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Two-Condition Within-Participant Statistical Mediation Analysis: A Path-Analytic Framework

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Researchers interested in testing mediation often use designs where participants are measured on a dependent variable *Y* and a mediator *M* in both of 2 different circumstances. The dominant approach to assessing mediation in such a design, proposed by Judd, Kenny, and McClelland (2001), relies on a series of hypothesis tests about components of the mediation model and is not based on an estimate of or formal inference about the indirect effect. In this article we recast Judd et al.'s approach in the path-analytic framework that is now commonly used in between-participant mediation analysis. By so doing, it is apparent how to estimate the indirect effect of a within-participant manipulation on some outcome through a mediator as the product of paths of influence. This path-analytic approach eliminates the need for discrete hypothesis tests about components of the model to support a claim of mediation, as Judd et al.'s method requires, because it relies only on an inference about the product of paths—the indirect effect. We generalize methods of inference for the indirect effect widely used in between-participant designs to this within-participant version of mediation analysis, including bootstrap confidence intervals and Monte Carlo confidence intervals. Using this path-analytic approach, we extend the method to models with multiple mediators operating in parallel and serially and discuss the comparison of indirect effects in these more complex models. We offer macros and code for SPSS, SAS, and Mplus that conduct these analyses.

Keywords: mediation, indirect effect, path analysis, within-participant design, resampling methods

Statistical mediation analysis allows an investigator to answer questions about the process by which some presumed causal variable *X* operates to affect an outcome variable *Y*. Using simple principles of linear modeling (though other analytical approaches are possible; Imai, Keele, & Tingley, 2010; Pearl, 2010, 2012), mediation analysis is used to quantify and test the pathways of influence from *X* to *Y*. In a mediation process, one of those pathways consists of a sequence of causal steps in which *X* affects a *mediator* variable *M*, which in turn causally influences *Y*. This *indirect effect of X*—the conjunction of the effect of *X* on *M* and the effect of *M* on *Y*—quantifies the degree to which *M* acts as the "mechanism" by which *X* affects *Y*. An indirect effect that is different from zero by an inferential test is used to support (but by no means definitively establishes or proves) a claim of mediation of *X*'s effect on *Y* by *M*.

Mediation analysis is commonplace in the social sciences, business, medical research, and many other areas. For example, White, Abu-Rayya, Bliuc, and Faulkner (2015) investigated how long-term interaction with a member of the same religion or a different religion (*X*) influenced intergroup bias (*Y*) through five different emotions (e.g., anger and sadness; *M*). Littleton (2015) found that pregnant women who had a history of sexual victimization (*X*) had higher rates of depression (*M*), which predicted increased somatic complaints (e.g., back pain; *Y*). Schuldt, Guillory, and Gay (2016) examined how the weight of a person recommending a recipe (*X*) influenced the perceived healthiness of the recipe (*Y*) through the perceived health of the recommender (*M*).

Discussions of mediation analysis and its application are most typically couched in terms of or conducted using data from research designs that are cross-sectional or "between-participant" in nature. Typically in these designs, participants are measured once on a proposed mediator *M* and dependent variable *Y*, as in the examples above. This may occur following random assignment of participants into one of two conditions (*X*) that vary via some manipulation (e.g., a "treatment" vs. a "control" group) that is presumed to cause differences in *M* and *Y*. Alternatively, measurement of *M* and *Y* may occur contemporaneously with the observation of *X* (rather than random assignment). For expositional convenience, we refer to designs of this sort (i.e., with or without random assignment to *X*) throughout this article as "between-participant" designs.

Less attention in the methodology literature has been dedicated to mediation analysis when the data come from repeated measurement of the same people on variables in the mediation process, even though such designs are common. In this article we address mediation analysis in a specific category of repeated measures designs. Researchers sometimes measure a dependent variable *Y* and a mediator *M* in two different situations or circumstances (*X*),

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with the same participants measured in *each* of those situations or circumstances on *M* and *Y*. Those situations may be manipulated by the experimenter, or they may represent the mere passage of time. In this design participants receive *X* in both forms. For example, Putwain and Best (2011), measured participants' math test scores (*Y*) and test anxiety (*M*) twice: once under high threat from the teacher and once under low threat from the teacher (*X*). They found that the effect of threat on test scores was mediated by increased test anxiety. Alternatively, *M* and *Y* can be measured pre- and post-intervention, with all participants experiencing the *same* intervention. In this variant, *X* is the mere passage of time, with change in *M* and *Y* presumed to be influenced by an intervention or some other event between the two time points. Examples include Converse and Fishbach (2012); Kearney et al. (2013); and Kearney, McDermott, Malte, Martinez, and Simpson (2012). Studies with participants measured in two circumstances and studies with participants measured pre- and post-intervention have similar data structures with participants experiencing both possible levels of *X* and having two measures of both *M* and *Y*. We will refer to such designs as "two-condition within-participant" designs in this article. Excluded from this label are those designs where *X* is measured over time on the same participants in addition to *M* and *Y*.

There are forms of repeated-measures designs that we will not address in this article. In one form, *X*, *M*, and *Y* are each measured over two or more occasions on the same participants, and the goal is to assess whether change in *X* brings about a change in *Y* through a change in *M*. Alternatively, *X* may be measured once between participants, with only *M* and *Y* measured repeatedly. Treatments of mediation in such designs can be found in Cheong, MacKinnon, and Khoo (2003); Cole and Maxwell (2003); Little, Preacher, Selig, and Card (2007); MacKinnon (2008), and Selig and Preacher (2009).

Judd, Kenny, and McClelland (2001), published in this journal, offers an approach to testing mediation in designs where the same individuals are measured on *M* and *Y* in *each* of two situations. Their approach, which we discuss in detail later, has become widely used and applied in research published in a variety of journals, including *Psychological Science* (e.g., Converse & Fishbach, 2012; Paladino, Mazzurega, Pavani, & Schubert, 2010), the *Journal of Personality and Social Psychology* (e.g., Cheryan, Plaut, Davies, & Steele, 2009; Grant & Gino, 2010), the *Journal of Consumer Research* (e.g., Spiller, 2011; Warren & Campbell, 2014), and *Organizational Behavior and Human Decision Processes* (e.g., De Kwaadsteniet, Rijkhoff, & van Dijk, 2013; Morris, Sheldon, Ames, & Young, 2007), among many others. The appeal of their approach is its similarity to a between-participant counterpart described by Baron and Kenny (1986), characterized by its reliance on a set of hypothesis tests and clearly defined criteria that a researcher needs to meet to claim that *M* is a mediator of the effect of *X* on *Y*. Furthermore, Judd et al.'s method is simple to conduct and can be undertaken with little statistical background other than an understanding of the dependent groups *t* test and some familiarity with multiple regression analysis. As a result, it has become the standard method used by researchers interested in questions about mediation in two-condition within-participant designs.

Recent developments in the methodology literature improve upon the causal steps logic popularized by Baron and Kenny (1986) in two important ways. First, mediation analysis is now conceptualized in a path-analytic framework rather than a set of discrete hypothesis tests about individual paths in the model. The result is a shift toward estimating the two pathways of influence from *X* to *Y*, one direct and one indirect (Hayes, 2013; Preacher & Hayes, 2004), rather than putting together a logical argument about the process based on a pattern of significant and non-significant effects. As a consequence, mediation is now assessed by a single test of the indirect effect rather than multiple tests, one for each step in the chain of effects linking *X* to *Y* (Hayes & Scharkow, 2013; MacKinnon, Fritz, Williams, & Lockwood, 2007; Preacher & Selig, 2012). This is important because all tests are fallible: By conducting multiple tests, the probability of at least one error occurring increases. The fewer tests you conduct to support a claim, the less likely you are to make an inferential error in the claim itself (cf. Hayes, 2013, p. 168; Hayes, 2015).

Second, a path-analytic framework facilitates the testing of mediation questions in more complicated models. For example, in models with more than one mediator, it is possible to empirically compare indirect effects through different mediators (MacKinnon, 2000; MacKinnon, 2008; Preacher & Hayes, 2008). Different theories may propose different mediators. By comparing indirect effects representing different explanations of the process, theories can be better tested against each other. When cast in terms of the parameters of a path analysis, the indirect effect can also be modeled as a function of another variable, allowing researchers to test how indirect effects depend on other variables (moderated mediation; see, e.g., Edwards & Lambert, 2007; Hayes, 2015; Preacher, Rucker, & Hayes, 2007). By modeling the contingencies of an indirect effect, we are modeling the boundary conditions of a process. Establishing the boundary conditions of an effect is just as important as establishing whether an effect exists.

In this article, we first offer a brief overview of the mechanics and mathematics of between-participant mediation analysis before outlining the method described by Judd et al. (2001) for two-condition within-participant designs. Our primary contribution to this literature stems from our translation of the mathematics of their method into a path-analytic form consistent with how mediation analysis is understood and conducted now in between-participant designs. This approach yields a formal estimate of the indirect effect, eliminates the need for discrete hypothesis tests of individual components of the process being modeled, and opens the door for extensions to more complicated models. With the indirect effect formally estimated, we then describe how inference can be conducted in a manner equivalent to how inference proceeds in between-participant designs. Following this, we discuss and illustrate implementation using structural equation modeling software, as well as macros for SPSS and SAS that greatly simplify the analysis. Finally, we extend this path-analytic approach to models with more than one mediator, including a treatment of the comparison of indirect effects through different mediators. We save a treatment of moderation of mediation in this design to another paper.

Working Example

The example we use throughout this article relies on data from Study 1 of Dohle and Siegrist (2014). The participants were asked to make judgments of fictitious drugs with simple (e.g., Fastinorbine) or complex (e.g., Cytrigmcmium) names. Following the

presentation of the drug name, participants judged how hazardous they perceived the drug to be, and how willing they would be to buy the drug. Judgments were made on a 1-to-7 scale, with a higher judgment reflecting a higher perceived hazardousness and a greater willingness to buy the drug. Motivated by fluency theory, Dohle and Siegrist (2014) reasoned that participants would have an aversive reaction to drugs with complex names given the difficulty of processing the name. This reaction would include perceiving the complex drugs as hazardous, and this would in turn lead to a reduced willingness to buy the complex drug relative to drugs with simple names. Thus, they proposed that the effect of the complexity of the drug name on willingness to buy would operate through the mechanism of perceived hazardousness.

We present the data in two different forms to enable us to show the parallels between methods used for between-participant and within-participant designs. Table 1 contains the data from the study as actually conducted in within-participant design form, where 22 participants made judgments of drugs with simple *and* complex names. Participants rated five simple and five complex drug names, and the data presented in the tables are the average judgment of hazardousness and willingness to buy in each drug name condition. In Table 2 we present the same data in a hypothetical between-participant form. The data would take such form if participants were instead randomly assigned to either a simple or complex drug name condition, and then made judgments for *only* the drugs in the condition to which they were assigned. In this form, there are 44 participants with each participant having their own unique identifier (ID). Condition is also identified with the variable *X*, coded 1 for the simple drug condition and 2 for the complex drug condition.

Two important features of the data in these tables are worth pointing out. First, the data in each of the rows are identical in the two tables. That is, reading across each row in Tables 1 and 2, the numbers are the same. Because each participant evaluated both simple and complex drug names in the within-participant version of the study, however, there is no need for a variable coding condition, so *X* is absent from Table 1. Second, within each row in Table 2 there are two participants, one who assessed the simple drug names (in the left hand columns) and one who assessed the complex drug names (in the right hand columns). The observations within each row in Table 2 are independent because they come from different participants. In Table 1, by contrast, the left and right hand columns within a row contain the same data as in Table 2 but they reflect measurements of *M* (hazardousness) and *Y* (willingness to buy) from the *same* person. That is, whereas *M* and *Y* for Participants 1 and 23 in Table 2 reflect measurements from two different people assigned to different conditions, in Table 1 the two measures of *M* and *Y* come from repeated measurements of the same person in each condition. This is true for all rows.

