal. If *a*b* is positive in the population, the sampling distribution will be positively skewed – with a shorter, fatter tail to the left.

Figure 5 about here

Figure 5 shows the distribution of *a*b* from a Monte Carlo simulation by the authors. We drew 1000 samples of size N = 102 from a multivariate normal population of standardized variates X, M, and Y with correlations of $r_{XM} = .4$, $r_{MY} = .4$, and $r_{XY} = .6$. For each of the 1000 Monte Carlo samples, we computed the Sobel test, estimating both the numerator and the denominator of Equation 4 via Equations 1 and 3 estimated from the 102 data points. The mean of the *a*b* estimates was 0.078 across 1000 such samples, and the mean of the standard errors was 0.040. To determine the power of the test to detect an indirect effect, we tested for each sample whether the 95% confidence interval for that sample included zero; 46.6% of such confidence intervals did not include 0, so power = 46.6%.

One can see, however, from Figure 5, that the sampling distribution of a*b is not normal. But with a typical Sobel test, one infers a 95% confidence interval that is symmetric about the mean. The average of the 1000 95% confidence intervals goes from -0.002 to +0.156 when these are constructed to be symmetric about the mean. But if one instead determined the 2.5% and 97.5% percentiles of the cumulative distribution of the *a*b* values in Figure 4, the 95% confidence interval goes from +0.006 to +0.164. The interval shifts to the right (making it less likely to include 0) because of the skewness in the distribution. Thus, if we could only know the shape of the distribution of a*b products, we could form higher power confidence intervals that are less likely to include 0.

The bootstrap test implemented by Preacher and Hayes (2004) solves that problem, generating an empirical sampling distribution of *a*b*. The bootstrap test takes the researcher's experimental sample of size N, and from it, samples *with replacement* to draw samples of N values of (X, M, Y). For each such bootstrap sample of N, Equations 1 and 3 are estimated, allowing estimation of *a* and *b*, and their product, *a*b*. After, say, 2000 such bootstrap samples have been drawn and *a*b* estimated for each, the SAS and SPSS macros estimate the indirect effect as the mean of these estimates. The standard error of the estimated indirect effect is simply the standard deviation of this empirical distribution of bootstrap estimates of *a*b*. But the bootstrap test actually relies on the 95% confidence intervals from the empirical distribution of *a*b* estimates. The lower bound of the 95% confidence interval is at the 2.5% point on this cumulative distribution – much like that shown in Figure 5 -- and the upper bound of the 95% confidence interval is at the 97.5% point on this distribution.

For the Monte Carlo simulation described above, we used the same set of 1000 samples of 102 date points to compute bootstrap tests for each sample. For the bootstrap test based on a given sample of 102 values, we drew 2000 bootstrap re-samples of 102 numbers sampled with replacement from the original set of 102 numbers, yielding 2000 estimates of *a*b*. We then sorted the 2000 a*b estimates and created 95% confidence intervals by determining the 2.5% and 97.5% values in the cumulative distribution. We determined whether the indirect effect was significant by whether the 95% confidence interval included zero. Of the 1000 such tests, 59.2% of the confidence intervals did not include 0, implying power of 59.2%, compared to the Sobel test power of 46.5% cited above. This improvement in power occurs because the bootstrap test does not assume that the sampling distribution of a*b products is symmetric and normally distributed about the mean. The bootstrap test is nearly always more powerful than the Sobel test, though sometimes only slightly, depending on the particular configuration of r_{XM} , r_{MY} , and r_{XY}.[5]

<u>Two More Surprises about Tests of the Indirect Effect</u>

[5] The bootstrap would always be more powerful except for the offsetting force that Sobel assumes very large samples and so a critical z of 1.96. In those rare instances in which Sobel is more powerful, it's because the researcher is inappropriately using a test that assumes very large samples. This makes researchers more likely to find a significant indirect effect, because they are risking more a Type 1 error. The boostrap test is always better than Sobel when both Type 1 and Type 2 errors are considered.

