# Mediation analysis revisited: Practical suggestions for addressing common deficiencies

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*Published in:*
Australasian Marketing Journal

*DOI:*
10.1016/j.ausmj.2018.03.002

# Mediation Analysis Revisited:

# Practical Suggestions for Addressing Common Deficiencies

## Abstract

Four issues that can affect statistical conclusions from mediation analysis are presented here: The implications of omitting mediators; not conducting reverse mediation analysis; using inappropriate measures; and not considering a wider array of experiment-based methods. Suggestions for addressing each of these are advanced. Previous issues of AMJ, JMR and JCR are then examined to gauge the extent to which these suggestions were used. Less than half of the published papers inspected (44.4% of the total) endeavoured to address at least three of the four issues raised above. AMJ authors will realize higher statistical as well as theoretical rigor if they consider these suggestions.

## 1. Introduction

Providing evidence of mediation in empirical experimental studies is critical to theory development and theory testing in marketing research. A mediator is "a variable (M) that transmits the effect of an antecedent variable (X) to an outcome variable (Y) in a casual sequence such that X causes M and M causes Y" (i.e., X → M → Y, MacKinnon, Kisbu-Sakarya, and Gottschall, 2013, p. 338). In the last 30 years significant advancements have been made on how to identify mediators, both theoretically (e.g., Baron and Kenny 1986; Spencer, Zanna and Fong, 2005) and statistically (e.g., Baron and Kenny 1986; Hayes, 2009). In this research note, we review some of the main approaches and their considerations when conducting mediation analysis, and then advance practical suggestions for how to address common pitfalls with respect to mediation analysis. Specifically, we discuss the statistical ramifications of omitting mediators, the benefits of reverse mediation analysis, using multi-item measures to reduce measurement error, and combining experiment-based methods. We conclude by perusing recent issues of *Journal of Marketing Research*, *Journal of Consumer Research* as well as *Australasian Marketing Journal* to assess the extent to which authors embrace one or more of the suggestions advanced.

## 2. The causal chain approach to simple mediation analysis

Efforts to unearth mediating variables have a long and rich history (consider, for example, Festinger, 1957). Early studies might have inferred mediators based on theoretical arguments or provided evidence through correlational analyses (Spencer, Zanna and Fong, 2005). Baron and Kenny's seminal article (1986) changed the landscape. They distinguished between mediators and moderators, and suggested a way of empirically testing for a mediation relationship based on a series of regression models, the causal chain approach.[1] They proposed that three conditions can demonstrate simple mediation. First, X should significantly influence M (i.e., path *a* in Figure 1). Second, X should significantly affect Y (path *c*). Finally, when both X and M are included in the model to predict Y, M should significantly influence Y (path *b*) and the effect of X on Y (path *c'*) should be no longer significant or reduced significantly compared to the direct effect of X on Y (path *c*). Thus, *ab* captures the indirect effect of X on Y via the mediator, and *c'* is the direct effect of X on Y. If path *c'* is zero, it indicates full mediation ($c = ab$); when path c' is not zero it suggests partial mediation. Since its explication three decades ago, the Baron and Kenny causal chain approach has proven quite popular in psychology as well as in marketing, cited over 73,500 times based on Google scholar as of January 2018.

***** Insert Figure 1 about here *****

However, weaknesses have been identified with this approach (Jacoby and Sassenberg, 2011; MacKinnon and Pirlott, 2015; Zhao, Lynch, and Chen, 2010). First, except in the case of full mediation (i.e., when the direct effect of X on Y becomes zero taking a mediator into account)[2], there is a possibility of omitted mediator(s) that might bias the effect of X on Y (Bullock, Green, and Ha, 2010; Zhao, Lynch, and Chen, 2010). Second, unobserved variables could have an influence on both X and M, or M and Y, which could also lead to biased statistical conclusions (MacKinnon and Pirlott, 2015). Third, there can be a measurement order effect. Measuring the mediator first can change Y, or conversely, measuring Y can influence the measurement of the mediator. Researchers often measure all

[1] It is also referred to as a 'measurement-of-mediation design' (Spencer, Zanna, and Fong, 2005).