Table 1
Data From Study 1 of Dohle and Siegrist (2014)

	Simple names		Complex names	
ID	M_1	Y_1	M_2	Y_2
1	3.8	4.4	4.4	3.6
2	4.2	4.2	5.2	2.0
3	4.0	4.0	4.0	4.0
4	4.4	3.0	3.0	5.2
5	2.2	5.2	4.2	3.6
6	3.2	4.8	4.2	3.6
7	6.6	1.0	7.0	1.0
8	2.8	4.6	4.8	3.8
9	3.4	4.6	4.8	3.2
10	3.2	4.6	4.6	3.8
11	4.2	4.8	4.0	5.0
12	4.4	2.8	5.6	1.6
13	3.6	2.0	4.8	1.2
14	7.0	1.0	5.4	1.0
15	4.0	4.0	4.0	4.0
16	3.8	4.2	3.8	4.2
17	4.0	3.0	4.4	2.6
18	2.6	5.4	5.0	4.4
19	2.6	5.0	5.8	3.8
20	4.0	4.0	4.0	4.0
21	4.0	4.0	4.0	4.0
22	3.2	4.2	5.8	2.8

Note. M = hazardousness; Y = willingness to buy.

Table 2
Data From a Hypothetical Between-Participant Study Based on Study 1 of Dohle and Siegrist (2014)

	Simple names				Complex names		
ID	*X*	*M*	*Y*	ID	*X*	*M*	*Y*
1	1	3.8	4.4	23	2	4.4	3.6
2	1	4.2	4.2	24	2	5.2	2.0
3	1	4.0	4.0	25	2	4.0	4.0
4	1	4.4	3.0	26	2	3.0	5.2
5	1	2.2	5.2	27	2	4.2	3.6
6	1	3.2	4.8	28	2	4.2	3.6
7	1	6.6	1.0	29	2	7.0	1.0
8	1	2.8	4.6	30	2	4.8	3.8
9	1	3.4	4.6	31	2	4.8	3.2
10	1	3.2	4.6	32	2	4.6	3.8
11	1	4.2	4.8	33	2	4.0	5.0
12	1	4.4	2.8	34	2	5.6	1.6
13	1	3.6	2.0	35	2	4.8	1.2
14	1	7.0	1.0	36	2	5.4	1.0
15	1	4.0	4.0	37	2	4.0	4.0
16	1	3.8	4.2	38	2	3.8	4.2
17	1	4.0	3.0	39	2	4.4	2.6
18	1	2.6	5.4	40	2	5.0	4.4
19	1	2.6	5.0	41	2	5.8	3.8
20	1	4.0	4.0	42	2	4.0	4.0
21	1	4.0	4.0	43	2	4.0	4.0
22	1	3.2	4.2	44	2	5.8	2.8

Note. X = condition (1 = simple, 2 = complex); M = hazardousness; Y = willingness to buy.

Mediation Analysis in the Two-Condition Between-Participant Design

The primary goal of statistical mediation analysis is to estimate the pathways of influence from *X* to *Y*, one that operates *through* a mediator *M*—the indirect effect of *X*—and the other *bypassing M*—the direct effect of *X*. A simple mediation model for a between-participant design in path-diagram form can be found in Figure 1. This diagram represents three linear equations that can be used to estimate various components of the process, assuming *M* and *Y* are modeled as continuous outcomes. Only the first two are necessary because *c* in Equation 3 can be derived from *a*, *b*, and *c′* in Equations 1 and 2:

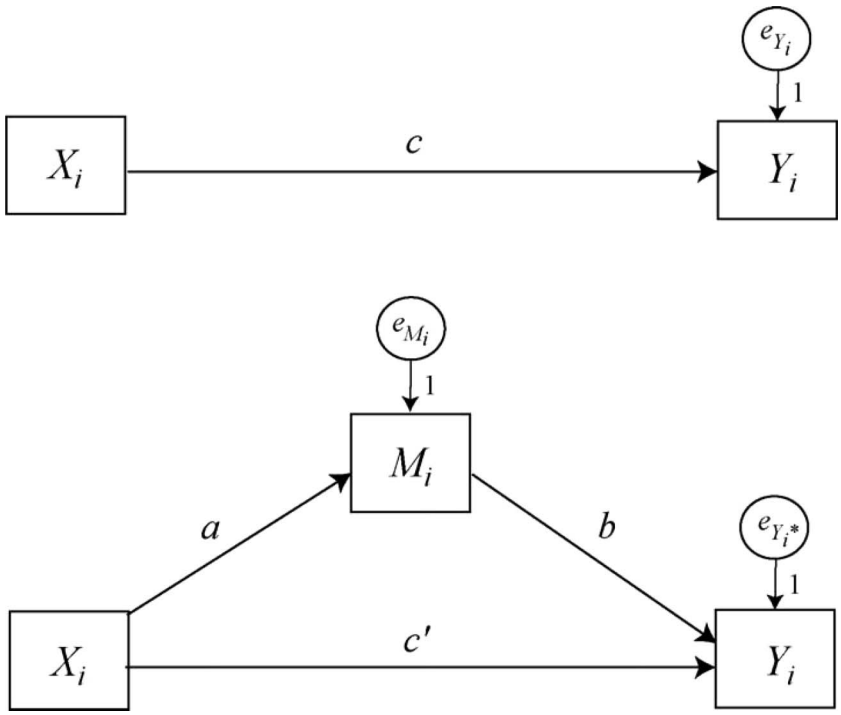

Figure 1. A between-participant simple mediation model in path diagram form.

$$M_i = a_0 + aX_i + e_{M_i} \quad (1)$$

$$Y_i = c'_0 + c'X_i + bM_i + e_{Y_i^*} \quad (2)$$

$$Y_i = c_0 + cX_i + e_{Y_i} \quad (3)$$

where a_0, c'_0, and c_0 are regression intercepts and e denotes the error in estimation. The star superscript indicates that $e_{Y_i^*}$ and e_{Y_i} are not the same. We denote case or observation number with an i subscript. The intercepts and coefficients a, b, c, and c' in these equations are often estimated using separate ordinary least squares regressions, but simultaneous estimation with a structural equation modeling program using maximum likelihood can also be used and will generally produce the same results. Throughout the manuscript we use "hat" notation when denoting sample-based estimates of specific population parameters (e.g., $\hat{a}$, $\hat{b}$, $\hat{c}$, and so forth).

The *indirect effect* of X is defined as the product of a from Equation 1 and b from Equation 2. It quantifies how much two cases that differ by one unit on X are expected to differ on Y as a result of a chain of events in which X causes M which in turn causes Y. This chain translates into an expected difference of ab units in Y with a one unit difference in X. A statistical test or confidence interval (CI) for the indirect effect, ab, provides an inference as to whether and to what degree M is functioning as a mediator of the effect of X on Y. The *direct effect*, c' in Equation 2, is the component of the effect of X on Y that does not operate through M. That is, c' quantifies how much two cases that differ by one unit on X but are equal on M are estimated to differ on Y. These two components of X's effect, ab and c', sum to give c, the total effect of X. The total effect c can also be estimated by regressing Y on X alone (Equation 3).

Applying this method to the hypothetical between-participant version of the drug name study using the data in Table 2, drugs with complex sounding names were perceived as $\hat{a} = 0.800$ units more hazardous on average than drugs with simpler names, $t(42) = 2.618$, $p = .012$, 95% CI [0.183, 1.417]. Additionally, holding complexity of the name constant, every one-unit difference in perceived hazardousness was associated with a $\hat{b} = -0.961$ difference in willingness to buy, $t(41) = -8.241$, $p < .0001$, 95% CI [−1.196, −0.725]. So the more hazardous a drug was perceived, the lower the willingness to buy. Therefore, the estimated indirect effect of drug name complexity on willingness to buy through hazardousness is $\hat{a}\hat{b} = (.800)(–.961) = -0.769$. This means that people rated themselves as 0.769 points less likely to buy more complex-sounding drugs relative to simple-sounding drugs as a result of the effect of name complexity on perceived hazardousness which in turn *lowered* willingness to buy (hence the negative indirect effect). The direct component of the effect of name complexity on willingness to buy was $\hat{c}' = 0.205$, $t(41) = 0.823$, $p = .415$, 95% CI [−0.298, 0.708]. This means that among a hypothetical group of people equal in how hazardous they perceive the drugs, those exposed to complex sounding drug names are estimated to be 0.205 units more willing to buy the drug than those exposed to the simpler sounding drugs. Adding the direct ($\hat{c}' = 0.205$) and indirect effect ($\hat{a}\hat{b} = -0.769$) of drug name complexity on willingness to buy yields the estimated total effect of name complexity, $\hat{c} = -0.564$, $t(42) = -1.516$, $p = .137$, 95% CI [−1.314, 0.187]. There is an overall mean difference of −0.564 units between the complex and simple sounding drugs, with participants less willing to buy the more complex drugs. These interpretations as mean differences are justified because the two

groups are coded with values of X that differ by one unit (i.e., $X = 1$ and 2).

Prior to the turn of the century, inference about a mediation process in a statistical mediation analysis was typically undertaken almost exclusively using the causal steps approach popularized by Baron and Kenny (1986). This method focuses on the outcome of the hypothesis tests for individual paths in the models that define the total effect (path c), the indirect effect (paths a and b), and the direct effect (path c'). By this traditional approach, if $\hat{c}$, $\hat{a}$ and $\hat{b}$ are all statistically different from zero, then mediation is established (of course, only insofar as design and theory warrant a causal claim). If the first three null hypotheses are rejected, a follow up hypothesis test on the direct effect ($\hat{c}'$) determines the degree to which X's effect is mediated by M. If $\hat{c}'$ is closer to zero than $\hat{c}$ and not statistically different from zero, this is considered "complete" mediation. If $\hat{c}'$ is statistically different from zero but smaller than $\hat{c}$ this is considered "partial" mediation.

Though popular, this method has fallen out of favor in the last decade, with greater emphasis placed on inference about the indirect effect ab rather than its constituent components a and b. Additionally, experts on mediation analysis agree that there is no need to require that $\hat{c}$ be statistically different from zero before one can claim mediation (Cerin & MacKinnon, 2009; Hayes, 2009, 2013; MacKinnon, 2008; Rucker, Preacher, Tormala, & Petty, 2011; Zhao, Lynch, & Chen, 2010). Accurate inferences about the indirect effect rely on methods that acknowledge the nonnormality of the sampling distribution of $\hat{a}\hat{b}$, with the bootstrap confidence interval being one of the more frequently recommended approaches (Biesanz, Falk, & Savalei, 2010; Hayes & Scharkow, 2013; MacKinnon, Lockwood, & Williams, 2004; Shrout & Bolger, 2002; Williams & MacKinnon, 2008).[1]

A 95% confidence interval for the indirect effect in the drug-complexity example, using the percentile bootstrap method with 10,000 bootstrap samples, is −1.316 to −0.173. We can rule zero out from the realm of plausible values of the indirect effect. Although bootstrap methods could be used for inference about the direct effect too, typically users rely on a hypothesis test or confidence interval using traditional approaches, because the direct effect is known to be normally distributed under the assumptions of linear regression. In this case, as noted above the direct effect is not statistically different from zero. In summary, this mediation analysis suggests that drug name complexity indirectly reduces willingness to buy through its positive effect on perceived hazardousness, which in turn lowers willingness to buy the drug. The evidence does not support the conclusion that drug name complexity operates independently of this mechanism to affect willingness to buy the drug.