There are two other surprising properties of the Sobel test. First, Lynch and Shin (2009) have shown that the power of the test to detect an indirect effect does not always increase when *a*b* increases. Moreover, they showed that holding constant the correlations r_{MY} and r_{XY}, it is not the case that one is more likely to detect a significant indirect effect when the parameter a = r_{XM} increases. For example, in Figure 5, we showed above that when $r_{XM} = .4$, $r_{MY} = .4$, and $r_{XY} = .6$, the indirect effect *a*b* = 0.077, and with sample size of N = 102, the power of the Sobel test to detect this effect was 46.5%. In another Monte Carlo for $r_{XM} = .3$, $r_{MY} = .4$, and $r_{XY} = .6$, the indirect effect *a*b* = 0.073; that *a*b* estimate is smaller than the case for $r_{XM} = .4$, yet the power of the Sobel test to detect the effect is 52.2%. If bootstrap tests are used instead of Sobel, the same result holds that power to detect an indirect *a*b* effect is higher for the smaller value of r_{XM} = a=.3 [power = 71.0%] than the larger value of r_{XM} = a=.4 [power = 59.2%].

Perhaps more unsettling, the sign of the indirect effect can actually become negative as a = r_{XM} becomes more positive! In another Monte Carlo simulation for $r_{XM} = .8$, $r_{MY} = .4$, and $r_{XY} = .6$ the mean indirect effect was -0.18. We suspect that it would be easy for researchers to inadvertently claim support for a positive mediation chain after a significant Sobel test of the "wrong" sign. Our informal poll of colleagues suggests that people do not look at the sign of the indirect effect when testing for its significance. Lynch and Shin show that this circumstance of indirect effect opposite in sign to the basic correlations arises when r_{XM}, r_{MY}, and r_{XY} are all significant and positive and a strong correlation r_{XM} co-exists with $r_{MY} << r_{XY}$. Journals should ask authors to report the actual *a* and *b* coefficients and not just the significance of the test of the indirect effect to avoid the unhappy event of publishing a result that is wrong.

If researchers are using the bootstrap test instead of Sobel to test the indirect effect, they must similarly pay attention not just to the statistical significance of the test but also to its sign.

We argue in the next section that the same circumstances of a very strong relationship between X and M should prompt the authors to re-examine the discriminant validity of their mediator from the construct X they are manipulating.

IS IT A MEDIATOR OR A MANIPULATION CHECK?
DISCRIMINANT VALIDITY IN MEDIATION ANALYSIS

Sometimes a researcher's data seem to conform to Baron and Kenny's conditions for mediation, but the "mediator" is not conceptually different from the independent variable: it is effectively a manipulation check. In other cases, the authors have a mediator M that is effectively an alternative measure of the dependent variable Y. In such cases, the data may seem to conform to Baron and Kenny's criteria, but the mediation analysis is theoretically meaningless.

Consider two studies from Levav and Argo (2009) to show the conditions under which a mediation analysis would or would not be informative to readers. The point of the paper is to show that physical contact increases risk taking by promoting a sense of security in participants who are touched. In one study, being lightly touched on the shoulder by a female experimenter caused people to choose riskier investments (more equity relative to investment in a fixed return alternative) in an incentive compatible task, and the authors explained this by the feelings of security engendered by being touched. Feelings of security are presumably distinct from both the construct of touch and the construct of riskiness of decisions, so the mediation analysis showing an indirect effect of touch on riskiness through security was helpful.

But in another study, the authors (appropriately) abstained from reporting a mediation analysis. Here, they primed thoughts of security or lack of security in one task, and in an ostensibly unrelated study, asked respondents to complete the same financial decision task as in the preceding study. Respondents primed with security made riskier choices than those primed with insecurity. Here, a measure of feelings of security is simply a manipulation check. It would add nothing to the study to show that this measure mediated the effect of the priming manipulation on the riskiness of decisions, and M→X→Y might fit as well as X→M→Y.

One of the authors of this paper often reviews manuscripts that are less discriminating in application of mediation claims. Because it is so common for measured mediators to be single item scales, it is difficult to show the discriminant validity of the putative "mediator" vis a vis the independent variable or dependent variable. When such discriminant validity is in doubt, the authors can build a more convincing case by having multi-item scales for the dependent variable and for the manipulation check of the independent variable to be able to show by confirmatory factor analysis that a 1 factor model will not fit either the combined measures of M and Y or the combined measures of a manipulation check for X and M. Armed with these multi-item scales, the authors could foreswear Baron and Kenny's regression approach and follow the structural equation approach advocated by Iacobucci (2008).