[2] In section 3.1 we present a simulation that demonstrates mediating variables could be omitted even in the case of full mediation.

constructs in one setting and it is common to measure the mediator(s) after measuring the dependent variable (Fiedler, Schott, and Meiser, 2011; Iacobucci, Saldanha, and Deng, 2007; Jacoby and Sassenberg, 2011). Fourth, although Baron and Kenny (1986) suggest using the Sobel (1982) test for confirming significance of the indirect effect of X on Y via M, the Sobel test assumes that the sampling distribution of the indirect effect is normal. Hayes (2009) notes that this may not be the case. A fifth issue to consider is that raised by Zhao, Lynch, and Chen (2010) and Shrout and Bolger (2002) who argue that there does not need to be a significant relationship between X on Y (path *c* in Figure 1) for there to be an indirect effect of X on Y through M. In such a situation, if a researcher were to strictly follow the methodology advanced by Baron and Kenny (1986), they would discontinue their investigation after the first X → Y regression model failed.

Scholars have therefore advanced different statistical approaches to conducting mediation analysis, most notably bootstrapping (Shrout and Bolger, 2002; Preacher and Hayes, 2004; Zhao, Lynch, and Chen, 2010). Simulations have shown bootstrapping to be a more powerful approach to identifying mediators relative to the causal chain approach (Hayes, 2009). In addition, researchers often use latent measurement models such as structural equation models because they consider the measurement error of the mediator(s) and/or dependent variable (Iacobucci, Saldanha, and Deng, 2007); it is also easier to run more complex models that consider a wider array of constructs/construct relationships (MacKinnon and Pirlott, 2015). Nevertheless, Baron and Kenny deserve much praise for greatly advancing the statistical approach to identifying mediators, and the underpinnings of their approach are largely intact.

Methodological advancements aside, within the next section, we discuss four issues that can adversely affect statistical conclusions reached from mediation analyses and suggest remedies. We start with the case where erroneous conclusions are reached regarding the residual direct effect of X on Y (path $c'$) due to the omission of one or more additional mediating variables.

## 3. Common issues and possible remedies

### *3.1. Consider the implications of omitted mediators*

Recall that if $c = ab$, there is no direct effect of X on Y; instead, M fully mediates the relationship between X and Y (Baron and Kenny, 1986; Shrout and Bolger, 2002; Zhao, Lynch, and Chen, 2010). In these situations researchers commonly assume that there is no omitted mediator(s). However, we demonstrate that there is a possibility that unaccounted mediators can exist, even when $ab = c$ (or $c' = 0$). Through a simulation, it is shown that significantly different conclusions can be reached (e.g., an insignificant direct effect becomes negative) due to incomplete model specification; this is a separate concern to that of under-defining the psychological processes at play, what Spencer, Zanna and Fong (2005) call the theoretical analysis.

Consider Figure 2. Assume in an omniscient world that there are three mediators in the model. $M_1$ (i.e., $a_1 \times b_1 = 1 \times 1 = 1$) and $M_2$ (i.e., $a_2 \times b_2 = 1 \times 1 = 1$) have positive influences on Y, whereas $M_3$ (i.e., $a_3 \times b_3 = 1 \times -1 = -1$) has a negative influence on Y. If a researcher theorized a parsimonious X → M → Y relationship, hence only measured $M_1$, the results would indicate full mediation and the researcher could erroneously assume that $M_1$ is the only mediating mechanism for X on Y. In fact, in this hypothetical case two opposite mediators (i.e., $M_2$ and $M_3$) exist. Claiming *full* mediation based on the single mediator is therefore risky[3].

Endeavouring to eliminate extraneous variables is a criterion for demonstrating causality. Here, we explore the statistical ramifications of the obverse: omitting relevant variables that can lead to confounder bias (MacKinnon and Pirlott, 2015). Referring again to Figure 2, if a researcher measured $M_1$ and $M_2$ in her empirical setting, the direct effect of X on Y (i.e., $c'$) could be significantly negative. In contrast, if the researcher measured only $M_1$ and $M_3$, the direct effect of X on Y (i.e., $c'$) could be significantly positive. We demonstrate this using a simulation.