Mediation Analysis in the Two-Condition Within-Participant Design

We now address mediation analysis using the data from the within-participant design version of the study (see Table 1). In this version, there were only 22 participants rather than 44, and each participant judged drugs with both simple and complex names. Judd et al. (2001) offer the only systematic treatment of mediation analysis in this type of design. In the data in Table 1, there is no X variable to regress Y and M on, and also two values of M and Y, so their approach does not follow the traditional path-analytic form outlined above, at least not explicitly. Later we make the connection between their approach and a path-analytic approach explicit—one of the main contributions of this article.

Judd et al.'s (2001) approach is similar to the Baron and Kenny causal steps procedure in its logic, while properly estimating standard errors and confidence intervals by acknowledging the repeated measurements of M and Y. As in Baron and Kenny (1986), Judd et al. (2001) require that one first establish whether there is a statistically significant difference in Y between the two conditions. That is, is there an effect of condition that can be mediated? This is accomplished by a dependent means t test comparing the two condition means on Y, or, equivalently, a one-sample t test using $Y_{2i} - Y_{1i}$ computed for each participant as the dependent variable, where Y_{2i} is participant i's rating of willingness to buy the complex drugs and Y_{1i} is participant i's rating of willingness to buy the simple drugs. In these data, participants were 0.564 units less willing to buy drugs with complex-sounding names than simple-sounding names, $t(21) = -2.917$, $p = .008$, 95% CI [−0.966, −0.162], thereby meeting the first criterion of mediation outlined by Judd et al. (2001).

Given evidence of an effect of drug name complexity, the next step in the Judd et al. (2001) procedure is to examine whether there is a difference between drug name conditions on the mediator. This is also accomplished using a dependent means t test comparing the means of M in the two conditions, or a one-sample t test for the mean difference in M calculated as $M_{2i} - M_{1i}$ for each participant, where M_{2i} is participant i's rating of the hazardousness of the complex drugs and M_{1i} is participant i's rating of the hazardousness of the simple drugs. In this case, the mean difference in perceived hazardousness is statistically significant. Drugs with more complex names were perceived as 0.800 units more hazardous on average, $t(21) = 3.102$, $p = .005$, 95% CI [0.264, 1.336]. This satisfies the second criterion.

The third stage of the Judd et al. (2001) causal steps approach asks whether there is evidence that the difference in M affects the difference in Y. In a two-condition within-participant design, there is a measure of the effect of M on Y in each condition. Judd et al. (2001) begin their discussion of how to estimate the effect of the difference in M on the difference in Y by formalizing two models of Y, one for each condition:

$$Y_{1i} = g_{10} + g_{11}M_{1i} + e_{Y_{1i}} \tag{4}$$

$$Y_{2i} = g_{20} + g_{21}M_{2i} + e_{Y_{2i}} \tag{5}$$

In these models, each outcome variable, Y_1 and Y_2, is estimated from the mediator measured in the same condition, M_1 and M_2, respectively. Judd et al. (2001, page 123) subtract Equation 4 from Equation 5 to get Equation 6:

$$Y_{2i} - Y_{1i} = g_{20} - g_{10} + g_{21}M_{2i} - g_{11}M_{1i} + e_{Y_{2i}} - e_{Y_{1i}}. \tag{6}$$

They note that

$$g_{21}M_{2i} - g_{11}M_{1i}$$

can be expressed in an equivalent form as

[1] There is a debate in the literature as to whether a percentile or a bias-corrected bootstrap confidence interval is superior, with the later sometimes showing higher Type I error in certain circumstances. Throughout this article, we will use only percentile bootstrap confidence intervals.

$$\frac{g_{21}+g_{11}}{2}(M_{2i}-M_{1i})+\frac{g_{21}-g_{11}}{2}(M_{2i}+M_{1i}),$$

which a little algebra verifies:

$$\begin{aligned}&\frac{g_{21}+g_{11}}{2}(M_{2i}-M_{1i})+\frac{g_{21}-g_{11}}{2}(M_{2i}+M_{1i})\\&=\frac{1}{2}(g_{21}M_{2i}+g_{11}M_{2i}-g_{21}M_{1i}-g_{11}M_{1i}\\&\quad+g_{21}M_{1i}-g_{11}M_{1i}+g_{21}M_{2i}-g_{11}M_{2i})\\&=\frac{1}{2}(2g_{21}M_{2i}-2g_{11}M_{1i})\\&=g_{21}M_{2i}-g_{11}M_{1i}\end{aligned}$$

So by substitution, Equation 6 can be written as

$$Y_{2i}-Y_{1i}=h+b_1(M_{2i}-M_{1i})+b_2(M_{1i}+M_{2i})+e_{Y_i}, \quad (7)$$

where $h=g_{20}-g_{10}$, $b_1=\frac{g_{21}+g_{11}}{2}$, $b_2=\frac{g_{21}-g_{11}}{2}$, and $e_{Y_i}=e_{Y_{2i}}-e_{Y_{1i}}$. The regression coefficient b_1 is the average effect of M on Y across the two conditions but also can be interpreted as the effect of the difference between the two M measurements on the difference between the two Y measurements.

But Equation 7 is not the model that Judd et al. settle on. Rather, they center $(M_{1i}+M_{2i})$ around its sample mean

$$\frac{1}{n}\sum_{i=1}^{n}(M_{1i}+M_{2i})=\overline{M_1+M_2}$$

prior to estimation, which results in

$$\begin{aligned}Y_{2i}-Y_{1i}=c'+\ &b_1(M_{2i}-M_{1i})\\&+b_2[(M_{1i}+M_{2i})-(\overline{M_1+M_2})]+e_{Y_i}. \quad (8)\end{aligned}$$

This centering has no effect on b_1 or b_2. However, it renders c' interpretable as the average difference between the conditions on Y that remains after accounting for the difference between the conditions on M. As we discuss later, this is equivalent to the direct effect of X. A little algebra shows that Equations 7 and 8 are same by defining c' as $h+b_2(\overline{M_1+M_2})$.

The third causal steps criterion is met—evidence of a relationship between the difference in M and the difference in Y—if $\hat{b}_1$ is statistically different from zero. In the drug names study, $\hat{b}_1=-0.598$, $t(19)=-5.287$, $p<.001$, 95% CI [−0.835, −0.361], thereby satisfying the third criterion. The negative regression coefficient reflects that as drugs with complex names are perceived as more hazardous relative to those with simple names, participants are less willing to buy the complex sounding drugs relative to the simple sounding drugs.

In this example, $\hat{c}'=-0.085$, $t(19)=-0.540$, $p=.595$, 95% CI [−0.415, 0.245]. So there is no statistically significant difference in willingness to buy drugs with complex relative to simple names after accounting for the effect of perceived hazardousness on willingness to buy. Judd et al. (2001) would characterize this as consistent with "complete mediation" of the effect of the drug name complexity on willingness to buy. A regression constant that is statistically different from zero would lead to a claim of "partial mediation."

It is worth making an important point about Equations 7 and 8. An investigator might be tempted to estimate the difference in the two Y measurement using Equation 7 but without $(M_{1i}+M_{2i})$ in the model. Doing so is equivalent to fixing b_2 in Equations 7 and 8 to zero, which itself is equivalent to assuming that M has the same effect on Y in each condition because $b_2=\frac{g_{21}-g_{11}}{2}$. But this assumption need not be made, and we do not recommend making this assumption, at least not without evidence. Evidence consistent with this assumption would be found in an estimate of b_2 that is not statistically different from zero. But a null hypothesis can never be proven true, so keeping $(M_{1i}+M_{2i})$ in the model of the difference in Y is safer than assuming $b_2=0$ and thereby possibly biasing the estimation of b_1.

Observations and Limitations

The procedure outlined by Judd et al. (2001) maps on perfectly to the popular causal-steps logic that dominated mediation analysis until a bit after the turn of the century. Each of these conditions directly corresponds to the criteria Baron and Kenny (1986) describe in the context of between-participant mediation analysis. But the logic of this "criteria-to-establish-mediation" manner of thinking about mediation analysis in the between-participant case has come under fire. Criticisms lodged at the approach popularized by Baron and Kenny in between-participant mediation analysis (e.g., Hayes, 2009, 2013; LeBreton, Wu, & Bing, 2009; MacKinnon, Lockwood, Hoffman, West, & Sheets, 2002; Zhao, Lynch, & Chen, 2010), apply to Judd et al. (2001) as well. Specifically, inference about the indirect effect does not rely on explicit quantification of the indirect effect, inference about mediation is undertaken with less power than alternative methods, it relies on evidence of a total effect of X on Y prior to attempting to establish mediation, and extending it to models with more than one mediator is, at best, awkward and inelegant. As noted earlier, the requirement that one must establish evidence of an effect to be mediated has largely been discarded by methodologists who write about mediation analysis. Additionally, this procedure uses three hypothesis tests to establish evidence of mediation, which makes it vulnerable to error given that hypothesis tests are fallible: the more tests one must conduct, the greater the likelihood of making a mistake in inference along the way. Rather, the current zeitgeist in statistical mediation analysis focuses on a single estimate of the indirect effect (i.e., how much X influences Y through M) and inference about its size rather than relying on inferences about its constituent components (i.e., $X\rightarrow M$ and $M\rightarrow Y$). Methods that are now routine in mediation analyses of between-participant designs provide both a point and an interval estimate of the indirect effect rather than the qualitative "complete," "partial," or "no" mediation interpretation, concepts that have also come under fire (Hayes, 2009, 2013; Rucker et al., 2011). The difference between complete and partial mediation is sample-size dependent. Investigators with less data are more likely to claim complete mediation rather than partial mediation relative to investigators with more data because power to detect a nonzero direct effect will be lower in smaller sample sizes. Thus, we do not recommend describing results using these terms.

A reading of Judd et al. (2001) does not make it obvious how to generate a point and interval estimate of the indirect effect. In addition, it is not clear how their form of mediation analysis can be applied to models with multiple mediators. Multiple mediator models can better represent how causal effects operate (often through more than one mechanism simultaneously), and they are particularly useful when one is interested in comparing the relative

magnitudes of indirect effects through different mediators in more complex models, as is often needed when doing comparative theory testing (as discussed by, e.g., Hayes, 2013; MacKinnon, 2000; Preacher & Hayes, 2008, in the between-participant mediation context). In the rest of this article, we address these issues after first translating Judd et al. (2001) into a more familiar path-analysis format.

Judd et al. (2001) in a Path-Analytic Framework

The Judd et al. (2001) causal steps approach should feel both foreign and familiar to researchers who use or read the empirical literature on mediation analysis. On the one hand, the causal steps approach for between and within-participant designs can be found in practice in journal articles both historically and more recently. On the other hand, nowhere in Judd et al. (2001) is a path diagram found, and rarely will one find the results of its application described using the familiar "mediation triangle" as in Figure 1. Researchers familiar with methods of testing mediation in between-participant designs might expect to setup a regression model predicting Y from both X and M. However, with the data set up as they are in Table 1 there is no X and there are two Ms and two Ys.

But Judd et al.'s approach can be conceptualized in the path-analytic framework that characterizes the description, discussion, and reporting of mediation analysis in its ubiquitous between-participant form. Putting two-condition within-participant mediation analysis for this kind of design in this form makes it explicit and obvious that the indirect effect can be constructed as a product of pathways of influence. It also makes it clearer how inference about the size of the indirect effect can be conducted without relying on the piecemeal causal steps logic. Once set up in this form, it is also fairly transparent how the model can be extended to models with multiple mediators operating in parallel or in serial, as we do later. It also opens the door to opportunities such as establishing moderation of the indirect effect, though that is beyond the scope of this article.