CONCLUSION

Baron and Kenny's (1986) framework for mediation analysis has become ubiquitous in the pages of the *Journal of Consumer Research*, with authors showing that an independent variable X affects some mediator M that in turn affects some dependent variable Y. Baron and Kenny followed Judd and Kenny (1981) in proposing a series of three regressions, showing: X affects M; X affects Y; and when X and M are included in the same regression, there is a significant partial effect of M and no significant partial effect of X. Statistical literature has disputed some of these points, but this literature has not affected practice in consumer research.

First, although Baron and Kenny and current practice hold up "full mediation" as the gold standard, descriptively, most published mediation analyses report "partial mediation." Authors report a significant direct path *c* in addition to a significant indirect path a*b. The direct path is rarely predicted or explained. We contend that the unexplained "direct" path can be the reflection of an omitted second mediator, and the sign of the direct path gives a clue to generate hypotheses about alternative mediators consistent with its sign. In this way, the fact of "partial mediation" can have a silver lining of promoting future theory building. "Future research" discussions are often either bland or self-serving calls for research on one's next project. They can be more enlightening than they are now if researchers would speculate about the possible meanings of unexpected "direct" effects and omitted mediators that might explain their sign.

Second, we argue that there need not be a significant X-Y link in a proper mediation analysis. This X → Y link represents the sum of the hypothesized indirect path *a*b* in Figure 1 and the (usually unexpected) direct path c. If *c* is opposite in sign to *a*b*, the researcher may find no significant effect of X on Y when no mediator is included in the model. For similar reasons, it is a mistake to advise one's doctoral students not to worry about mediation at the outset of a project and to "first just establish an effect" of X on Y. If there are multiple mediators of opposite signs, the project may never get out of the blocks. When hypothesized effects don't pan out, think about dual mediation models.

Equally important is to realize that the only requirement for mediation that one should take away from Baron and Kenny is that the indirect effect *a*b* should be significant. When one is at the point of writing one's results up for publication, use the tree diagram in Figure 4 to determine the form of the mediation and as a guide to discussing the implications of the results for theory.

Third, it is not necessarily embarrassing to a mediation account of one's data when one finds no partial effect of the mediator M on Y when the independent variable X and M are both included in the model. This can occur when the link between X and M is very strong, producing multicollinearity that inflates the standard error of the multiple regression coefficient on M. Unfortunately, this will reduce the power of the test of the indirect effect *a*b* that we argue is the one key test for establishing mediation.

Fourth, more recent "bootstrap" tests of the indirect effect have higher power than the Sobel test popularized by Baron and Kenny. If a study seemed to work but the Sobel test was not quite significant, authors may find that the indirect effect achieves conventional levels of significance by the more rigorous bootstrap test, because the Sobel test improperly sets confidence intervals that are symmetric about the obtained estimate of *a*b*. Because the sampling distribution of a*b is skewed, symmetric confidence intervals are more likely to include 0 than the asymmetric confidence intervals in the bootstrap test.

In testing the indirect effect *a*b* by either of these tests, authors should be alert to the sign of the indirect effect. It is possible to have significant positive correlations between X and M, X and Y, and M and Y, and still have the indirect effect be negative in sign.

Finally, authors should know when to refrain from *any* test of mediation. In cases in which the "mediator" is simply an alternative indicator of either X or Y – e.g., if M is a manipulation check -- no mediation analysis should be reported.

We consumer researchers would be better off if we used mediation tests less often than we do and if we could unlearn some "truths' taken on board when we mastered Baron and Kenny

(1986) in our doctoral programs. We hope that the points elucidated in our paper will cause readers to reconsider the advice they give to graduate students and to re-examine old projects that didn't seem to quite work. Results that didn't pass some (unnecessary) Baron and Kenny test may be more promising than the authors or reviewers suspected. That is good news. But our points should also cause readers to double-check that the mediation analyses that they published actually supported the asserted sign of the indirect effect of X on Y through M, and whether their mediators can clearly be distinguished from both X and Y.