***** Insert Figure 2 about here *****

Consider the following general model:

$$M_1 = a_1X + u_1$$
$$M_2 = a_2X + u_2$$
$$M_3 = a_3X + u_3$$
$$Y = b_1M_1 + b_2M_2 + b_3M_3 + c'X + \varepsilon$$

To be consistent with the settings in Figure 2, we set $a_1$, $a_2$, and $a_3$ to 1, $b_1$ and $b_2$ to 1, $b_3$ to -1, and $c' = 0$. Let X be drawn from a standard uniform distribution and assume that each error term follows a normal distribution with a mean of 0 and a variance of 1. The sample size is 1,000.

Table 1 shows the parameters and the estimates of the full mediation model and the models with one or two mediators omitted. The results are consistent with our claims. Model 1 includes all three mediators. Here, all the coefficients of the mediators are significant and the 95% confidence intervals include the true parameters, i.e., $b_1 = 1$, $b_2 = 1$, and $b_3 = -1$. In this

[3] Rucker et al. (2011) made a similar warning regarding full mediation; however, their argument is based on measurement error in the case of two mediators.

model, the coefficient of X is not significant, or equivalently the confidence interval includes 0. The next three models consider the effect of omitted mediators.

In Model 2 there is only one mediator, $M_1$, and X. The coefficient of $M_1$ is 1.020 and its confidence interval includes the true parameter of 1. Without $M_2$ and $M_3$, the coefficient of X is not significant and its confidence interval includes 0. In this case, the researcher could falsely conclude the simple model is fully mediated, as the next two models demonstrate.

Model 3 includes two mediators, $M_1$ and $M_2$, and X. The coefficients of $M_1$ and $M_2$ are significant and close to the true parameters (i.e., 1 for each), and the confidence intervals include the true parameters. However, omitting $M_3$ causes a biased coefficient of X. Though the true value is $c' = 0$, the coefficient of X is estimated to be -1.115, which is significantly different from 0. In this case, adding the additional mediating variable yields a very different conclusion regarding the direct effect of X on Y, suggesting it is negative.

In Model 4 there are two mediators, $M_1$ and $M_3$. The coefficients of $M_1$ and $M_3$ are significant and close to the true parameters (i.e., 1 and -1, respectively), and the confidence intervals include the true parameters. However, omitting $M_2$ also causes a biased coefficient of X. Though the true value is 0 the coefficient of X is estimated to 0.894, which is significantly different from 0. That is, omitting the mediator with a positive effect wrongly increases the direct effect of X on Y, in this case from significantly negative (Model 3) to significantly positive.

***** Insert Table 1 about here *****

This hypothetical simulation demonstrates the perils of omitting potential mediating variables from a statistical analysis standpoint, not merely in reducing explained variance, but in reaching erroneous conclusions regarding the residual direct effect of X on Y. Remedies of omitted mediators include: 1) being comfortable that the theoretical model being tested is sufficiently robust, but not excessive (multiple mediators considered); 2) focusing on revealing causality (for example, by combining experiments discussed below in section 3.4) rather than the magnitude of the effect/explained variance; and 3) conducting multiple mediation analyses.

*3.2. Conduct reverse mediation analysis*

Even if mediation analysis provides preliminary support that X → M → Y, there is another reason that could explain significant results. One of them is ‘reversed’ or ‘correlate’ mediation, where M “is not a causal mediator but simply a correlate of the dependent variable” (Fielder, Schott, and Meiser, 2011, p. 1232). Researchers often measure Y prior to measuring M, hence one’s response to Y could significantly influence the response to M (Iacobucci, Saldanha, and Deng, 2007; Jacoby and Sassenberg, 2011). Kim, Kim and Park (2012) considered this possibility. These authors suggested a causal relationship between resource availability (X) and immoral behaviour, in this case intentions to purchase a counterfeit product (Y). They theorized that people’s intension for the immoral behaviour would be heightened when their cognitive resources were not limited (vs. limited) and that the mediator was justifiability of immoral behaviour (M).