Figure 2 contains the mathematics of Judd et al. (2001) in the form of a path diagram, which represents a set of equations for the various effects of one variable on another:

$$M_{2i} - M_{1i} = a + e_{M_i} \tag{9}$$

$$Y_{2i} - Y_{1i} = c' + b(M_{2i} - M_{1i}) + d[0.5(M_{1i} + M_{2i}) - 0.5\overline{(M_1 + M_2)}] + e_{Y_i^*} \tag{10}$$

$$Y_{2i} - Y_{1i} = c + e_{Y_i}. \tag{11}$$

As X does not actually exist in the data (as represented in Table 1 for the specific example we are using), X is absent from these equations. The effect of X is carried in the difference scores. Given these equations are models of the differences in M and the difference in Y between the two conditions, they are really models of the effect of X. More specifically, as will be seen, all but one of the effects of X are quantified by the regression constants in these equations, which are often denoted with a triangle in a path diagram in the structural equation modeling literature (Curran & Bauer, 2007), as we use in Figure 2. We have also replaced the sum of the mediators in Judd et al. (2001) with their average,[2] because we feel this parameterization is more interpretable. The regression coefficient for the sum in Judd et al. (2001) captures moderation of the direct effect of X on Y as a function of individual difference on M and individual differences are often measured with averages rather than sums.

The three effects of X in this path diagram (c, c', and ab) correspond to their counterparts in a between-participant path diagram as in Figure 1. The total effect of X on Y is c from Equation 11, the direct effect of X on Y is c' from Equation 10, and the indirect effect of X on Y through M is the product of a from Equation 9 and b from Equation 10. The usual path analysis algebra holds: $c = c' + ab$. That is, the total effect partitions into direct and indirect components. We illustrate this next and also discuss inference.

Estimation and Inference

The direct, indirect, and total effects of X can be estimated in a piecemeal fashion individually by estimating the coefficients in Equations 9, 10, and 11 using an ordinary least squares (OLS) regression routine, or they can be estimated simultaneously using a structural equation modeling program such as Mplus or LISREL. In Judd et al. (2001), their regression for the difference in Y (our Equation 8) generates what we label b and c' in Equation 10, along with estimated standard errors that can be used for hypothesis testing or confidence interval construction in the usual way. Estimation of these paths for the drug names study data in Table 1 using OLS regression yields, $\hat{b} = -0.598$, $\widehat{se}_{\hat{b}} = 0.113$, $\hat{c}' = -0.085$, $\widehat{se}_{\hat{c}'} = 0.158$.

Because most OLS regression routines do not allow constant-only regression (i.e., typically at least one predictor variable is required), paths a and c can instead be estimated as the mean difference in M and Y, respectively, as discussed by Judd et al. (2001). A one-sample t test on these differences yields standard error estimates, p values, and confidence intervals for these effects. These paths have already been reported above for the drug names study as $\hat{a} = 0.800$, $\hat{c} = -0.564$, with estimated standard errors of 0.258 and 0.193, respectively.

Using the estimates of a and b, their product is easily constructed by hand to produce a point estimate of the indirect effect. In the drug name study, $\hat{a}\hat{b} = 0.800(-0.598) = -0.478$, meaning that participants are 0.478 units *less* willing to buy drugs with complex names relative to simple names through the process of drug name complexity's influence on perceived hazardousness. This results from the drugs with complex names being perceived as 0.800 units more hazardous which, according to the process theorized, in turn lowers willingness to buy the complex drugs by $0.800(-0.598)$ units relative to the simpler drugs. Importantly, observe that when the indirect effect of -0.478 is added to the direct effect $\hat{c}' = -0.085$, the results is $\hat{c}$, the total effect of X. That is, $\hat{c}' + \hat{a}\hat{b} = -0.478 + (-0.085) = -0.598 = \hat{c}$. So using Equations 9 through 11, the total effect of X partitions cleanly into direct and indirect components.

With a quantification of the indirect effect now available, we recommend an inference about this quantity, rather than the causal-

[2] As a consequent, d in Equation 10 will be twice b_2 in Equation 8. But this substitution does not affect any of the parameter estimates quantifying the indirect, direct, or total effect.

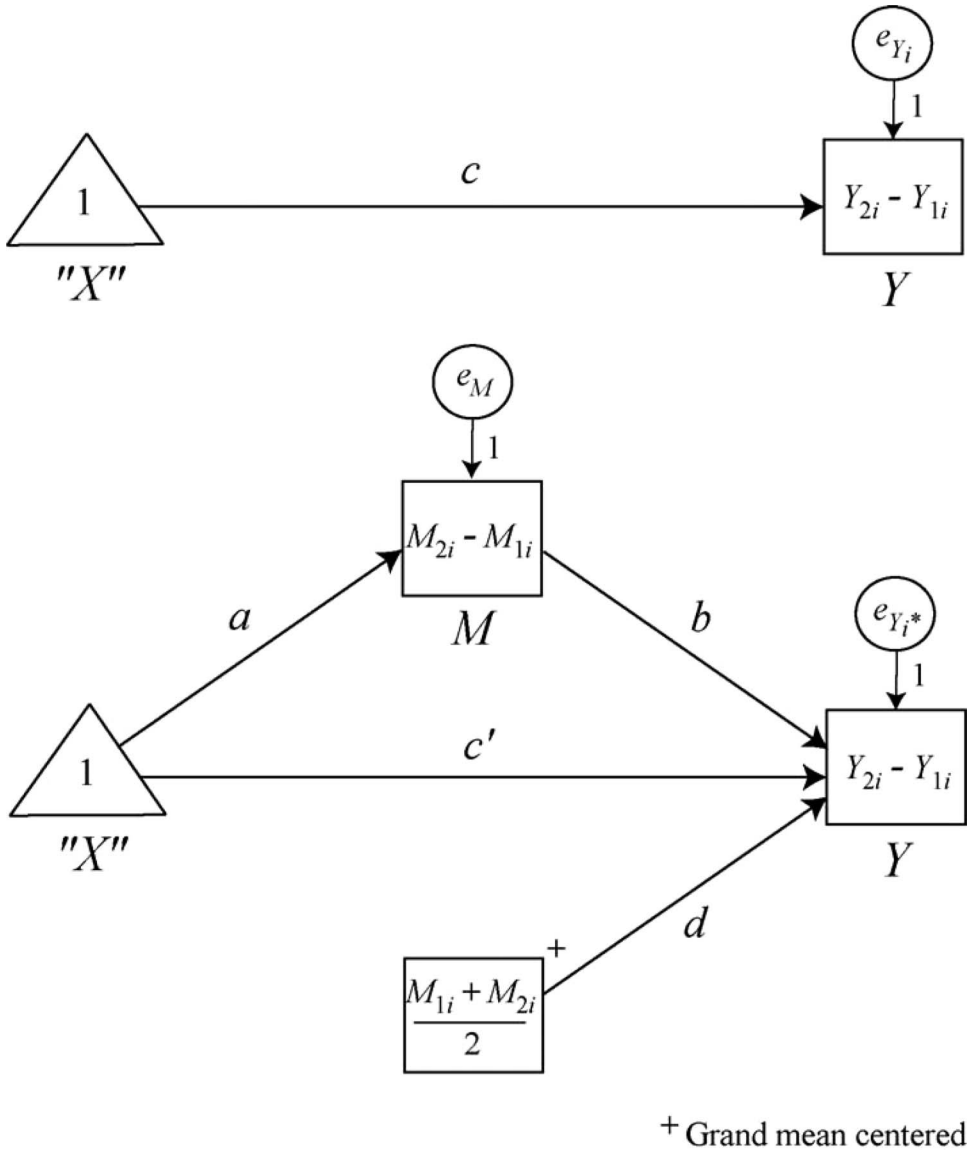

Figure 2. The mathematics of Judd et al. (2001) in path-analytic form for a single mediator.

steps argument relied on by Judd et al. (2001) that says that if both $\hat{a}$ and $\hat{b}$ are deemed different from zero, then mediation is established. We do not recommend this causal-steps approach for the same reasons it is not recommended in between-participant mediation analysis. The indirect effect is *ab*, not its individual components *a* and *b*. Recognizing and respecting this compels one to base an inference about mediation on an inference about *ab, which actually quantifies the effect of interest*, and not on a joint hypothesis test about *a* and *b*.

An estimate of the standard error of $\hat{a}\hat{b}$ can be derived using the multivariate delta method (Sobel, 1982), but we do not recommend this because the sampling distribution of $\hat{a}\hat{b}$ is not typically normal (the product of two normally distributed random variables is not normal; see, e.g., Bollen & Stine, 1990; Stone & Sobel, 1990). Rather, we recommend one of several approaches which respect the irregular shape of the sampling distribution of $\hat{a}\hat{b}$ and have been shown to perform well in the between-participant context. A bootstrap confidence interval is one such approach that is already widely used and recommended in mediation analysis, so it is the approach we emphasize here and have implemented in software for estimation we describe later.

Bootstrapping is a computationally intensive procedure that involves sampling of the rows of the data with replacement to build a new sample of size *n* from the original sample. In the new "resample," *ab* is estimated using Equations 9 and 10. This process is repeated *B* times (ideally, *B* is in the thousands) to build a bootstrap distribution of the indirect effect. A 95% confidence interval for the indirect effect using the percentile method is defined by two values of $\hat{a}\hat{b}$ in the bootstrap distribution of *B* estimates that define the 2.5th (the lower limit) and the 97.5th (the upper limit) percentiles of the distribution of estimates taken from the *B* resamples. As with other confidence

interval-based methods of inference, an indirect effect can be said to be different from zero if the confidence interval excludes zero.

The downside of bootstrapping is that it must be implemented for the specific model being estimated and in the software being used to perform the estimation, and most software is not initially setup to do this. In the next section we describe some options available that make bootstrapping a viable option for researchers wishing to use this approach. Using the SPSS macro introduced later, we constructed a bootstrap confidence interval for the indirect effect of drug name complexity on willingness to buy through perceived hazardousness. The resulting confidence interval based on 10,000 bootstrap samples was −0.742 to −0.206. As this interval does not include zero, we conclude that the indirect effect of drug name complexity on willingness to buy through hazardousness is negative and probably somewhere inside this interval.

There are a few alternatives to bootstrap confidence intervals that also respect the non-normality of the sampling distribution of the indirect effect. These methods may be particularly useful when bootstrapping cannot be conducted (e.g., when the original data are not available but point estimates and standard error estimates are). Using only the point estimates and estimated standard errors of $\hat{a}$ and $\hat{b}$, the distribution of the product method discussed in MacKinnon, Fritz, Williams, and Lockwood (2007) could be used. Tools for this method are available using the Rmediate package for R (Tofighi & MacKinnon, 2011). Alternatively, a Monte Carlo confidence interval (see, e.g., Preacher & Selig, 2012) could be constructed using various resources available online, including existing code for R, SPSS, or SAS (e.g., Hayes, 2013; Appendix B; Tofighi & MacKinnon, 2011). We generated 95% confidence intervals for the indirect effect in the drug name data using these tools, and they are the same for both of the approaches: −0.868 to −0.160. These identical results (to the third decimal place) are expected given that these tests are largely exchangeable and only rarely produce different inferences about the indirect effect (Hayes & Scharkow, 2013).

The Monte Carlo confidence interval, the multivariate delta method, the distribution of the product, and the test of joint significance (a joint hypothesis test for a and b using the tests from the Judd et al. approach) all require the standard error estimates of the $\hat{a}$ and $\hat{b}$ paths calculated from the model using the data available. Anything that affects the quality of these point estimates such as heteroscedasticity (Hayes & Cai, 2007; Long & Ervin, 2000; Yuan & MacKinnon, 2014) could potentially affect the inference. Because bootstrapping does not rely on estimates of the standard errors of the $\hat{a}$ and $\hat{b}$ paths, in principle it should be less sensitive to violations of this kind. For this reason, we recommend researchers estimate confidence intervals for the indirect effect using bootstrapping.

