**The Impossible Mediation Test:
A Method for Dealing with Plausibly Confounded Mediation**

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Abstract

In much psychology research, mediators are measured, not manipulated. Therefore, the paths from mediators to outcomes—the so-called *b* paths—can be confounded by omitted variable bias, as in any other correlational analysis. The present research builds on the logic of falsification tests in econometrics and sensitivity analysis in statistics to propose the *impossible mediation test*, which can quantify the amount of confounded mediation. Researchers can add to an experiment a condition in which the dependent variable is measured *first*, then the manipulation is implemented, and finally, the posited mediator is measured. This allows for assessment of the spurious association between the dependent variable and the mediator, and statistics can be estimated as if the dependent variable was measured after the manipulation was implemented, to assess whether the spurious association is sufficiently strong to yield the false appearance of mediation. This estimate of “impossible” mediation can be compared to the results obtained from data where the dependent variable is actually measured in the conventional order after the mediator, to determine whether evidence of mediation is stronger in the latter case than the former. Evidence of mediation that survives the impossible mediation test constitutes a strong basis for a claim about mediation of a causal process. The paper illustrates this procedure with an empirical example.

Keywords: Mediation, causal inference, counterfactual model, experimental methods, quantitative psychology.

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An enormous amount of behavioral science research has tested hypotheses that an experimental manipulation (X) causes an outcome (Y) through a mediator (M) (Rucker, Preacher, Tormala, & Petty, 2011). Mediation analysis is often used to test such hypotheses (Baron & Kenny, 1986; Fiedler, Schott, & Meiser, 2011; Imai, Keele, & Tingley, 2010; Keele, 2015; MacKinnon & Fairchild, 2009; Rucker et al., 2011; Vanderweele, 2015). If the indirect effect (ab path)—a function of the effect of X on M (the a path) and the partial effect of M on Y (the b path)—is significantly different from zero, then researchers often conclude that M is a causal mechanism explaining the effect of X on Y .

However, traditional mediation analysis can involve a confound even when research participants have been randomly assigned to experimental conditions (see Figure 1, Panel B in the method section below; Bullock, Green, & Ha, 2010; Imai, Keele, & Tingley, 2010; Keele, 2015; Pearl, 2014; Vanderweele, 2015). Random assignment assures no confounds between treatment assignment and a mediator and between treatment assignment and an outcome. But when the mediator is a measured variable, it can be associated with the dependent variable because both are caused by unmeasured third variables (see Figure 1, Panel B). This possibility runs head on into the conventional assumption that the observed association is due only to causal influence of the mediator on the outcome, and this possibility introduces the potential that a traditional test of mediation will yield a seemingly positive result that is false.¹

¹ Imai et al. (2010) stated, “similar to the ignorability of treatment assignment in observational studies, it is difficult to know for certain whether the ignorability of the mediator holds even after researchers collect as many pretreatment confounders as possible” (p. 313).

To illustrate, consider the following study. High school freshmen in an online interaction with confederates ($N = 610$) were randomly assigned either to be excluded from their peer group (by receiving few “likes” from peers on their profiles) or to be included (by receiving many “likes”). As expected, exclusion (X) increased feelings of being rejected (M), standardized a path = .21, $p < .001$. And feelings of being rejected predicted increased reporting of depression (Y), b path = .57, $p < .001$. Multiplying the two, the ab indirect path is .12, bootstrapped $p < .001$. This is consistent with the claim that feelings of rejection mediated the impact of the exclusion manipulation on depression.

However, in this study, depression was measured *prior* to the manipulation, so mediation is actually impossible, despite appearances to the contrary. Yet the traditional analysis yielded statistically significant results consistent with mediation. The manipulation caused the expected change in the mediator, and feelings of being rejected were positively correlated with depression even before the manipulation was implemented, and the manipulation did not completely eliminate that association. Because more depressed students later reported more rejection, multiplying the a and b paths produced a statistically significant result that could not have been due to mediation.

Several influential papers in statistics and political science have made this point and offered various different recommendations for coping with the problem. Some have argued that confounding in the b path makes causal mediation analysis with correlational data impossible (Bullock et al., 2010). Others have argued that the same tools that are effective for making causal inferences from observational data in the simple “ X causes Y ” case (Morgan & Winship, 2014) can be applied to the “ M mediates effects of X on Y ” case (Imai, Keele, & Tingley, 2010; Keele, 2015).

Figure 1. Possible and impossible mediation, with potential confounds depicted. Note: Values of the experimental manipulation (X) are randomly assigned. White boxes and circles identify variables that are manipulated or observed, and gray circles represent unmeasured variables that may or may not exist. Inclusion of U allows for the possibility of unobserved causes of the outcome (Y) and mediator (M) and the possibility of a reverse causal effect of Y on M . b_{causal} represents the portion of the naïve b path due to changes induced by the experimental manipulation, whereas $b_{confounded}$ represents the portion due to U .

One suggestion made along the latter lines is *sensitivity analysis* (Imai & Yamamoto, 2013). Because the shared causes of M and Y are unobserved, an investigator cannot directly assess the impact of those shared causes on the mediator and outcome. Sensitivity analysis asks how large the associations of the omitted variable confound (U) with the mediator (M) and outcome (Y) must be in order to render the indirect, ab path non-significant (Imai, Keele, & Tingley, 2010; Imai, Keele, Tingley, & Yamamoto, 2014; Imai & Yamamoto, 2013).