To test their theory, participants completed either a difficult task (a complex, rules-based letter finding task) or an easy task (finding a specific letter without additional rules). After doing so, participants were then asked to answer three purchase intensions items (i.e., 1= likely to buy/willing to buy/interested in buying the genuine sweater, to 9 = likely to buy/willing to buy/interested in buying the counterfeit sweater), and then to rate how justifiable they perceived the act of buying the counterfeit product along two items (1 = weakly justifiable/not easy to defend, to 9 = highly justifiable/easy to defend). Consistent with expectations, they found that people's purchase intensions for the counterfeit sweater were higher when participants' cognitive resources were not limited, and that perceived justifiability significantly mediated the relationship between X and Y.

However, the authors acknowledged that participants may have answered the justification questions such that they were consistent with their purchase intentions. To discount this possibility, they performed a second 'reverse mediation' analysis (Lemmer and Gollwitzer, 2017), with perceived justification as the dependent variable and purchase intension the mediator ($X \rightarrow Y \rightarrow M$). In the alternative model there was a residual direct effect, suggesting that resource availability affected justification independent of purchase intentions. An assumption of causal chain mediation analysis is that the causal order of variables is correctly specified. Here, the authors were not questioning their theory (i.e., the causal order), but rather considering the possibility that the data collection process may have introduced a confound. Showing a residual direct effect in the reversed model discounts this concern.

Since the practice of measuring M after Y is common in experimental studies, it is strongly suggested that reverse mediation analysis be conducted. This is recommended in the case of serial mediation models, too (e.g., $X \rightarrow M_1 \rightarrow M_2 \rightarrow Y$). Reverse mediation models in these situations come in various permutations (e.g., $X \rightarrow Y \rightarrow M_1 \rightarrow M_2$ or $X \rightarrow M_2 \rightarrow M_1 \rightarrow Y$; Bellezza, Paharia, and Keinan, 2016; White, Lin, Dahl, and Ritchie, 2016). Likewise, even if experimental settings afford the opportunity to counter-balance the order of measuring M and Y (Iacobucci, Saldanha, and Deng, 2007) or if multiple studies are being conducted to reveal a psychological process (e.g., $X \rightarrow M$, $M \rightarrow Y$; Spencer, Zanna, and Fong, 2005) and measurement order is reversed across the different studies, reverse mediation analysis is encouraged[4]. Doing so, lends support for the veracity of the proposed underlying theoretical process.

*3.3. Use multi-item measures for the mediator(s)*

Many research efforts involve measuring the mediator as opposed to randomly assigning subjects to the X and M conditions (MacKinnon and Pirlott, 2015). It is often the case that the mediator to measure is an abstract construct as opposed to a concrete construct, that is, they "are complex and multicomponential, and the important realization is that responses to the components cause the attribute, that is, they 'make the attribute appear'.'' (Rossiter, 2002, p. 314). Abstract constructs should be assessed using multi-item measures not only for theoretical reasons, but practical reasons as well: tapping abstract constructs with a single item is likely to exacerbate measurement error.

With multiple measures, one can assess the reliability (internal consistency) of the measures. The reverse mediation approach advanced previously would be biased if the reliability of the mediator was lower than that of Y (Lemmer and Gollwitzer, 2017). Without multi-item measures, a construct's reliability is unknown. In addition, using multiple measures can test and improve discriminant validity between the dependent variable and mediator variable (Zhao, Lynch, and Chen, 2010), a necessary condition for causal chain analysis. If M and Y are not theoretically distinct, analysis may return a statistically significant mediation effect when in fact the underlying psychological process is that the

[4] It is possible that the reversed model returns significant effects similar to that of the original model. As Thoemmes (2015, p. 230; see also Danner, Hagemann, and Fielder, 2015) forcefully argues, it is entirely inappropriate to test 'equivalent class' models to see which one is superior: "Instead researchers must invoke and defend assumptions that lend support to their favored model", in reference to the underlying theory. In cases where the reversed model is equivalent, it should be acknowledged that there are issues with the study including: problems with the measurement process, that M and Y are not sufficiently distinct (lack discriminate validity), executional issues such as how subjects were assigned to conditions, or one's underlying theory. Making conclusions regarding mediation would therefore be highly suspect.

independent variable is affecting two correlated outcome variables (Spencer, Zanna, and Fond, 2005). Based on these considerations, it is recommended that multi-item measures be used for the mediator rather than single-item measures. If feasible, it is also recommended that researchers use different approaches across multiple experiments to tap the mediator, such as coupling psychological measurement with reaction times and eye-tracking evidence across multiple studies (Jiang, Adaval, Steinhart, and Wyer, 2014).