Implementation Using Structural Equation Modeling and Macros for Regression in SPSS and SAS

Any statistical program can be used to estimate the path coefficients in Equations 9, 10, and 11. Calculating bootstrap confidence intervals for the product of paths based on two models (i.e., Equations 9 and 10), requires either a structural equation modeling program capable of bootstrapping functions of model coefficients or some other computational aide. In the Appendix, we provide Mplus code that estimates this model as well as more complex models described in the following section. It also generates bootstrap confidence intervals for inference. We dedicate this section to describing two easy-to-use tools for SPSS and SAS that researchers can use for estimation and inference.

SPSS and SAS are well known and widely taught commercial software packages for data analysis that are available at little to no cost at most universities through site license arrangements. They both have language features that allow the user to construct new commands as "macros," and there are numerous published articles describing macros researchers have created for use in mediation analysis (e.g., Fairchild, MacKinnon, Taborga, & Taylor, 2009; Hayes, 2013; MacKinnon et al., 2007; Preacher & Hayes, 2004, 2008; Valeri & VanderWeele, 2013). Some of these are dedicated purely to inference about the indirect effect or the construction of effect size measures. Others estimate the entire model and provide inferential tests for direct and indirect effects. The latter are dedicated largely to the analysis of data from between-participant designs.

The PROCESS macro for SPSS and SAS (introduced and documented in Hayes, 2013, and freely available at www.processmacro.org) is widely used in mediation analysis in between-participant designs. It has a heretofore undocumented implementation of Judd et al. (2001) using OLS regression but with the addition of inference for the indirect effect using either percentile or bias-corrected bootstrap confidence intervals. It requires the user to first construct four new columns of data for each case in the data containing the differences between mediators ($M_2 - M_1$) and outcomes ($Y_2 - Y_1$), the average of the mediators $0.5(M_1 + M_2)$, as well as a constant containing all ones. Remember for the data in Table 1, variables with the subscript 1 are measures for the simple drug condition and variables with the subscript 2 are measures for the complex drug condition. Assuming Y_1, Y_2, M_1, and M_2 are named "buy1," "buy2," "hazard1," and "hazard2," respectively, the SPSS code below conducts the analysis.

```
compute ydiff=buy2−buy1.
compute mdiff=hazard2−hazard1.
compute mavg=0.5*(hazard1+hazard2).
compute const=1.
process vars=ydiff mdiff mavg const/y=ydiff/
  m=mdiff/x=const/model=4/ws=1/percent=1/
  boot=10000.
```

The equivalent code in SAS, assuming the data are in a file named "drugname," is

```
data drugname;set drugname;
ydiff=buy2−buy1;mdiff=hazard2−hazard1;
const=1;mavg=0.5*(hazard1+hazard2);run;
%process (data=drugname,vars=ydiff mdiff
  mavg const,y=ydiff,m=mdiff,x=const,model=4,
  ws=1,percent=1,boot=10000);
```

The output includes estimates of a, b, c, and c' from OLS regression models based on Equations 9 through 11. A point estimate and bootstrap confidence interval is also provided for the indirect effect.

We have developed a macro for SPSS and SAS dedicated to mediation and moderation analysis in within-participant designs

that has features not available in PROCESS and is a bit easier to use. Named MEMORE (MEdiation and MOderation analysis for REpeated measures designs, and pronounced like "memory"), it has functions built in for estimating the direct, indirect, and total effects in the two-condition within-participant design that is the topic of this article. It can estimate models with multiple mediators, as discussed in the next section, and it can also estimate other models we do not discuss here. Bootstrap methods of inference for the indirect effect can be generated by MEMORE, but it also provides Monte Carlo confidence intervals as an option, as well as the multivariate delta method. MEMORE is freely available and can be downloaded from www.afhayes.com, where the documentation describing its use can also be found.

Unlike PROCESS, MEMORE does not require the user to manually construct averages or differences of mediators or outcomes. Rather, the variables containing the data are merely included in the MEMORE command in the proper order as discussed in the documentation, and the macro does the rest. The SPSS version of the MEMORE command that estimates the model for drug names study and constructs a percentile bootstrap confidence interval (the default) for the indirect effect using 10,000 bootstrap samples is

```
memore y=buy2 buy1/m=hazard2 hazard1/
  samples=10000.
```

The equivalent command in SAS is

```
%memore (data=drugname,y=buy2 buy1,m=hazard2
  hazard1,samples=10000);
```

A dialog box for SPSS is also available for SPSS users who prefer the point-and-click interface rather than syntax.

Output from the SPSS version for this analysis can be found in Figure 3. As can be seen, MEMORE provides estimates of all the paths in the model while also offering a summary section at the end containing estimates and inferential tests for the total, direct, and indirect effects of X, including a bootstrap confidence interval for the indirect effect. Monte Carlo generation rather than bootstrapping can be used to construct the confidence interval by specifying the option "mc=1" in the MEMORE command line.

More Than One Mediator

Causal processes often operate through more than one mechanism simultaneously, and models with more than one mediator are routinely estimated and tested in between-participant designs (e.g., Jones, Willness, & Madey, 2014; Kurti & Dallery, 2014; Valentine, Li, Penke, & Perrett, 2014;). When multiple processes are theorized to carry X's effect on Y, it is better to estimate a model that respects such complexities. Although one could estimate a set of single-mediator models, doing so confounds estimates of the indirect effect for each mediator with indirect effects through the mediators not in the model. Estimating a model with multiple indirect effects gets around this problem while also facilitating competitive theory testing by allowing for the comparison of the size of indirect effects through different mediators. In this section, extending the path-analytic framework introduced earlier, we describe estimation and inference about indirect effects in models with multiple mediators in the two-condition within-participant design.

The Parallel Multiple Mediator Model

Figure 4 depicts a parallel multiple-mediator model with k mediators. In a parallel multiple-mediator model, the mediators may be correlated, but no formal commitment is made about any causal influence between mediators and hence none are causally connected by a unidirectional arrow. The result is a model with one direct effect of X and k *specific indirect effects* of X on Y, one through each mediator. A specific indirect effect is the effect of X on Y through a mediator M_j controlling for the effect of the other proposed mediators in the model on Y. In a two-condition within-participant design, the mediators are differences between measurements of the same mediator in the two conditions, and the outcome is the difference in Y between conditions, as in a single-mediator model. The grand mean-centered means of each mediator pair are used as covariates in the model of $Y_2 - Y_1$.

The path coefficients of this model can be estimated using OLS regression or structural equation modeling by specifying the model in the form of a set of equations, where M_{j1i} and M_{j2i} are the measures of mediator j in Conditions 1 and 2, respectively, for participant i:

$$M_{j2i} - M_{j1i} = a_j + e_{M_{ji}} \tag{12}$$

$$Y_{2i} - Y_{1i} = c' + \sum_{j=1}^{k} b_j(M_{j2i} - M_{j1i}) + \sum_{j=1}^{k} d_j[0.5(M_{j1i} + M_{j2i}) - \overline{0.5(M_{j1} + M_{j2})}] + e_{Y_i^*} \tag{13}$$

The specific indirect effect of X on Y through mediator j is estimated as $\hat{a}_j\hat{b}_j$, the product of the estimates of a_j and b_j from Equations 12 and 13. The *total indirect effect* is the sum of the specific indirect effects: $\sum_{j=1}^{k} a_j b_j$. Inference for these effects can be conducted using any of the methods described above for the single-mediator model, such as a bootstrap confidence interval. The direct effect, c' in Equation 13, and the total indirect effect sum to yield c, the total effect of X. That is, $c = c' + \sum_{j=1}^{k} a_j b_j$. The total effect of X can also be estimated with Equation 11. Inference for the direct and total effects can proceed as usual by relying on estimates of their standard errors for hypothesis testing or confidence interval construction.

We illustrate by adding a mediator to the earlier example. Dohle and Siegrist (2014) argued there may be two potential mechanisms through which drug name complexity could influence willingness to buy the drug: perceived hazardousness and perceived effectiveness. The representativeness heuristic would lead participants to perceive drugs with more complex names as more effective than drugs with simpler names, and this would in turn prompt a greater willingness to buy the drugs with complex names. By controlling for perceived effectiveness in the model, the indirect effect of drug name complexity on willingness to buy *specifically* through hazardousness can be estimated (i.e., the effect is not confounded with the indirect effect through effectiveness). To investigate these two separate indirect effects, Dohle and Siegrist (2014) also had participants rate their perception of the effectiveness of each drug. When added to a model already including perceived hazardousness as a mediator,

```
************************** MEMORE Procedure for SPSS **************************
Variables:
Y = buy2     buy1
M = hazard2  hazard1

Computed Variables:
Ydiff =          buy2      -       buy1
Mdiff =          hazard2   -       hazard1
Mavg =  (        hazard2   +       hazard1  )        /2       Centered

Sample Size:   22
******************************************************************************
Outcome: Ydiff =  buy2      -       buy1

Model
        Effect         SE          t         df          p       LLCI       ULCI
'X'     -.5636      .1932    -2.9168    21.0000      .0082     -.9655     -.1618
******************************************************************************
Outcome: Mdiff =  hazard2   -       hazard1

Model
        Effect         SE          t         df          p       LLCI       ULCI
'X'      .8000      .2579     3.1024    21.0000      .0054      .2637     1.3363
******************************************************************************
Outcome: Ydiff =  buy2      -       buy1

Model Summary
          R       R-sq        MSE          F        df1        df2          p
      .7721      .5961      .3667    14.0213     2.0000    19.0000      .0002

Model
           coeff         SE          t         df          p       LLCI       ULCI
'X'       -.0851      .1577     -.5399    19.0000      .5955     -.4152      .2449
Mdiff     -.5981      .1131    -5.2869    19.0000      .0000     -.8349     -.3613
Mavg      -.1818      .1683    -1.0803    19.0000      .2935     -.5341      .1705

********************** TOTAL, DIRECT, AND INDIRECT EFFECTS **********************

Total effect of X on Y
     Effect         SE          t         df          p       LLCI       ULCI
     -.5636      .1932    -2.9168    21.0000      .0082     -.9655     -.1618

Direct effect of X on Y
     Effect         SE          t         df          p       LLCI       ULCI
     -.0851      .1577     -.5399    19.0000      .5955     -.4152      .2449

Indirect Effect of X on Y through M
          Effect     BootSE   BootLLCI   BootULCI
Ind1      -.4785      .1363     -.7423     -.2063

Indirect Key
Ind1  X        ->       M1diff   ->       Ydiff
```

Figure 3. Output from the MEMORE macro for SPSS for the single-mediator model.

the result is a parallel multiple-mediator model with two mediators. Table 3 contains the data, which is equivalent to the data in Table 1 with the addition of the two measurements of perceived effectiveness, denoted $M_{2\bullet}$.

Figure 5 contains model output from the MEMORE macro for SPSS containing the direct, indirect, and total effects, including estimates of all paths with standard error estimates and confidence intervals. Confidence intervals for the indirect effects are from the percentile bootstrap method from 10,000 bootstrap samples. This output was generated with this command:

```
memore y=buy2 buy1/m=hazard2 hazard1
  effect2 effect1/samples=10000/contrast=1.
```

where perceived effectiveness of the simple and complex drugs are named "effect1" and "effect2," respectively. The equivalent command in SAS is

```
%memore (data=drugname,y=buy2 buy1,
  m=hazard2 hazard1 effect2 effect1,
  samples=10000,contrast=1);
```

The corresponding code for Mplus can be found in the Appendix.