Sensitivity analysis has been useful in the causal inference literature in sociology, economics, and political science for some time (Blackwell, 2014; Crosnoe, 2011; Frank, Maroulis, Duong, & Kelcey, 2013; Imai & Yamamoto, 2013). Furthermore, *mediation*, a popular and free statistical package in *R*, outputs sensitivity analysis results by default, and the package's authors have argued that analysts should use and report such results with every mediation test (Imai et al., 2014; Imai & Yamamoto, 2013).

Remarkably, we do not know of any experiments in psychology journals that report and interpret a causal mediation sensitivity analysis, despite some emphasis from psychologists (MacKinnon & Pirlott, 2015). Analysts have added caveats because of the possibility of confounding (Duckworth, White, Matteucci, Shearer, & Gross, 2016; Tackman & Srivastava, 2016) but have not taken empirical steps to provide reassurance about the likelihood that apparent evidence of mediation is illusory.

One possible reason for this is that results of the sensitivity test do not have clear, widely-accepted implications. Statisticians have not yet agreed upon or even proposed reasonable cut-points for evaluating sensitivity analysis results. Indeed, Keele et al. (2015) said that “there is no accepted threshold for which a particular [sensitivity] result can be dichotomously judged to be unacceptable” because sensitivity analyses “are highly model-specific, in that a different procedure has to be derived for each particular combination of mediator and outcome models” (p. 950).

The present research offers a way to quantify the plausibility of a confounded indirect effect in one’s own data and with one’s own design, without having to rely solely on argument, eye-balling, or outside data sources (as in past studies; Blackwell, 2014; Crosnoe, 2011; Frank et al., 2013; Imai & Yamamoto, 2013). The approach builds on a tradition of “falsification tests” in econometrics. In one well-known falsification test, the finding that obesity was contagious through a social network was challenged by showing that the same statistical model showed that height and acne (which can’t be contagious) spreads through social networks (Cohen-Cole & Fletcher, 2008; also see Rothstein, 2009 for an example in teacher value-added scores).

A similar falsification test can be conducted in the context of mediation. We call it the *impossible mediation test* (see panel A in Figure 1). The logic is straightforward. A researcher

can add a fully-crossed factor to an experiment, such that in one condition, the outcome variable is measured before the manipulation is implemented, and in the other condition, the outcome variable is measured after the manipulation is implemented. Among participants for whom the outcome was measured before the experimental manipulation, an effect of a treatment on an outcome is impossible (i.e., c path equals zero at expectation, due to random assignment), and the effect of a treatment on the mediator (the a path) can be estimated without bias, at expectation, due to random assignment. The association of the mediator with the outcome (the b path) is a result *only* of covariance that existed prior to the experimental manipulation (Figure 1 Panel A). Using data from participants among whom the outcome variable was measured after the experimental manipulation, the mediated pathway can be gauged correcting for the measured confound in the “impossible mediation” condition. If the corrected indirect effect is different from zero, this is stronger support for a causal pathway from X to Y through M .

Below, we define the new estimator for addressing confounding in mediation analysis—the impossible mediation test—and outline a novel experimental design for estimating it. We then present an empirical case study.

Method

Impossible Mediation Test

Consider the product of coefficients method for testing mediation (ab indirect effect) (e.g. Mackinnon, 2002), under linearity and monotonicity assumptions. The direct effect of the experimental manipulation X on the outcome Y (c path, or c_1) is represented in equation (1). The effect of the experimental manipulation X on the mediator M (a path, or a_1) is represented in equation (2). The effects of the mediator M (b path, or b_2) and of the experimental manipulation X (conditional on M , the c' path, or c_3) on the outcome Y are represented in equation (3).

$$Y_i = \alpha_{1j} + c_1 X_i + \varepsilon_{i1} \quad (1)$$

$$M_i = \alpha_{2j} + a_2 X_i + \varepsilon_{i2} \quad (2)$$

$$Y_i = \alpha_{3j} + b_3 M_i + c_3 X_i + \varepsilon_{i3} \quad (3)$$

Imai, Keele, and Yamamoto (2010) defined the residual error terms as $\varepsilon_{ij} = \lambda_j U_i + \varepsilon'_{ij}$, where $j \in 2, 3$, corresponding to equations 2 and 3, U is the unobserved possible confound, λ_j is the regression coefficient for the possible confound, and ε'_i are independent and normally distributed. The naïve indirect effect, $ab_{\text{naïve}}$, under the product of coefficients, is defined as $a_2 \cdot b_3$, from equations 2 and 3. We call it “naïve” because it does not take into account potential confounding in the b path. The remaining (non-mediated) direct effect is c_3 , and it and the a_2 coefficients are estimated without bias due to random assignment of X .

Imai, Keele, and Yamamoto (2010) showed that if there are unobserved confounds in the relation between M and Y —that is, if the residuals in equations 2 and 3 are correlated, $\text{Cov}(\varepsilon_{2i}, \varepsilon_{3i}) \neq 0$, due to associations with an unobserved confound, U (see Figure 1)—then the estimate of b_3 (and therefore $ab_{\text{naïve}}$, because it is the product of a_2 and b_3), will suffer from omitted variable bias. Imai et al. (2010) referred to the possibility of confounding in the b_3 path as a violation of “sequential ignorability,” defined as the assumption that, first, the a_2 is unconfounded (which it is usually safe to make, due to random assignment), and, second, the assumption that b_3 is unconfounded (which is often not safe to make).