### *3.4. Explore experimental methods*

Spencer, Zanna and Fong (2005, Table 1, p. 848) argue that if it is easy to manipulate both the independent variable and the proposed mediating variable, then it is better to use a series of experiments to demonstrate causality rather than the Baron and Kenny (1986) approach. They propose two experimental designs. The first is the 'experimental-causal-chain design' which requires at least two studies, one that tests the causal relationship between X and M (manipulating X and measuring M) and a second that manipulates M to show its effect on Y. This approach allows one to make strong claims regarding causality, but it does not shed insight into the variance explained.

The second method they advance, the 'moderation-of-process design', also assumes that it is relatively easy to manipulate the intervening process. In these designs, X is manipulated as well as M or a moderating variable influencing M (which could be a categorical or continuous variable). It must be shown that the moderator's influence on Y is only through M, and that M's effect on Y is conditional on the moderator.[5] For example,

[5] MacKinnon, Kisbu-Sakarya and Gottschall (2013, p. 350) further categorized two types of moderation-of-process designs, 'the blockage design' and 'an enhancement design'.

Kim, Kim and Park (2012) used a ‘moderation-of-process design’ approach. They proposed that cognitive resource availability (X) would positively affect the ability to justify immoral behaviour (M) which in turn would increase intentions to purchase a counterfeit sweater (Y). Study 1 provided support. To delve deeper into the underlying psychological process (and to provide further empirical support), in a second experiment they manipulated both cognitive resource availability (X) and accountability, a proposed moderator of M. They hypothesized that people in the high (vs. low) accountability condition could not justify engaging in immoral behaviour (hence purchase intentions would be low), thus the aforementioned X → M → Y theorized relationship would only be realized in the low accountability condition. Empirical findings confirmed this outcome. Thus, if it is feasible to manipulate the independent variable as well as the mediating mechanism, combining experiments is the recommended approach[6]; however, it may not be feasible to manipulate both factors, in which case the causal chain approach is still appropriate (Bullock, Green, and Ha, 2010; MacKinnon and Fairchild, 2009; Spencer, Zanna, and Fong, 2005; Stone-Romero and Rosopa, 2008).

In sum, these two experimental approaches are highly recommended alternatives to the measurement-of-mediation approach. This is because they can significantly reduce various weaknesses of the measurement-of-mediation (e.g., reverse causality of M and Y, measurement issue of M). Therefore, combining both approaches across different studies, as did Kim, Kim and Park (2012), is ideal because it provides triangulated evidence of mediation using different methods across multiple studies. Furthermore, researchers could combine two designs (i.e., measurement-of-mediation design and moderation-of-process

[6] In line with our recommendation, Mukhopadhyay, Raghubir, and Wheeler (2018) have recently suggested to use moderation analysis to demonstrate mediation process.

design) in a single study: mediated moderation or moderated mediation (Muller, Judd, and Yzerbyt, 2005; Preacher, Rucker, and Hayes, 2007).

## 4. Evaluating published mediation analyses regarding these suggestions

One aim of this research effort is to assess the extent to which recent experimental research using mediation analysis published in the *Australasian Marketing Journal* (AMJ), the *Journal of Marketing Research* (JMR), and the *Journal of Consumer Research* (JCR) embrace the four suggestions discussed in this research note.

### *4.1. Search criteria and coding process*

In order to screen research for inclusion in the analysis, two search criteria were used. First, we browsed the two most recent years of papers published in JMR and JCR. We also reviewed ten years of work from AMJ. The first two journals were chosen because they are the most frequently referenced journals in marketing. Second, using key words such as 'experiment', 'mediator' and 'mediation', we selected only those papers from each journal that employed an experimental design with mediation analysis. This resulted in 34 publications from JMR, 69 from JCR, and 5 from AMJ – 108 publications in total.