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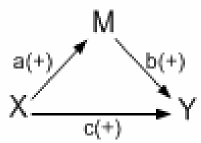
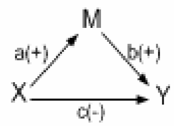
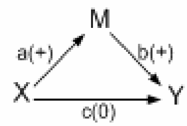
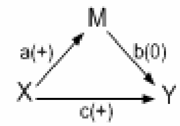
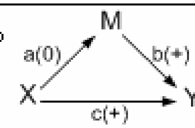
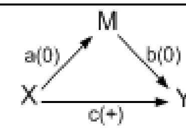
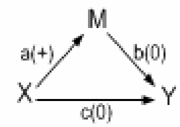
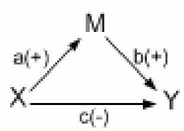
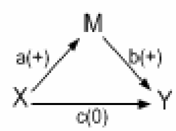
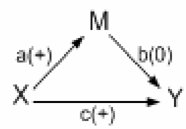
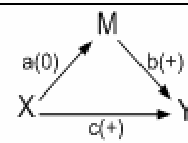
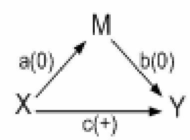
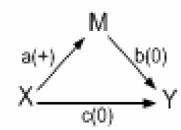
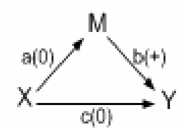
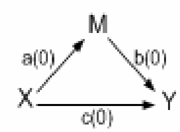
TABLE: A Typology of Mediations & Non-Mediations*

	Mediations			Non-Mediations	
	1. **Complement**	**2.** **Competition**	**3.** **Indirect-Only**	**4.** **Direct-Only**	**5.** **No Effect**
S. **Positive or Negative Total Effect** $c' \neq 0$ $p(ab+c) < .05$	1S 	2S  $\|ab\| \neq \|c\|$	3S 	4Sa  4Sb  4Sc 	5S 
N. **Near Zero Total Effect** $c' \approx 0$ $p(ab+c) \geq .05$	1N 1N is impossible, as a complement model cannot produce a total effect that is statistically non significant.	2N  $\|ab\| \approx \|c\|$	3N 	4Na  4Nb  4Nc 	5Na  5Nb  5Nc 

* We show here a simplified typology of prototypes. 36 unique cells can be added by reversing the coding scale of X, M or Y in some of the 15 prototypes. Each reversal would flip the signs of the two paths pointing at or from the reversed variable. Another 4 unique cells can be added by separating *ab>c* from *ab<c* for 2S. The additions would lead to an expanded typology of 51 cells. The Table displays more positive paths than negative paths because we used positive path(s) for the prototypes whenever appropriate. The expanded typology would have an equal number of positive and negative paths.

+:	Clearly positive path.	*ab*:	Indirect effect of X on Y through M.	*ab+c*:	Total effect that combines direct and indirect effects (same as *c'*).
- :	Clearly negative path.	*c*:	Direct effect of X on Y.	$\|ab\| \neq \|c\|$:	Indirect paths are significantly stronger or weaker than the direct path.
0:	Near-zero path.	*c'*:	Total effect (same as *ab+c*).	$\|ab\| \approx \|c\|$:	Indirect paths are roughly equal to the direct path.