Following Appendix D in Imai et al. (2010) (p. 67) and Bullock et al. (2010), it can be shown that when substituting equation 2 into equation 3, the causal b path from the mediator to the outcome can be represented as:

$$b_{\text{causal}} = b_3 - \frac{\text{cov}(\varepsilon_2, \varepsilon_3)}{\sigma^2_M} \quad (4)$$

Where $\frac{cov(\varepsilon_2, \varepsilon_3)}{\sigma^2_M}$ is a bias parameter. A derivation is presented in the Appendix of this article.

In an “impossible mediation” condition, $b_3 = \frac{cov(\varepsilon_2, \varepsilon_3)}{\sigma^2_M}$ at expectation, because $b_{causal} = 0$ by construction (because the experimental manipulation occurs after the outcome is measured). When the experiment is conducted with the outcome variable measured after the experimental manipulation, a researcher can estimate $\hat{b}_{causal}$ by calculating $\hat{b}_3$ and then conditioning on $\widehat{Cov}(\varepsilon_2, \varepsilon_3)$, and then testing whether the remaining $\widehat{ab}_{causal}$ parameter (the product $\hat{a}_2 \cdot \hat{b}_{causal}$) is different from zero.

The ability of the impossible mediation test to properly document causal mediation rests on a key assumption: *temporal reversibility* (cf., Holland, 1986). Temporal reversibility is the assumption that the correlation between the residuals of the mediator and the outcome in Figure 1 is the same regardless of when the outcome variable is measured: $Cov(\varepsilon_2, \varepsilon_3)_{\text{conventional order}} = Cov(\varepsilon_2, \varepsilon_3)_{\text{impossible condition}}$.²

It is not possible to directly test temporal reversibility, because doing so would require observing the correlation between the errors among participants in the two groups that reported the variables in the two different orders. Consequently, the assumption that measuring the outcome variable before the manipulation is implemented does not change the effect of the manipulation on the mediator and does not change the measurement of the mediator must be tested indirectly. For example, we can test whether the manipulation of question order affected the mean and variance of the mediator.

² Temporal reversibility might not hold if measuring the outcome variable affected the effect of the manipulation on the mediator.

No new software is needed to test whether the estimate of $\hat{b}_{causal}$ produced by the impossible mediation test is different from zero. It can be done via structural equation modeling using the *lavaan* package in *R* (Rosseel, 2012) or the *mediation* package in *R* (Tingley, Yamamoto, Hirose, Keele, & Imai, 2014). The results should be identical in some cases, but the *mediation* package relaxes the need for some assumptions (e.g., linearity, no Manipulation $\times$ Mediator interaction; see Imai et al., 2010) and so is more flexible, but this approach has the limitation of being susceptible to random variation in the estimate of the a path across conditions, as explained below.

Data for Empirical Case Study

We collected data from a study that executed a typical design in psychology: a randomized manipulation (X) affects a person's report of one psychological variable (Y) through reports of an intermediary psychological variable (M). Study hypotheses and analyses were pre-registered prior to data collection, and study materials, data and syntax are posted (<https://osf.io/q3ums/>). Unlike many studies, our study added a randomized factor such that in the *conventional question order* condition Y was measured after X was implemented and M was measured, but in an *impossible mediation condition* Y was measured first.

The experimental manipulation variable X was coded such that $1 = \text{Treatment}$, $0 = \text{Control}$. Participants who received the treatment were expected to show increased values of the mediator M , which was a 10-point self-report measure whose values were re-coded by a simple linear transformation to range continuously from 0 to 1. The mediator M was in turn expected to be negatively associated with the outcome Y . The mediator M was also a self-report measured on a 10-point scale that was linearly transformed to range continuously from 0 to 1. Participants

randomly assigned to receive the treatment materials were expected to show lower values of the psychological measure Y . Hence, we expected a negative indirect path from X to Y via M .

Data were collected via the Internet from American adults who volunteered to complete questionnaires for money and answered questions while in their homes or in any other locations of their choosing (Maximum of $N = 3,035$; degrees of freedom varied across analyses due to missing data on some measures). See the supplemental material for more detail on the sample.

Analytic Approach

Below, we report the results of analyses testing for mediation when a study was conducted in the conventional order and in the impossible mediation condition using common methods: (1) that described by Baron and Kenny (Baron & Kenny, 1986); (2) a bootstrapped indirect effect with the a and b paths estimated in separate regressions (Preacher & Hayes, 2004); (3) structural equation modeling of indirect effects; and (4) the Imai et al. (2010) method, which employs seemingly unrelated regressions and bootstrapped confidence standard errors.

After this preliminary analysis, we describe the results of the impossible mediation test implemented using both SEM and the Imai et al. method.

Results

Traditional Mediation Techniques That Do Not Account for Confounding

Preliminary tests of manipulation effects. As expected, the experimental manipulation, X , affected M and Y as expected. When the variables were measured in the conventional order, there was a significant a path ($X \rightarrow M$, see row 1 in Table 1 or column 1 in Table 2). The experimental manipulation furthermore altered the outcome as expected, resulting in a significant c path ($X \rightarrow Y$, row 2 in Table 1 or column 2 in Table 2).

Among participants in the impossible mediation condition, there was again a significant a path ($X \rightarrow M$; see row 3 in Table 1 or column 3 of Table 2), but, as expected, no significant c path ($X \rightarrow Y$), because measurement of the “outcome” occurred prior to implementing the experimental manipulation (row 4 in Table 1 or column 4 of Table 2). In sum, all main effects aligned with expectations.