As can be seen in the output, $\hat{a}_1 = 0.800$, $\hat{a}_2 = -0.300$, $\hat{b}_1 = -0.591$, $\hat{b}_2 = 0.185$, $\hat{c}' = -0.036$. The indirect effects are formed as products of the paths to $(\hat{a}_j)$ and from $(\hat{b}_j)$ a common mediator. The indirect effect of drug name complexity through perceived hazardousness is $\hat{a}_1\hat{b}_1 = 0.800(-0.591) = -0.472$,

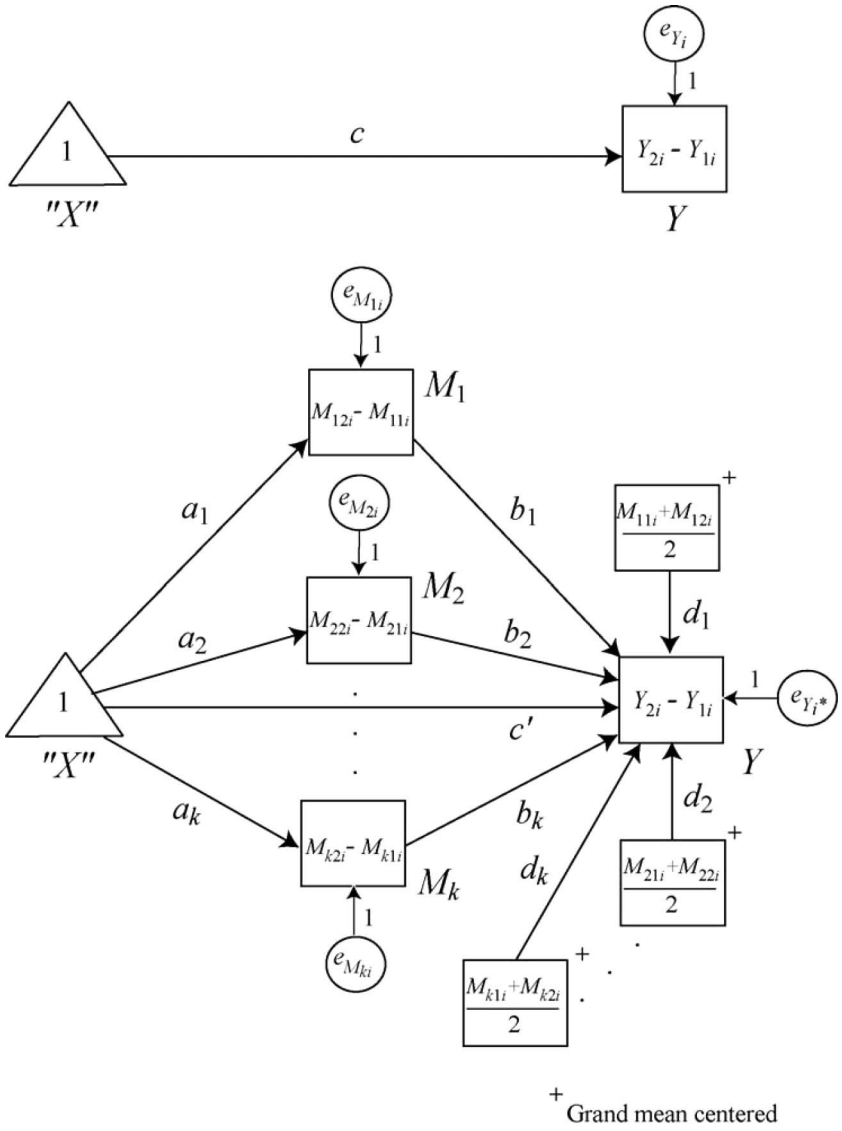

Figure 4. A within-participant parallel multiple-mediator model with k mediators in path analysis form (covariances between errors in the estimation of mediator differences are excluded to reduce visual clutter).

with 95% bootstrap CI [−0.745, −0.164]. Note that the estimated indirect effect through hazardousness in this model is slightly smaller than the estimated indirect effect in the single-mediator model. Differences would be expected because the parallel mediation model is controlling for effectiveness when estimating the indirect effect through hazardousness. The indirect effect of drug name complexity through perceived effectiveness is $\hat{a}_2\hat{b}_2 = -0.300(0.185) = -0.055$, with 95% bootstrap CI [−0.218, 0.194]. This effect is not significantly different from zero, and is actually opposite of the sign predicted by the representative heuristic. These two indirect effects sum to the total indirect effect: −0.528, with a 95% bootstrap CI [−0.767, –0.217]. The direct effect $\hat{c}' = -0.036$ is not statistically different from zero, $t(17) = -0.235$, $p = .817$, 95% CI [−0.356, 0.284]. And observe that the total effect of drug name complexity, $\hat{c} = -0.564$, does indeed equal the sum of the direct and indirect effects: $-0.036 + (-0.472) + (-0.055) = -0.564$.

Comparing Specific Indirect Effects

These findings support the fluency theory prediction over the prediction based on consumers' use of the representativeness heuristic. Complexly named drugs were perceived as more hazardous and, controlling for perceived effectiveness, this increase was related to a lower willingness to buy the complex drugs relative to the more simply named drugs; hence, the negative indirect effect of drug name complexity through perceived hazardousness. The evidence does not support the competing process—that complexly named drugs would be perceived as more effective, thereby increasing willingness to buy; this indirect effect was small and not definitively different from zero.

Table 3
Data From Study 1 of Dohle and Siegrist (2014)

	Simple names			Complex names		
ID	M_{11}	M_{21}	Y_1	M_{12}	M_{22}	Y_2
1	3.8	4.2	4.4	4.4	4.0	3.6
2	4.2	4.4	4.2	5.2	3.6	2.0
3	4.0	4.0	4.0	4.0	4.0	4.0
4	4.4	4.2	3.0	3.0	4.8	5.2
5	2.2	5.8	5.2	4.2	4.4	3.6
6	3.2	5.2	4.8	4.2	4.8	3.6
7	6.6	1.0	1.0	7.0	1.8	1.0
8	2.8	4.4	4.6	4.8	4.0	3.8
9	3.4	4.2	4.6	4.8	3.6	3.2
10	3.2	5.2	4.6	4.6	3.6	3.8
11	4.2	4.0	4.8	4.0	4.0	5.0
12	4.4	4.2	2.8	5.6	3.8	1.6
13	3.6	4.6	2.0	4.8	3.4	1.2
14	7.0	6.6	1.0	5.4	5.6	1.0
15	4.0	4.0	4.0	4.0	4.0	4.0
16	3.8	4.4	4.2	3.8	4.8	4.2
17	4.0	4.0	3.0	4.4	4.4	2.6
18	2.6	4.4	5.4	5.0	5.0	4.4
19	2.6	4.6	5.0	5.8	2.2	3.8
20	4.0	4.0	4.0	4.0	4.0	4.0
21	4.0	4.0	4.0	4.0	4.0	4.0
22	3.2	4.6	4.2	5.8	5.6	2.8

Note. $M_{1\cdot}$ = hazardousness; $M_{2\cdot}$ = effectiveness; Y = willingness to buy.

But difference in significance does not imply significantly different. One indirect effect could be different from zero while another is not, yet these two indirect effects may not be different from each other. Furthermore, it is possible that two indirect effects representing competing theoretical processes are both different from zero, or neither may be, leaving an investigator wondering if one theory is better supported by the data than another. A more precise approach to testing these competing predictions would be to compare the indirect effects to each other using an estimate of their difference.

Statistical methods for comparing indirect effects in multiple-mediator models have been developed for between-participant designs (Hayes, 2013; MacKinnon, 2000, 2008; Preacher & Hayes, 2008; Raykov, Brennan, Reinhardt, & Horowitz, 2008). Application of these methods to within-participant designs is straightforward. The difference between indirect effects through mediator i and mediator j, estimated as $\hat{a}_i\hat{b}_i - \hat{a}_j\hat{b}_j$, is subject to sampling variance just as is any estimator. With a standard error and assumptions about the shape of the sampling distribution, a hypothesis test or confidence interval can be used for inference about the difference. Formulas for doing so in between-participant designs are provided by Hayes (2013) and MacKinnon (2008), which also generalize to within-participant designs. This approach can be implemented in Mplus in the MODEL CONSTRAINT section of the code. See the Appendix.

Alternatively, a bootstrap or Monte Carlo confidence interval could test the contrast without making any assumptions about the shape of the sampling distribution of the difference (Preacher & Selig, 2012, discuss comparing indirect effects with the Monte Carlo method). The MEMORE macro implements both approaches, and bootstrapping can be conducted in Mplus. In MEMORE, all possible pairwise comparisons between specific indirect effects in a multiple-mediator model are conducted by specifying "contrast=1" in the command line, as above. The bottom of the output in Figure 5 shows the results for this analysis. As can be seen, the indirect effect through perceived hazardousness is significantly different than the indirect effect through perceived effectiveness, $\hat{a}_1\hat{b}_1 - \hat{a}_2\hat{b}_2 = -0.417$, 95% bootstrap CI [−0.877, −0.179].

The Serial Multiple-Mediator Model

In a serial mediation model, each mediator is presumed to affect other mediators causally downstream. Numerous examples of the between-participant form of this model can be found in the substantive literature (e.g., Kan, Lichtenstein, Grant, & Janiszwski, 2014; Tsang, Carpenter, Roberts, Frisch, & Carlisle, 2014). The mechanics of estimation, inference, as well as interpretation in the between-participant design are discussed in Hayes (2013), Hayes, Preacher, and Myers (2011), and Taylor, MacKinnon, and Tein (2008).

Figure 6 depicts a path diagram of a serial multiple-mediator model with two mediators for the two-condition within-participant design. This model represents three equations, two for the mediators and one for the outcome:

$$M_{12i} - M_{11i} = a_1 + e_{M_{1i}} \tag{14}$$

$$M_{22i} - M_{21i} = a_2 + a_3(M_{12i} - M_{11i}) + d_0[0.5(M_{11i} + M_{12i}) - \overline{0.5(M_{11} + M_{12})}] + e_{M_{2i}} \tag{15}$$

$$Y_{2i} - Y_{1i} = c' + \sum_{j=1}^{2} b_j(M_{j2i} - M_{j1i}) + \sum_{j=1}^{2} (d_j[0.5(M_{j1i} + M_{j2i}) - \overline{0.5(M_{j1} + M_{j2})}]) + e_{Y_i^*} \tag{16}$$

There are three specific indirect effects of X in this model, one through M_1 only (estimated as $\hat{a}_1\hat{b}_1$), one through M_2 only (estimated as $\hat{a}_2\hat{b}_2$), and one through M_1 and M_2 in serial (estimated as $\hat{a}_1\hat{a}_3\hat{b}_2$) which sum to yield the total indirect effect of X. Any of the methods discussed thus far could be used for inference about these indirect effects. Equation 16 estimates the direct effect of X, $\hat{c}'$. When added to the total indirect effect, the result is the total effect of X. That is, $\hat{c} = \hat{c}' + \hat{a}_1\hat{b}_1 + \hat{a}_2\hat{b}_2 + \hat{a}_1\hat{a}_3\hat{b}_2$. Alternatively, the model in Equation 11 would provide the same estimate of c. Inference for the direct and total effects can proceed as already discussed.