Each publication was then analyzed and coded based on the following criteria: i) whether multiple mediators including alternative(s) were measured and tested; ii) whether reverse mediation analysis was conducted; iii) whether multi-item measures were used rather than single-item measures; and, iv) whether additional experimental methods (i.e., moderation-of-process and experimental-causal-chain) were employed. Many articles had multiple studies, some of which may have met different criteria. We erred on the lenient side.

Thus, for example, if one study within a paper used a single item measure and another study used a multi-item measure (e.g., Jiang, Gorn, Galli, and Chattopadhyay, 2016), we coded the article as having met the criterion of using multiple-item measures. The coding was cross-examined amongst three authors for completeness and consistency.

*4.2. Findings*

The percentage of publications addressing each criterion is illustrated in Figure 3. Amongst the four suggestions proposed in this review, the use of reverse mediation analysis was by far the least used, appearing in 7.4% ( = 8/108) of all articles across the three journals (e.g., White et al., 2016; Yan, Sengupta, and Hong, 2016). Multiple mediators (60.2% = 65/108) were used less frequently than multi-item measures (79.6% = 86/108) or the use of experimental methods (86.1% = 93/108). The majority of experimental methods were the moderation-of-process method (e.g., Bastos and Brucks, 2017; Biswas, Szocs, Chacko, and Wansink, 2017; Stephen, Zubcsek, and Goldenberg, 2016).

Amongst the 108 publications examined, only 3.7% considered all four suggestions, followed by 40.7% that adopted three suggestions and 40.7% adopted two. In sum, by incorporating the suggestions made here there is still room for improvement in developing more rigorous experimental design mediation analysis.

***** Insert Figure 3 about here *****

## 5. Conclusion

Despite significant methodological advancements since Baron and Kenny’s (1986) seminal paper, four issues that can affect statistical conclusions from mediation analysis were presented herein. The four issues identified were: 1) the implications of omitting mediators; 2) not conducting reverse mediation analysis; 3) using inappropriate measures; and, 4) not considering a wider array of experiment-based methods. Suggestions for addressing each of these were then advanced.

Past issues of AMJ, JMR and JCR were then perused to gauge the extent to which these suggestions were considered. Reverse mediation analysis was rare across papers (7.4%) with multiple mediators (60.2%), multi-item measures (79.6%) and experimental methods (86.1%) more frequently used. Less than half of the publications (over 44.4% of the total) endeavoured to address at least three of the issues we raised above. While all five publications from AMJ examined used multi-item measures, they were less likely to use multiple mediators and experimental methods compared to papers in JMR and JCR. AMJ authors would realize higher statistical as well as theoretical rigor if they considered these suggestions.

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**Table 1.**

**Simulation Results**

| True Parameters | (1) Full mediation model | (2) Model with $M_1$ | (3) Model with $M_1$ and $M_2$ | (4) Model with $M_1$ and $M_3$ |
|---|---|---|---|---|
| $b_1$ =1 | $1.046^{***}$<br>[0.984, 1.107] | $1.020^{***}$<br>[0.914, 1.125] | $0.997^{***}$<br>[0.911, 1.083] | $1.069^{***}$<br>[0.984, 1.154] |
| $b_2$ =1 | $0.979^{***}$<br>[0.915, 1.043] | - | $1.008^{***}$<br>[0.919, 1.098] | - |
| $b_3$ =-1 | $-0.980^{***}$<br>[-1.042, -0.918] | - | - | $-1.008^{***}$<br>[-1.094, -0.922] |
| $c'$ =0 | -0.123<br>[-0.280, 0.034] | -0.096<br>[-0.316, 0.123] | $-1.115^{***}$<br>[-1.317, -0.914] | $0.894^{***}$<br>[0.697, 1.091] |