Table 1. Means and Standard Deviations of the Mediator and the Outcome Variable, by Question Order and Experimental Condition

Variable	Experimental Condition				Effect of Treatment Assignment on the Mean of Each Variable	
	Control: X = 0		Treatment: X = 1			
	<i>M</i>	<i>SD</i>	<i>M</i>	<i>SD</i>	<i>t</i>	<i>p</i>
<u>Outcome Measured Last</u> (Conventional Order Condition)						
Mediator (<i>M</i>):	.49	.35	.63	.33	7.67	<.001
Outcome (<i>Y</i>):	.69	.21	.66	.23	3.27	.001
<u>Outcome Measured First</u> (Impossible Mediation Condition)						
Mediator (<i>M</i>):	.50	.34	.62	.33	6.69	<.001
Outcome (<i>Y</i>):	.68	.20	.69	.21	0.33	0.74

Finally, as expected the X (experimental manipulation) $\times$ Order of measurement interaction was statistically significant when predicting the outcome, Y , $F(1, 3031)=7.22$, $p=.007$. Thus, the non-significant effect of the manipulation among participants in the impossible mediation condition was significantly different from the statistically significant effect of the manipulation among participants whose outcome variable was measured after the manipulation.

Reversibility assumption. Consistent with the *temporal reversibility* assumption, the effect of the manipulation on the mediator was the same among participants regardless of the order in which the outcome variable was measured: the interaction of the Experimental manipulation $X \times$ the order in which the variables were measured was non-significant predicting the mediator, M , $F(1, 2841)=0.66$, $p=.42$.

Second, the question order manipulation did not change the variance in the mediator, M , either among participants in the control group ($X=0$), Bartlett's $K^2=0.16$, $df=1$, $p=.69$, or among participants in the treatment group ($X=1$), $K^2=1.91$, $df=1$, $p=.17$. And this non-significant effect of the question order manipulation on the variance was the same in these two groups of participants: the interaction of question order with the experimental manipulation X when predicting the variance of the mediator was non-significant, $K^2=5.25$, $df=3$, $p=.15$. These results are all consistent with the assumption that the question order manipulation did not disrupt the measurement of the mediator.³ In sum, these analyses suggest that the present empirical case study provides an informative example for demonstrating the impossible mediation test.

³ Future studies might implement additional tests to address the temporal reversibility assumption if appropriate additional measures are collected. For instance, it might be possible to test whether concurrent and predictive validity of the mediator are consistent regardless of the order in which the variables are measured by gauging associations of the mediator with variables other than the outcome. No such variables were measured in the study being described here.

Baron and Kenny's method. According to Baron and Kenny (1986), a treatment effect is mediated when (1) the a path is statistically significant, (2) the b path is statistically significant, (3) the c path is statistically significant, and (4) the c' path is weaker than the c path when accounting for the b path, as assessed by a Sobel test.

Among participants asked questions in the conventional order, criteria 1-3 were met, as shown in rows 1 and 2 of Table 1 and columns 1 and 2 of Table 2. A Sobel test was highly significant, $\widehat{ab}_{naive:conventional\ order} Z = 7.67, p < .001$, satisfying the fourth criterion. Thus, participants who answered questions in the conventional order manifested evidence consistent with mediation per Baron and Kenny's (1986) approach.

Among participants in the impossible mediation condition, criteria 1 and 2 were met, and, as expected, criterion 3 was not met (because a c path was impossible and non-significant, see row 4 of Table 1 and column 4 of Table 2). A Sobel test was highly significant $\widehat{ab}_{naive:impossible\ condition} Z = 6.10, p < .001$, satisfying criterion 4. Thus, because participants in the impossible mediation condition did not manifest a significant c path, Baron and Kenny's (1986) standards for mediation are not met, as expected.

Preacher and Hayes bootstrapping method. Next, we implemented Preacher and Hayes' (2004) bootstrapping method, calculated with the MBESS package in *R* (Kelley, 2007).⁴ With 1000 re-samples and the percentile-corrected 95% confidence intervals, the indirect effect (the ab path) of the experimental manipulation X on the outcome Y via the mediator M among participants who reported the outcome variable after the manipulation was implemented was $\widehat{ab}_{naive:conventional\ order} = -.036, 95\%CI [-4.68, -.026]$. Among participants in the impossible

⁴ The bootstrapping method has replaced Baron and Kenny's (1986) as the most commonly recommended in much of psychology (Zhao, Lynch, & Chen, 2010).

mediation condition, the indirect effect was also significant, $\widehat{ab}_{naive:impossible\ condition} = -.021$, 95%CI [-2.93, -.01]. Thus, the Preacher and Hayes (2004) method produced evidence of significant mediation, even though mediation was not possible due to the study design.

Structural equation modeling method. In a structural equation model estimated with data from participants who answered questions in the conventional order, the indirect effect (the ab path) of the experimental manipulation X on the outcome Y via the mediator M was again significant, $\widehat{ab}_{naive:conventional\ order} = -.036$, $SE = .005$, $Z = 6.836$, $p < .001$ (see unstandardized coefficients in Column 1 of Table 3, and standardized estimates in Figure 2, Panel B). The same model estimated for participants in the impossible mediation condition also produced a substantial and significant indirect effect of X on Y , $\widehat{ab}_{naive:impossible\ condition} = -.023$, $SE = .004$, $Z = 5.879$, $p < .001$, even though mediation is not possible (see Table 3, Column 3 and Figure 2, Panel A).

Table 2. Baron and Kenny Regressions Estimates the a , b , and c' paths, by Question Order.