Applied to the drug names study, suppose complex drug names are hypothesized to induce a sense of greater hazardousness than drugs with simpler names, and this relative difference in hazardousness is presumed to result in a perception of greater effectiveness of the complexly named drugs, which in turn prompts greater willingness to buy drugs with more complex names. In terms of Figure 6, M_1 is hazardousness and M_2 is effectiveness. Figure 7 contains output from the MEMORE macro for SPSS containing the direct, indirect, and total effects, including estimates of all paths with standard errors and confidence intervals. Confidence intervals for the indirect effects are from the percentile bootstrap method from 10,000 bootstrap samples. This output was generated with the command

```
memore y=buy2 buy1/m=hazard2 hazard1
  effect2 effect1/samples=10000/serial=1.
```

The equivalent command in SAS is

```
%memore (data=drugname,y=buy2 buy1,
```

```
************************** MEMORE Procedure for SPSS *************************
Variables:
Y =   buy2     buy1
M1 =  hazard2  hazard1
M2 =  effect2  effect1

Computed Variables:
Ydiff =          buy2      -       buy1
M1diff =         hazard2   -       hazard1
M2diff =         effect2   -       effect1
M1avg  = (       hazard2   +       hazard1  )       /2       Centered
M2avg  = (       effect2   +       effect1  )       /2       Centered

Sample Size:   22
*****************************************************************************
Outcome: Ydiff =  buy2      -       buy1

Model
        Effect         SE          t         df          p       LLCI       ULCI
'X'     -.5636      .1932    -2.9168    21.0000      .0082     -.9655     -.1618
*****************************************************************************
Outcome: M1diff = hazard2   -       hazard1

Model
        Effect         SE          t         df          p       LLCI       ULCI
'X'      .8000      .2579     3.1024    21.0000      .0054      .2637     1.3363
*****************************************************************************
Outcome: M2diff = effect2   -       effect1

Model
        Effect         SE          t         df          p       LLCI       ULCI
'X'     -.3000      .1798    -1.6683    21.0000      .1101     -.6740      .0740
*****************************************************************************
Outcome: Ydiff =  buy2      -       buy1

Model Summary
          R       R-sq        MSE          F        df1        df2          p
      .8212      .6744      .3304     8.8040     4.0000    17.0000      .0005

Model
            coeff         SE          t         df          p       LLCI       ULCI
'X'        -.0357      .1517     -.2352    17.0000      .8169     -.3557      .2844
M1diff     -.5905      .1165    -5.0684    17.0000      .0001     -.8364     -.3447
M2diff      .1851      .1596     1.1599    17.0000      .2621     -.1516      .5218
M1avg      -.2898      .1738    -1.6679    17.0000      .1137     -.6564      .0768
M2avg      -.2361      .1625    -1.4528    17.0000      .1645     -.5791      .1068

********************* TOTAL, DIRECT, AND INDIRECT EFFECTS *********************
Total effect of X on Y
     Effect         SE          t         df          p       LLCI       ULCI
     -.5636      .1932    -2.9168    21.0000      .0082     -.9655     -.1618

Direct effect of X on Y
     Effect         SE          t         df          p       LLCI       ULCI
     -.0357      .1517     -.2352    17.0000      .8169     -.3557      .2844

Indirect Effect of X on Y through M
          Effect     BootSE   BootLLCI   BootULCI
Ind1      -.4724      .1469     -.7445     -.1644
Ind2      -.0555      .0964     -.2177      .1943
Total     -.5280      .1411     -.7695     -.2173

Indirect Key
Ind1  X        ->       M1diff   ->       Ydiff
Ind2  X        ->       M2diff   ->       Ydiff

Pairwise Contrasts Between Specific Indirect Effects
          Effect     BootSE   BootLLCI   BootULCI
(C1)      -.4169      .2045     -.8774     -.0178

Contrast Key:
(C1)  Ind1      -       Ind2
```

Figure 5. Output from the MEMORE macro for SPSS for a parallel multiple-mediator model.

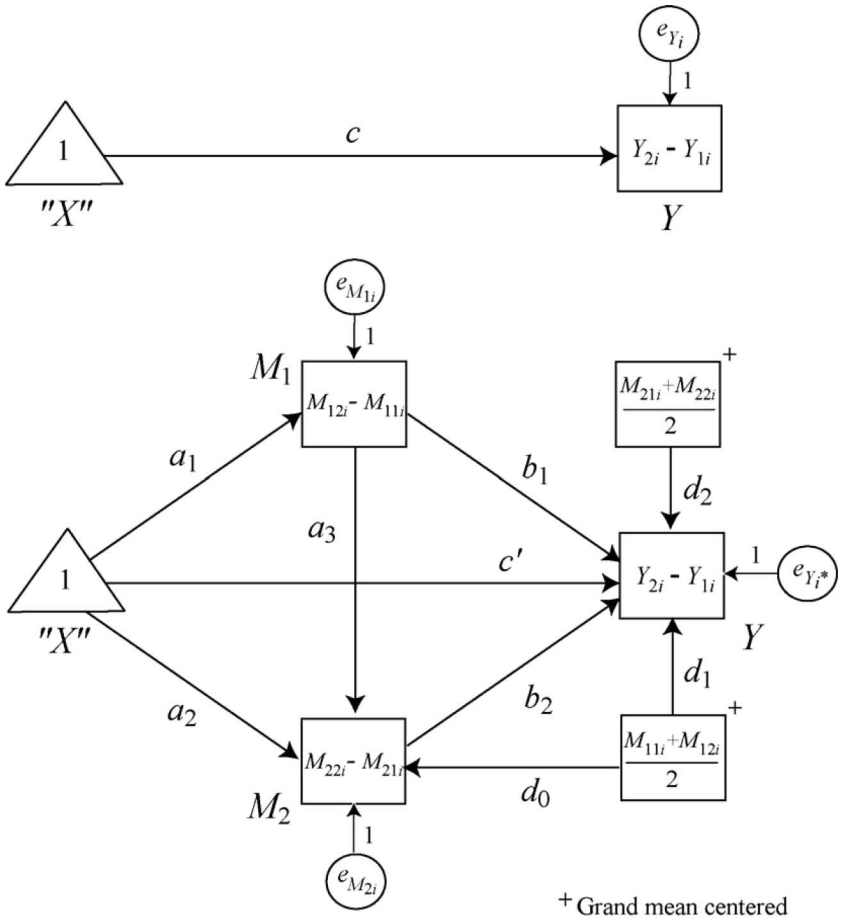

Figure 6. A within-participant serial mediation model with two mediators in path analysis form.

```
m=hazard2 hazard1 effect2 effect1,
samples=10000,serial=1);
```

The "serial = 1" specification tells MEMORE to estimate a serial mediation model rather than a parallel mediation model. Corresponding code for Mplus can be found in the Appendix.

As can be seen in Figure 7, $\hat{a}_1 = 0.800$, $\hat{a}_2 = -0.122$, $\hat{a}_3 = -0.222$, $\hat{b}_1 = -0.591$, $\hat{b}_2 = 0.185$, and $\hat{c}' = -0.036$. The indirect effect of drug name complexity through perceived hazardousness alone is $\hat{a}_1\hat{b}_1 = 0.800(-0.591) = -0.472$, with 95% bootstrap CI [−0.745, −0.164]. The indirect effect of drug name complexity through perceived effectiveness alone is $\hat{a}_2\hat{b}_2 = -0.122(0.185) = -0.023$, with 95% bootstrap CI [−0.153, 0.109]. The serial indirect effect through both hazardousness and effectiveness is $\hat{a}_1\hat{a}_3\hat{b}_2 = (0.800)(-0.222)(0.185) = -0.033$ with 95% bootstrap CI [−0.240, 0.150]. These indirect effects sum to the total indirect effect: −0.528, with 95% bootstrap CI [−0.770, −0.217]. The direct effect in this model is necessarily the same as the direct effect in the parallel model: $\hat{c}' = -0.036$ and not statistically different from zero, $t(17) = -0.235$, $p = .817$, 95% CI [−0.356, 0.284]. Observe that the total effect of drug name complexity, $\hat{c} = -0.564$ equals the sum of the direct and indirect effects: $-0.036 + (-0.472) + (-0.023) + (-0.033) = -0.564$.

These results do not support the existence of a serial mediation process from complex drug names to greater perceived hazardousness to greater perceived effectiveness to greater willingness to buy. Rather, the only definitive evidence favors the fluency theory interpretation, where drugs with complex names are perceived as more hazardous than drugs with simple names, and this *lowers* willingness to buy. If a researcher was interested in comparing the specific indirect effects, as in the parallel mediation example, all possible pairwise comparisons between specific indirect effects can be conducted by specifying "contrast=1" in the MEMORE command line.

Extensions and Alternatives

By recasting the Judd et al. (2001) approach into a path-analytic framework, many extensions to more complicated models which may be of interest to psychological researchers become apparent. We have discussed models with multiple mediators as one set of extensions. We close by briefly discussing moderation and extensions to more than two conditions. Additionally, we discuss some alternative analytical approaches for statistical mediation analysis in within-participant designs of this sort.

Moderation of Mechanisms

The model we have described focuses on the estimation of and inference about average effects across people and inferences, ignoring

```
************************** MEMORE Procedure for SPSS **************************
Variables:
Y =   buy2     buy1
M1 =  hazard2  hazard1
M2 =  effect2  effect1

Computed Variables:
Ydiff =          buy2      -       buy1
M1diff =         hazard2   -       hazard1
M2diff =         effect2   -       effect1
M1avg  = (       hazard2   +       hazard1  )       /2       Centered
M2avg  = (       effect2   +       effect1  )       /2       Centered

Sample Size:   22
*****************************************************************************
Outcome: Ydiff =  buy2      -       buy1

Model
       Effect         SE          t         df          p       LLCI       ULCI
'X'     -.5636      .1932    -2.9168    21.0000      .0082     -.9655     -.1618
*****************************************************************************
Outcome: M1diff = hazard2   -       hazard1

Model
       Effect         SE          t         df          p       LLCI       ULCI
'X'      .8000      .2579     3.1024    21.0000      .0054      .2637     1.3363
*****************************************************************************
Outcome: M2diff = effect2   -       effect1

Model Summary
          R       R-sq        MSE          F        df1        df2          p
      .3308      .1094      .7003     1.1675     2.0000    19.0000      .3325

Model
            coeff         SE          t         df          p       LLCI       ULCI
'X'       -.1224      .2179     -.5618    19.0000      .5808     -.5785      .3337
M1diff    -.2220      .1563    -1.4200    19.0000      .1718     -.5493      .1052
M1avg      .0411      .2326      .1766    19.0000      .8617     -.4457      .5278
*****************************************************************************
Outcome: Ydiff =  buy2      -       buy1

Model Summary
          R       R-sq        MSE          F        df1        df2          p
      .8212      .6744      .3304     8.8040     4.0000    17.0000      .0005

Model
            coeff         SE          t         df          p       LLCI       ULCI
'X'       -.0357      .1517     -.2352    17.0000      .8169     -.3557      .2844
M1diff    -.5905      .1165    -5.0684    17.0000      .0001     -.8364     -.3447
M2diff     .1851      .1596     1.1599    17.0000      .2621     -.1516      .5218
M1avg     -.2898      .1738    -1.6679    17.0000      .1137     -.6564      .0768
M2avg     -.2361      .1625    -1.4528    17.0000      .1645     -.5791      .1068

********************* TOTAL, DIRECT, AND INDIRECT EFFECTS *********************
Total effect of X on Y
    Effect         SE          t         df          p       LLCI       ULCI
    -.5636      .1932    -2.9168    21.0000      .0082     -.9655     -.1618

Direct effect of X on Y
    Effect         SE          t         df          p       LLCI       ULCI
    -.0357      .1517     -.2352    17.0000      .8169     -.3557      .2844

Indirect Effect of X on Y through M
         Effect     BootSE   BootLLCI   BootULCI
Ind1     -.4724      .1469     -.7445     -.1644
Ind2     -.0227      .0611     -.1531      .1085
Ind3     -.0329      .0912     -.2401      .1499
Total    -.5280      .1411     -.7695     -.2173

Indirect Key
Ind1  X        ->       M1diff   ->       Ydiff
Ind2  X        ->       M2diff   ->       Ydiff
Ind3  X        ->       M1diff   ->       M2diff   ->       Ydiff
```

Figure 7. Output from the MEMORE macro for SPSS for a serial multiple-mediator model.

individual variation in these effects. There is a growing literature on methods for examining the moderation of components of a mediation process (Edwards & Lambert, 2007; Fairchild & MacKinnon, 2009; Hayes, 2013; Muller, Judd, & Yzerbyt, 2005; Preacher, Rucker, & Hayes, 2007). In the within-participant two-condition design we have been addressing, potential moderators of the indirect or direct effect could include order of stimulus presentation, or stable individual differences such as personality, attitudes, or experiences. The average of the mediator measured across conditions could even be a potential moderator. For instance, in our working example, the effect of drug name complexity on perceived hazardousness could vary systematically as a function of how hazardous people perceive drugs to be in general, or their trust in the pharmaceutical industry, or whether they know someone who has had a bad reaction to a drug. In principle, the methods already developed in the between-participant mediation literature could be applied to this model. How to do so is beyond the scope of this article.