FIGURE 1:
A Three-Variable Non-Recursive Causal Model

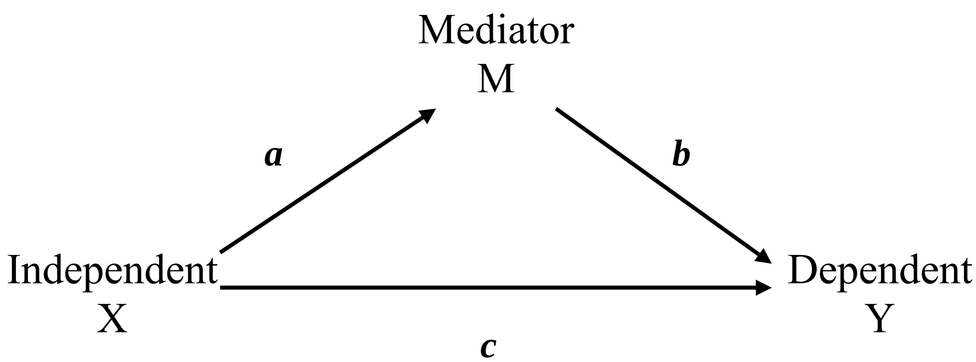
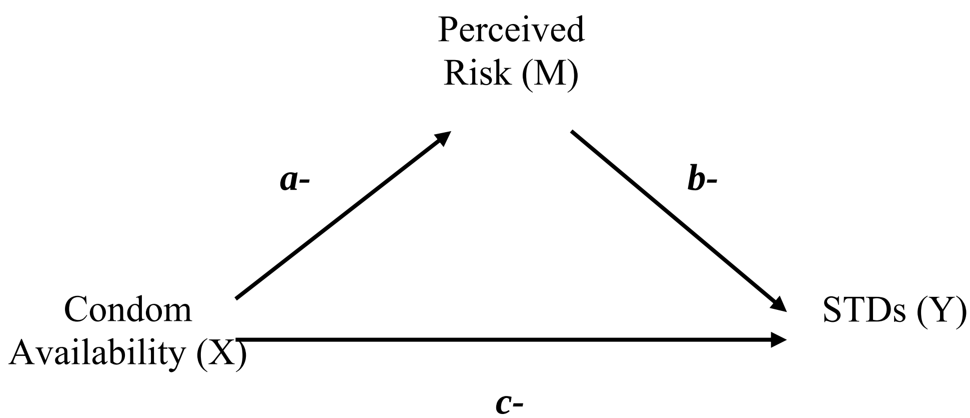
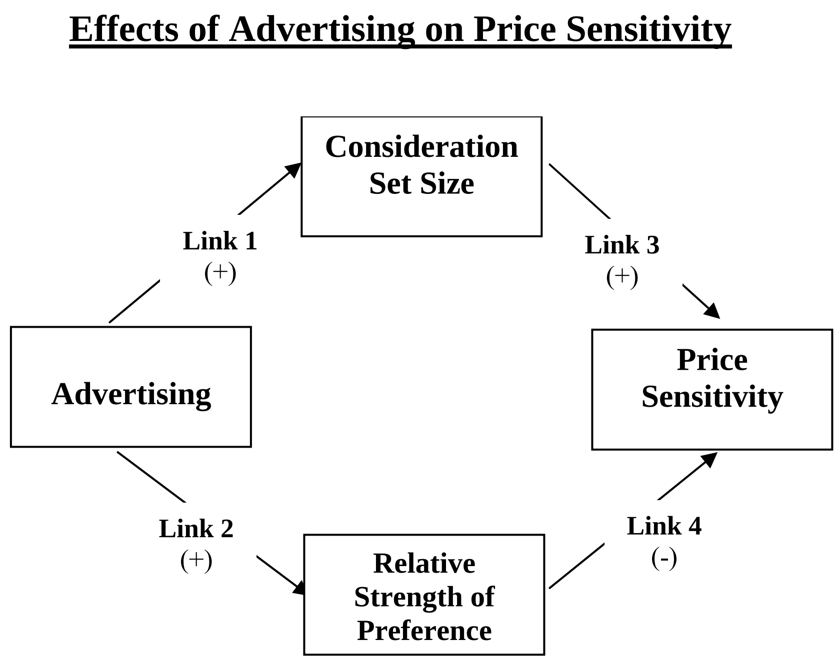


FIGURE 2:
”Competitive” Mediation



Indirect effect positive, direct effect negative.

FIGURE 3: Dual Mediation That Will Appear as "Competitive Mediation" If One Mediator Is Omitted from the Model



Adapted from: Mitra and Lynch (1995). We omit one path from their figure to simplify our point.