Predictor	Outcome Measured Last (Conventional Order Condition) (N = 1435)				Outcome Measured First (Impossible Mediation Condition) (N = 1410)			
	<u>Predicting M</u>		<u>Predicting Y</u>		<u>Predicting M</u>		<u>Predicting Y</u>	
	b	p	b	p	b	p	b	p
X	0.139	<.001	-0.001	>.50	0.118	<.001	0.028	<.001
M			-0.264	<.001			-0.18	<.001

Imai et al. method. Using the *mediate* function, which relaxes linearity and no-interaction assumptions (Tingley et al., 2014), among participants who answered questions in the

conventional order, the estimated indirect effect was very similar in magnitude to that yielded by the structural equation modeling approach, $\widehat{ab}_{naive:conventional\ order} = -.037$, 95% CI [-.048, -.027], $p < .001$. Furthermore, the indirect effect was again significant among participants in the impossible mediation condition, $\widehat{ab}_{naive:impossible\ condition} = -.021$ 95% CI [-.028, -.014], $p < .001$.

Summary. Thus, all four methods that have been used frequently in the social and behavioral sciences—the Sobel test, the bootstrapping method, the SEM method, and the Imai et al. method—produced indications of significant mediation even when none was possible. This is the first empirical demonstration that we know of illuminating the validity of the concerns expressed by critics of mediation who have said that conventional tests can yield false mediation due to correlated residuals (Imai, Keele, & Tingley, 2010; Imai et al., 2014; Imai & Yamamoto, 2013).

Furthermore, this empirical demonstration challenges the recommendation that investigators can legitimately reach the conclusion that mediation occurred even when the effect of the manipulation on the outcome variable is non-significant. For example, (Zhao, Lynch, & Chen, 2010) said: “There need not be a significant r_{XY} in a proper mediation analysis ... it is a mistake to advise students to ‘first just establish an effect (to be mediated)’ before starting to think about and test mediation. The only requirement for mediation is that the indirect effect $a \times b$ be significant” (p. 205). But we have shown that in the impossible mediation condition, this indirect effect appears to be significant when it is impossible. Of course, in the impossible mediation condition, the effect of the manipulation on the outcome variable is zero (because the latter is measured before the former is implemented), and that non-significance is the safeguard against a false positive conclusion.

Correcting for Confounding: The Impossible Mediation Test

We next describe results obtained using two methods to test for mediation while eliminating the misleading impact of the correlation between residuals (see Figure 2 for a depiction of the method).

Structural equation modeling. Among participants in the impossible mediation condition, a structural equation model can be used to estimate the covariance between the residuals to the mediator and the outcome, $\widehat{\text{Cov}}(\varepsilon_2, \varepsilon_3)$ while constraining $\hat{b}_{causal}$ to equal zero (Column 5, Table 5). Then, the parameters of the model can be estimated using data from participants who answered questions in the conventional order, but constraining the covariance of the residuals to M and Y to be equal to the estimate of that covariance obtained from participants in the impossible mediation condition, $\widehat{\text{Cov}}(\varepsilon_2, \varepsilon_3)$ (Column 7, Table 3). The estimate of the $\hat{b}_{causal}$ path is then purged of any distortion caused by the covariance of the residuals.

The resulting estimate of the causal effect of the mediator on the outcome was statistically significant and in the expected direction, $\hat{b}_{causal} = -.074$, $SE = .018$, $Z = -7.69$, $p < .001$, and the indirect effect of X on Y via M was also in the expected direction and significant, $\widehat{ab}_{causal} = -0.010$, $SE = .003$, $Z = 3.66$, $p < .001$. Hence, this analysis suggests that the causal mediation “survives” the impossible mediation test in the present study.

Table 3 raises two additional concerns about traditional mediation testing. First, conventional testing results (Column 1, Table 3) are consistent with the conclusion of “complete” mediation—that is, the c' path is not different from zero (Row 1). A researcher might therefore conclude that the mechanism of the causal influence is fully explained. However, this would be wrong. The estimate corrected for pre-manipulation confounding (Row 1, Column 7,

Table 3) shows a significant c' path, meaning that a portion of the effect of the manipulation on the outcome is unexplained.

Table 3. Parameter Estimates of Causal Pathways

Predictor	$\widehat{\text{Cov}}(\varepsilon_2, \varepsilon_3) = 0$, Outcome Measured Last		$\widehat{\text{Cov}}(\varepsilon_2, \varepsilon_3) = 0$, Outcome Measured First		$\widehat{\text{Cov}}(\varepsilon_2, \varepsilon_3)$ Estimated, Outcome Measured First		$\widehat{\text{Cov}}(\varepsilon_2, \varepsilon_3)$ Fixed at the Estimated Value, Outcome Measured Last	
	b	p	b	p	b	p	b	p
<u>Predicting Y</u>								
X (c' path)	.001	.920	.033	.003	.010	.376	-.024	.026
M (b path)	-.256	.000	-.195	.000	.000	-	-.074	.000
<u>Predicting M</u>								
X (a path)	.139	.000	.118	.000	.118	.000	.139	.000
Residual covariance: $\widehat{\text{Cov}}(\varepsilon_2, \varepsilon_3)$	.000		.000		-.021		-.021	
Mediation ($\widehat{ab}$)	-.036	.000	-.023	.000	-	-	-.010	.000

Note: Y is a latent variable consisting of two indicators of the outcome. $\widehat{\text{Cov}}(\varepsilon_2, \varepsilon_3)$ is the covariance between the residuals to M and Y.