When *X* is Multicategorical

For simplicity and due to space constraints, our discussion has been restricted to designs in which *M* and *Y* are measured in two conditions that vary by design. This is a very popular design, but in some within-participant designs, participants are measured on *M* and *Y* in more than two circumstances, meaning *X* has more than two instantiations. For instance, Cooke, Kavussanu, McIntyre, and Ring (2013) examined competitive performance on a physical task as mediated by enjoyment and anxiety in four conditions that varied with respect to the nature of the competition (e.g., one vs. one, two vs. two, and so forth). All participants engaged in the task in all four competitive conditions and were measured on several potential mediators as well as measures of performance in all four conditions.

Hayes and Preacher (2014) discuss mediation analysis in the between-participant design when *X* is multicategorical, and Judd et al. (2001) discuss an extension of their approach to mediation analysis when *M* and *Y* are repeatedly measured within-participant in more than two conditions. The Judd et al. approach relies on the construction of difference scores representing various contrasts of interest. In principle, this extension to the multicategorical *X* case could be represented in path-analytic form as we have done for the two-condition case. A description of the mathematics and mechanics is beyond the scope of this article. Suffice it to say that the direct and indirect effects for various contrasts would be "relative" and dependent on how the contrasts are formed, yet still sum to their corresponding total effects, as described by Hayes and Preacher (2014). But the question as to whether *M* mediates *X*'s effect will necessarily depend on how the contrasts are constructed. One set of contrasts could lead to a claim of mediation, but a different set may not. Because Judd et al. (2001) rely on the causal-steps logic of mediation and do not discuss the estimation of the indirect effect, they do not offer what could be construed as an "omnibus" test of mediation that is based on the indirect effect rather than tests of significance of the individual paths in the model. This is potentially a fruitful area for future research.

Alternative Analytical Approaches

When the data for a within-participant analysis are represented in "wide" form, as in Table 1, *X* is missing from the data. Its effect on *M* and *Y* is captured in the model as a regression constant rather than as a regression coefficient. There is an alternative way of representing the data that does involve a coding of *X*, thereby allows *X* to be a variable in the model. The data can be represented in "long" form, with each participant's data in two rows, one corresponding to measurements of *M* and *Y* in one condition and the second row containing measurements of *M* and *Y* in the other condition. With the data in this form, it is tempting to approach the analysis as one would a between-participant mediation analysis using Equations 1 and 2. But such an analysis with the data in this form would not properly estimate all the effects or their standard errors, confidence intervals would be incorrect, and hypothesis tests invalid. Doing so would not, among other things, properly take into account the paired nature of the data and would violate the assumption of independence in the errors of estimation.

But in the example presented here, the two measures of *M* and *Y* in Table 1 are actually aggregates of *five* measurements in the simple drug names condition, and *five* in the complex drugs names condition. Such aggregation is common in within-participant designs that use multiple versions of a stimulus in different conditions. Yet it is not necessary if the analysis is approached from a multilevel perspective rather than an ordinary regression perspective. A multilevel analysis of these data, represented in long form with 10 rows per participant, would construe the 10 manipulations of *X* (simple or complex drug name, coded 1 and 2, for example), *M* and *Y* as Level 1 observations nested within person, the Level 2 unit. This would allow for the estimation of the direct, indirect, and total effects as either random or fixed between participants, while still providing an estimate of the average effects across participants. There is a developing literature on multilevel mediation analysis that could be applied to this problem (see, e.g., Bauer, Preacher, & Gil, 2006; Kenny, Korchmaros, & Bolger, 2003; Zhang, Zyphur, & Preacher, 2009). We encourage researchers using multiple versions of a stimulus in each condition to consider this multilevel framework as an alternative. How to do so is also beyond the scope of this article. Some advantages of this approach include being able to model the effects of more than two conditions using methods other than those outlined in Judd et al. (2001), as well as being able to model both fixed and random effects. The disadvantage of a multilevel approach is that requires special software capable of estimating multiple multilevel models simultaneously, though this can be implemented in Mplus. Additionally, it demands careful thought about whether to estimate an effect as fixed or random, model convergence is not guaranteed, inferential procedures for the indirect effect in multilevel analysis are still undergoing development and refinement and their performance is less well understood, and it is generally more difficult to do without familiarity with this more complex method and the relevant literature. Additional alternatives include latent difference score approaches to mediation, which are beyond the scope of this manuscript, but are aptly described in MacKinnon (2008) and Selig and Preacher (2009).

Summary

In this article, we addressed statistical mediation analysis in the two-condition within-participant design, recasting the approach introduced by Judd et al. (2001) in a path-analytic framework. Doing so makes it apparent how the total effect of a within-participant manipulation of *X* on *Y* can be broken into direct and indirect components through a mediator *M*, with the latter being the product of the effect of *X* on *M* and the effect of *M* on *Y*. By providing a point and interval estimate of the indirect effect, we eliminate the need to rely on a series of hypothesis tests about its component effects. We recommend bootstrap confidence intervals for inference about the indirect effect rather than the causal-steps logic advocated by Judd et al. (2001). Set up in the form of a path analysis, this method is easily extended to multiple-mediator models, a topic previously unaddressed within the framework of two-condition within-participant designs. We discussed how to analyze both parallel and serial multiple-mediator models, including how to compare specific indirect effects when testing competing theories. This method is easy to implement in Mplus with code we provide, and even easier to apply with freely available macros for SPSS and SAS that conduct all the computations, including bootstrap and Monte Carlo confidence intervals for inferences about indirect effects.

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Appendix

Model Estimation and Inference With Mplus

Mplus is a covariance structure modeling program that has become widely used in part because of its versatility. Though not the only structural equation modeling program that could be used to conduct the type of analysis described in this article, two features that make it particularly convenient are a bootstrapping algorithm activated with the ANALYSIS command and options for constructing new model parameters that are functions of others using the MODEL CONSTRAINT command. The Mplus code below estimates the single-mediator model in Figure 2. The data file the code reads takes the form of a text file formatted as in Table 3.

```
TITLE: Mplus code for the single-mediator model
DATA:
  file is 'c:\mplus\table3.dat';
VARIABLE:
  names are hazard1 effect1 buy1 hazard2 effect2 buy2;
  usevariables mdiff ydiff mmeanc;
DEFINE:
  mdiff=hazard2-hazard1;
  ydiff=buy2-buy1;
  mmeanc=0.5*((hazard1+hazard2)-8.546);
ANALYSIS:
  !bootstrap=10000;
MODEL:
  ydiff on mdiff (b)
           mmeanc;
  [mdiff] (a);
  [ydiff] (cp);
  [mmeanc@0];
MODEL CONSTRAINT:
  new (direct indirect total);
  direct=cp;
  indirect=a*b;
  total=cp+a*b;
OUTPUT:
  !cinterval (bootstrap);
```

(*Appendix continues*)

As written, this Mplus code will produce inference for the indirect effect by estimating the standard error and producing a p value and confidence interval assuming the sampling distribution of $\hat{a}\hat{b}$ is normal. A bootstrap confidence interval for the indirect effect (and, indeed, all model parameters) using the percentile method and based on 10,000 bootstrap samples is generated by removing the exclamation points in the ANALYSIS and OUTPUT lines.

The grand-mean centering of the mean of the mediators is accomplished in the DEFINE line. This mean centering is not undertaken during the bootstrapping procedure. This does not affect the estimation of the paths that define the indirect effect(s), nor does it affect their bootstrap confidence interval(s). However, it will affect the bootstrap inference for the direct effect and the total effect constructed in the MODEL CONSTRAINT line. But because inference for the direct and total effects is generally not based on bootstrap methods, this is inconsequential in application. Nevertheless, the bootstrap confidence intervals for the total and direct effects that result from this code (and the code in the examples that follow) when bootstrapping is enabled should not be interpreted.

Parallel Multiple-Mediator Model

The Mplus code below estimates the parallel multiple-mediator model in Figure 4 for two mediators corresponding to the example in the text and generates a test of the difference between the two specific indirect effects. It can easily be modified to include more than two mediators and contrasts. Remove the exclamation points for bootstrap inference for the indirect effects and contrasts.

```
TITLE: Mplus code for the parallel multiple mediator model
DATA:
  file is 'c:\mplus\table 3.dat';
VARIABLE:
  names are hazard1 effect1 buy1 hazard2 effect2 buy2;
  usevariables m1diff m2diff ydiff m1meanc m2meanc;
DEFINE:
  m1diff=hazard2-hazard1;
  m2diff=effect2-effect1;
  ydiff=buy2-buy1;
  m1meanc=0.5*((hazard1+hazard2)-8.546);
  m2meanc=0.5*((effect1+effect2)-8.427);
ANALYSIS:
  !bootstrap=10000;
MODEL:
  ydiff on m1diff (b1)
           m2diff (b2)
           m1meanc m2meanc;
  [m1diff] (a1);
  [m2diff] (a2);
  [ydiff] (cp);
  [m1meanc@0];
  [m2meanc@0];
MODEL CONSTRAINT:
  new (direct ind1 ind2 totalind inddiff total);
  direct=cp;
  ind1=a1*b1;
  ind2=a2*b2;
  totalind=ind1+ind2;
  total=cp+ind1+ind2;
  inddiff=ind1-ind2;
OUTPUT:
!cinterval (bootstrap);
```

(*Appendix continues*)

Serial Multiple-Mediator Model

The code below estimates a serial multiple-mediator model with two mediators, as in Figure 6 and the example in the text. It also generates all possible pairwise comparisons between specific indirect effects. Remove the exclamation points for bootstrap inference for the indirect effects and contrasts.

```
TITLE: Mplus code for the serial multiple-mediator model
DATA:
  file is 'c:\mplus\table 3.dat';
VARIABLE:
  names are hazard1 effect1 buy1 hazard2 effect2 buy2;
  usevariables m1diff m2diff ydiff m1meanc m2meanc;
DEFINE:
  m1diff=hazard2-hazard1;
  m2diff=effect2-effect1;
  ydiff=buy2-buy1;
  m1meanc=0.5*((hazard1+hazard2)-8.546);
  m2meanc=0.5*((effect1+effect2)-8.427);
ANALYSIS:
  !bootstrap=10000;
MODEL:
  m2diff on m1diff (a3)
      m1meanc;
  ydiff on m1diff (b1)
      m2diff (b2)
      m1meanc m2meanc;
  [m1diff] (a1);
  [m2diff] (a2);
  [ydiff] (cp);
  [m1meanc@0];
  [m2meanc@0];
MODEL CONSTRAINT:
  new (direct ind1 ind2 ind3 totalind inddiff1 inddiff2 inddiff3 total);
  direct=cp;
  ind1=a1*b1;
  ind2=a2*b2;
  ind3=a1*a3*b2;
  totalind=ind1+ind2+ind3;
  total=cp+ind1+ind2+ind3;
  inddiff1=ind1-ind2;
  inddiff2=ind2-ind3;
  inddiff3=ind1-ind3;
OUTPUT:
!cinterval (bootstrap);
```

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