FIGURE 4: Decision Tree for
Establishing and Understanding Types of Mediation and Non-Mediation

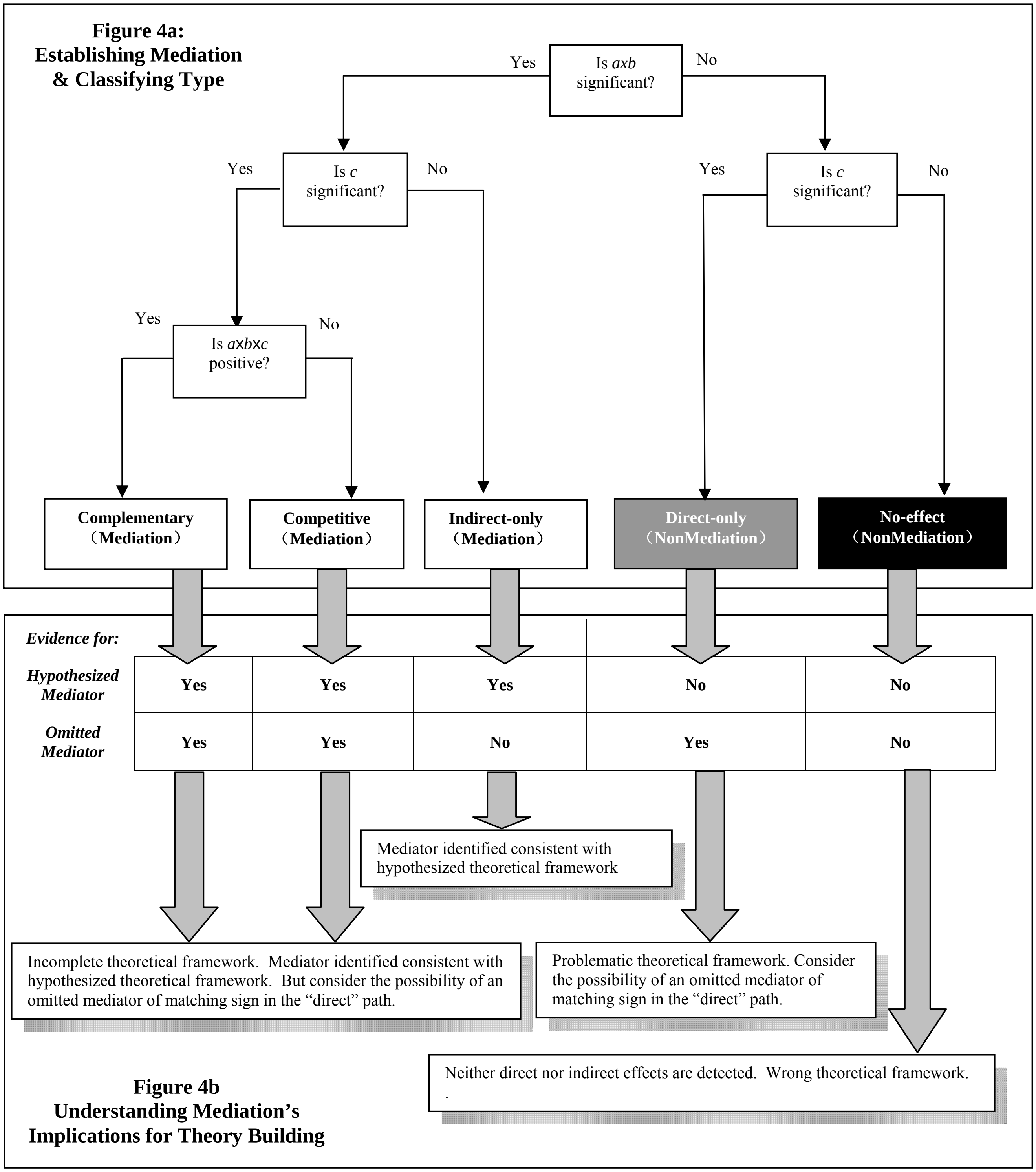


FIGURE 5: Monte Carlo Distribution of Estimates of the Indirect Effect *a*b*

The distribution shown is for the case when random samples of N = 102 cases were drawn from a multivariate normal distribution of standardized X, M, and Y, with $r_{XM} = +0.4$, $r_{MY} = +0.4$, and $r_{XY} = +0.6$. For each sample drawn, we estimated the parameters *a* and *b* from Equations 1 and 3, respectively, yielding one estimate of *a*b*. One can see that the distribution of estimates is skewed, so the left tail is shorter and fatter than the right tail. The mean of the distribution is .0769, so the true 95% confidence interval goes from +0.006 to +0.164. The Sobel test confidence limits are improperly symmetric about the mean (-0.002 to +0.156), leading to lower power to detect the effect.