Second, in the impossible mediation condition, conventional testing yielded a statistically apparent effect of the manipulation X on the outcome Y controlling for the mediator, even though that causal influence cannot have occurred (Row 1, Column 3, Table 3). This also raises doubts about advice that researchers can legitimately claim that a manipulation affected an outcome if a significant association between those variables appears only when controlling for a posited mediator (sometimes called a "suppression" effect; see MacKinnon & Fairchild, 2009; Rucker et al., 2011; Shrout & Bolger, 2002; Zhao et al., 2010).

Figure 2. Possible and impossible mediation for the ease of retrieval effect: X = Experimental manipulation, M = Mediator, Y = Outcome. Note: Numbers are standardized effect sizes.

Possible mediation indirect effect in Panel B removes the confounded variance estimated in Panel A. $N = 3,035$

Relaxing model assumptions with the Imai method. The Tingley et al. (2014)

sensitivity analysis suggests that the relation between the residuals to the mediator M and the outcome Y must be $r = .40$ or stronger in order for the true indirect causal effect to be truly zero. In fact, however, the correlation between the residuals to M and Y , estimated among participants in the impossible mediation condition, was only $r = .33$, which is smaller than .4 and thereby enhances the plausibility of causal influence of the manipulation on the outcome via the mediator. Thus, as above, the estimate of the correlation between the residuals did not undermine the conclusion of the causal influence.

The viability of at least some causal mediation is further supported through a direct comparison of data from participants who answered questions in the two orders, via moderated mediation analysis (Tingley et al., 2014). A moderated mediation analysis tests the null hypothesis that $\widehat{ab}_{naive:conventional order} - \widehat{ab}_{naive:impossible condition} = 0$. Since as noted (and

as shown in the Appendix) we have defined $\widehat{ab}_{causal}$ to be equal to this difference in indirect effects, then the moderated mediation tests the null hypothesis that $\widehat{ab}_{causal} = 0$. In the present case, the moderated mediation analysis rejected this null hypothesis, $p=.012$. Therefore the results are consistent with causal mediation that “survives” the impossible mediation test.

Although this moderated mediation approach is a sensible one, the structural equation modeling approach has one key advantage. The moderated mediation method compares estimates of the indirect causal pathway, which are computed using estimates of both the a and b paths. But the correlation between residuals—the primary focus of the present effort to reduce confounding—distorts only the estimate of the b path. Imagine that in our example, the Tingley et al. (2014) moderated mediation method fails to reject the null hypothesis that $\widehat{ab}_{causal} = 0$. That null result could occur because of differences between the two question-order groups in the estimated effect of the experimental manipulation X on the mediator M (a path) that occur simply due to chance alone and random assignment to condition. The structural equation modeling method, by contrast, estimates the confounded portion of the b path alone, so the comparison of mediation effects are less likely to be affected by random fluctuations in the strength of the a paths across conditions.

Discussion

Is a significant indirect effect evidence of mediation, or is it the result of confounded variance? A simulation study using the impossible mediation test (syntax and results posted at <https://osf.io/q3ums/>) shows that in a sample of $N=200$, if the residuals of a mediator and an outcome are correlated at .30, then a traditional mediation analysis will produce evidence consistent with significant mediation, even though mediation is impossible.

Knowing this, researchers interested in mechanisms can either discard statistical mediation analysis altogether (Bullock et al., 2010), conduct an experiment to manipulate the mediator (Imai, Keele, & Tingley, 2010; Pirlott & MacKinnon, 2016), or conduct a sensitivity analysis and make a principled argument about the plausibility of a confound (Imai & Yamamoto, 2013; Keele, 2015). Some mechanisms cannot be directly manipulated, and some scholars are not ready to give up on mediation analysis. Furthermore, it has proven difficult to evaluate sensitivity analysis results because, as Imai et al. (2010) stated, “there is no absolute threshold” (p. 316).

In that light, the impossible mediation test seems potentially useful. It is a study-specific method to quantify the plausibility of correlated residuals. The present illustration of the method shows that (a) traditional mediation analysis, using two common methods, can produce significant results seemingly consistent with mediation but incorrect; (b) correlated residuals can significantly inflate the apparent size of an indirect effect, but (c) causal mechanism claims can nevertheless be true when the impossible mediation condition’s indirect effect is not stronger than what would invalidate the claim of an indirect effect.

In future research using the impossible mediation test, power will be a concern. A simulation study we conducted suggests that sample sizes typical of many psychology studies (e.g., $N=200$) are unlikely to produce evidence of significant mediation after correcting for correlated residuals, even when such mediation exists (syntax and results posted at <https://osf.io/q3ums/>). If only 10% of the naïve b path is due to pre-manipulation confounding, then power to detect mediation with $N = 200$ is 80%. But as the percent of confounding in the naïve b path moves from 25% to 50% and 75%, power is 65%, 25%, and 5% when $N=200$,

respectively. In the real-world example shown here, confounding was 75%, thereby justifying the large sample size.

As noted, the present research is at odds with the recommendation to relax the need for a direct effect of X on Y in order to claim mediation, as some have proposed (MacKinnon & Fairchild, 2009; Rucker et al., 2011; Shrout & Bolger, 2002); (Baron & Kenny, 1986). The present illustrative example showed that what appear to be indirect effects can be significant and substantial, even when they are impossible.