***: $p< .01$.

**Figure 1.**

**The Baron and Kenny's Model of Mediation**

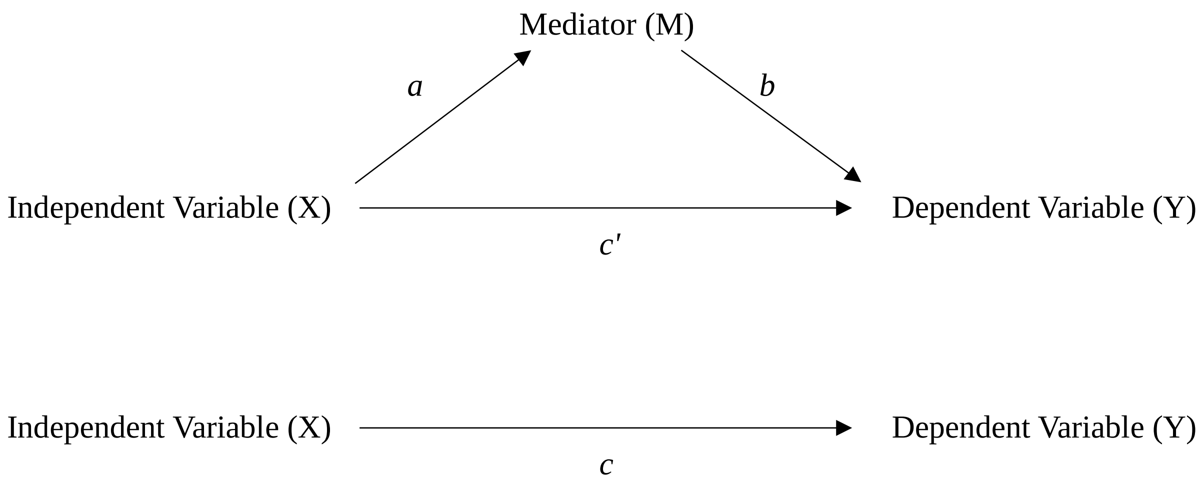

**Figure 2.**

**Three Mediators Model: Hypothetical Illustration**

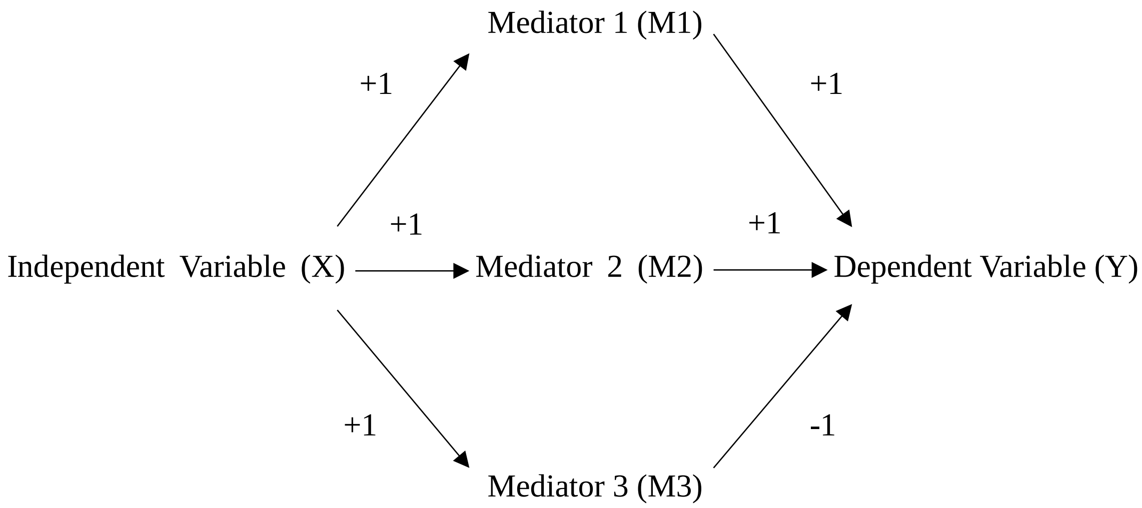

**Figure 3.**

**Percentage of Publications Addressing Each Issue**

**(5 articles in AMJ, 34 in JMR and 69 in JCR)**