The present proposal constitutes a small step toward enhancing causal mediation testing and has limitations. For example, the approach described here requires further refinement for application to cases of multiple mediators, multiple mediators that interact or cause one another, and more complex formulations (Imai & Yamamoto, 2013).

One might imagine that the impossible mediation test would be even more informative if in an experiment, the outcome is measured twice, once before the manipulation is implemented, and again after. In theory, the resulting data could be used to estimate the parameters of a model with a lagged effect of the outcome measured pre-manipulation on the outcome measured post-manipulation. That approach would be more efficient, because it would cut in half the number of participants from whom data must be collected. However, repeated measurement of the same variable on the same people during a short time interval could tip participants off to the hypothesis being tested in the study or create sensitivity to the outcome variable and perhaps cause inertia in it, reducing treatment effects. If researchers have no reason to suspect these problems are likely, future studies might compare the results obtained with the impossible mediation approach to results obtained from this alternative approach.

The impossible mediation test seems to represent an easy-to-implement method for enhancing the value of mediational testing. Reviewers and journal editors can ask for the test when there is reason to wonder about reverse- or joint-causality in the mediator-outcome relationship. We hope that such efforts will enhance the replicability and value of social science.

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SOM-R
Appendix: Justification for the Empirical Approach

Justification using DAGS

This appendix offers justifications for addressing correlated residuals in causal paths, adapting logic offered by Pearl (1995) and others (e.g., Morgan & Winship, 2014).

Consider the Directed Acyclic Graph (DAG; Pearl, 1995) in Fig. A1, which presents a causal relation without making assumptions about functional form.

Fig. A1. DAG representing *confounded* and *causal* mediation associations among participants among whom variables were measured in the traditional order. Note: Solid circles are observed variables, and empty circles are unobserved.

The $X \rightarrow M$ path and the $X \rightarrow Y$ path are not subject to correlated residuals, due to random assignment of participants to levels of X . Hence, those pathways (the a and c paths, respectively) can be viewed as documenting causal effects.

However, the $M \rightarrow Y$ path represents both the true causal effect of M on Y as well as other, unknown joint causes or correlates of M and Y , represented by the path $M \leftarrow \epsilon_2 \leftarrow U \rightarrow \epsilon_3 \rightarrow Y$. In Pearl's (1995) language, there is a "back door" path from M to Y via the correlated error terms. We therefore define b paths as below:

$$b_{causal} = M \rightarrow Y \quad (A1)$$

$$b_{confounded} = M \leftarrow \epsilon_2 \leftarrow U \rightarrow \epsilon_3 \rightarrow Y \quad (A2)$$

$$b_{naive:conventional\ order} = f(b_{causal}, b_{confounded}) \quad (A3)$$

The term $b_{naive:conventional\ order}$ is what might typically be estimated in a traditional mediation analysis when variables are measured in the conventional order. Clearly, that observed relation is a function both of the causal influence of interest and the correlated residuals.

Next, consider Fig. A2, which displays a DAG in which the *only* relation of M to Y is due to correlated residuals—i.e. where there is only a “back door” path. This would be true in an impossible mediation condition in which the outcome Y was measured before administration of the manipulation, X.

Fig. A2. DAG illustrating a spurious relation between variables in the impossible mediation condition. Note: Solid circles are observed variables, and empty circles are unobserved.

Fig. A2 shows that the path $X \rightarrow M$ is, as above, causal, but there is no causal path $X \rightarrow Y$, because Y was measured before X was manipulated. Moreover, there is no causal path $M \rightarrow Y$, because Y was measured before M.

Yet there is an unobserved confounder in the relation between M and Y, $M \leftarrow \epsilon_2 \leftarrow U \rightarrow \epsilon_3 \rightarrow Y$.

Therefore, estimating a naive b path using data from participants in the impossible mediation condition is equal to:

$$b_{naive:impossible} = M \leftarrow \epsilon_2 \leftarrow U \rightarrow \epsilon_3 \rightarrow Y = b_{confounded} \quad (A4)$$

This analysis leads to the following simple conclusion: Conditioning the values in equation A3 on the values in equation A4—that is, conditioning $b_{naive:conventional\ order}$ on $b_{naive:impossible}$ —leads to an estimate of b_{causal} .

Furthermore, this analysis shows that estimating the remaining “direct” effect of X on Y when controlling for M would be misleading—i.e., this approach constitutes “conditioning on a collider” (see Chapter 4, Morgan & Winship, 2014).

Justification for Using Linear Models

Next, we offer the same justification in the context of linear structural equation modeling. This exposition follows the same logic as traditional proofs for omitted variable bias in multiple regression models. We reproduce edited versions of proofs provided by Bullock, Green, and Ha (2010) (p. 551) and (Imai, Keele, & Yamamoto, 2010).

First, let us assume that the model in Fig. A1 is the true model. Next, assume that a researcher estimates a mediation effect by estimating the parameters of equations 2 and 3 in the paper:

$$M_i = \alpha_{2j} + a_2 X_i + \varepsilon_{i2} \quad (2)$$

$$Y_i = \alpha_{3j} + b_3 M_i + c_3 X_i + \varepsilon_{i3} \quad (3)$$

Mediation is typically defined as the product $a_2 \cdot b_3$ at expectation over the sample of individuals, i .

As shown in Fig. A1, only b_3 is subject to distortion due to omitted variables, and so we focus on omitted variable bias in b_3 . Fig. A1 and the paper (e.g. Fig. 1) define the error terms ε_2 and ε_3 as:

$$\varepsilon_{ij} = \lambda_j U_i + \varepsilon'_{ij} \quad (A5)$$

where j indicates equations 2 and 3, respectively, U is the unobserved omitted variable, λ_j is the regression coefficient for the omitted variable, and ε'_i are independent and normally distributed.

With these definitions, as is well-known, the formula for b_3 , which fails to account for the confound, is:

$$b_{naive:conventional\ order} = \frac{cov(M, Y|c_3 X)}{\sigma^2_M} \quad (A6)$$

such that $Y|c_3 X$ is equal to the residual score of Y conditional on (i.e. when accounting for) the remaining direct effect of X .

Next, it is possible to plug in $Y|c_3 X$ from equation 3 into equation A6:

$$b_{naive:conventional\ order} = \frac{cov(M, \alpha_3 + b_{causal} M + c_3 X + \varepsilon_3 | c_3 X)}{\sigma^2_M}$$

Multiplying out, we have these terms:

$$\begin{aligned} b_{naive} \cdot \sigma^2_M &= cov(M, \alpha_3) + &&= 0; \text{ constants do not vary} \\ &cov(M, b_{causal} M) + \\ &cov(M, c_3 X) + &&= 0; \text{ estimating variance } |c_3 X \\ &cov(M, \varepsilon_3) + \end{aligned}$$

Next, $cov(M, b_{causal} M) = b_{causal} cov(M, M) = b_{causal} \cdot \sigma^2_M$, and, cancelling, we see that:

$$b_{naive:conventional\ order} = b_{causal} + \frac{cov(M, \varepsilon_3)}{\sigma^2_M}$$

It is next possible to substitute equation 2 into the above, such that

$$b_{naive:conventional\ order} = b_{causal} + \frac{cov(\alpha_2 + a_2X + \varepsilon_2, \varepsilon_3)}{\sigma^2_M}$$

However, $cov(\alpha_2, \varepsilon_3) = 0$ because constants do not vary, and $cov(a_2X, \varepsilon_3) = 0$ due to random assignment of participants to levels of X . Therefore,

$$b_{naive:conventional\ order} = b_{causal} + \frac{cov(\varepsilon_2, \varepsilon_3)}{\sigma^2_M}$$

such that a naïve OLS estimate of b_3 is equal to the true b_{causal} plus a bias term, which is defined as the correlation of the error terms divided by the standard deviation of the mediator. From Fig. A1 and equation A5, we assume that the error terms could be correlated, because U is depicted as a cause of both error terms.

Next, Fig. A2 and equation A5 make it apparent that among participants in the impossible mediation condition, the effect of M on Y is *only* due to the correlation between residuals, via the influence of U . Thus, in the impossible mediation condition, $b_{causal} = 0$ by construction, because the experimental manipulation was administered after the outcome was measured. Hence, the impossible mediation condition involves this estimator:

$$b_{confounded} = \frac{cov(\varepsilon_2, \varepsilon_3)}{\sigma^2_M} \quad (A7)$$

σ^2_M is known, so only $cov(\varepsilon_2, \varepsilon_3)$ is needed in order to make the causal model identified. Thus, by these equations, we have the impossible mediation causal effect estimate used in the paper:

$$b_{causal} = b_{naive:conventional\ order} - b_{naive:impossible}$$

When should an analyst be worried that b_{causal} is actually no different from zero when accounting for $b_{confounded}$? The answer is: if either of the λ_j terms are non-zero and sufficiently large to explain $b_{naive:possible}$. This can be shown by plugging equation A5 into equation A7. Specifically,

$$b_{confounded} = \frac{cov(\varepsilon_2, \varepsilon_3)}{\sigma^2_M} = \frac{cov(\lambda_2 U + \varepsilon'_2, \lambda_3 U + \varepsilon'_3)}{\sigma^2_M}$$

where

$$\begin{aligned} cov(\lambda_2 U + \varepsilon'_2, \lambda_3 U + \varepsilon'_3) &= cov(\lambda_2 U, \lambda_3 U) + \\ &\quad cov(\lambda_2 U, \varepsilon'_3) + \end{aligned} \quad = 0; \text{ constants do not vary}$$

$$\begin{aligned}
& cov(\varepsilon'_2, \lambda_3 U) + & = 0; \text{ constants do not vary} \\
& cov(\varepsilon'_2, \varepsilon'_3) + & = 0; \text{ constants do not vary} \\
= & \lambda_2 \lambda_3 cov(U, U) = \lambda_2 \lambda_3 var(U) = \lambda_2 \lambda_3 \sigma^2_U
\end{aligned}$$

Thus, the bias term is:

$$b_{confounded} = \frac{\lambda_2 \lambda_3 \sigma^2_U}{\sigma^2_M} \quad (A8)$$

Equation A8 leads to the following conclusions. First, if the confound U has no association with the mediator (i.e., if $\lambda_2 = 0$), then the bias term $b_{confounded}$ is also zero. Next, if the confound U has no association with the outcome (i.e., if $\lambda_3 = 0$), then, again, the bias term $b_{confounded}$ is zero. Third, the strength of the confound is a function of the product of the two associations.

Of course, the unobserved confound U is by definition not measurable, so the λ_j parameters are theoretical. This is why Imai et al. (2010) recommended sensitivity analysis (to ascertain how large λ_j parameters must be in order to invalidate the evidence of causal impact). Supplementing this, we recommend that sometimes, it can be useful to estimate the magnitude of the correlation between residuals directly using equation A7, using data from an impossible mediation condition (see Fig. A2).