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**Moderation Analysis in Two-Instance Repeated Measures Designs:  
Probing Methods and Multiple Moderator Models**

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#### Author Note

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## Abstract

Moderation hypotheses appear in every area of psychological science, but the methods for testing and probing moderation in two-instance repeated-measures designs are incomplete. This article begins with a short overview of testing and probing interactions in between-participant designs. Next I review the methods outlined in Judd, McClelland, and Smith (1996) and Judd, Kenny, and McClelland (2001) for estimating and conducting inference on an interaction between a repeated-measures factor and a single between-participant moderator using linear regression. I extend these methods in two ways: first, by showing how to probe interactions in a two-instance repeated-measures design using both the pick-a-point approach and the Johnson-Neyman procedure. Second, I extend the models described by Judd et al. (1996) to multiple moderator models, including additive and multiplicative moderation. Worked examples with a published dataset are included to demonstrate the methods described throughout the article. Additionally, I demonstrate how to use Mplus and MEMORE (Mediation and Moderation in Repeated-Measures Designs, available at [akmontoya.com](http://akmontoya.com)), an easy-to-use tool available for SPSS and SAS, to estimate and probe interactions when the focal predictor is a within-participant factor, reducing the computational burden for researchers. I describe some alternative methods of analysis including structural equation models and multilevel models. I conclude by touching on some extension of the methods described in the paper and potentially fruitful areas of further research.

## Moderation Analysis in Two-Instance Repeated-Measures Designs:

### Probing Methods and Multiple Moderator Models

Across areas of experimental psychology and many other scientific fields, researchers are interested in questions which address the boundaries and contingencies of certain effects they observe. Do women feel more comfortable around men after learning their sexual orientation, or will it depend on whether the man is hetero- or homosexual (Russell, Ickes, and Ta, 2018)? Does fear-based advertisement always work, or will thinking about God make these methods less effective (Wu and Cutright, 2018)? Are all veterans equally likely to experience post-service stress, or will certain psychological characteristics impact the risk of stress (Mobbs and Bonanno, 2018)? These are all questions of *moderation* or *interaction*. Though some differentiate between these two terms, I will use them interchangeably (See VanderWeele, 2009 for a discussion of the differences from a causal modeling perspective). Statistical moderation analysis is used to test if the relationship between a *focal predictor*,  $X$ , and an *outcome variable*,  $Y$ , depends on some *moderator*,  $W$ . For example, Kraus and Callaghan (2016) found that higher class individuals were more likely to help than lower class individuals in public contexts, but the opposite was true when the context was private, where lower class individuals helped more than higher class individuals. Here, the relationship between class ( $X$ ) and helping ( $Y$ ) depends on context ( $W$ ). Learning has been shown to improve when adjunct questions are included in a text, but Roelle, Rahimkhani-Sagvand, Berthold (2017) found that when reading texts with adjunct questions, receiving immediate feedback ( $X$ ) had a detrimental effect on learning ( $Y$ ) for students who felt that answering the questions was highly demanding ( $W$ ). So, how is social class related to helping? Does immediate feedback lead to worse learning outcomes? It depends. Moderation analysis is a statistical method for testing whether these relationships depend on certain proposed variables (i.e., moderators).

In moderation analysis we test whether the relationship between the focal predictor ( $X$ ) and the outcome ( $Y$ ) depends on the moderator ( $W$ ). If the analysis suggests the answer is “Yes” the next natural question is “How?” An interaction can look many different ways, and the practical implications of significant interactions often depend on how the relationship between  $X$  and  $Y$  changes across the range of

$W$ . For example, the relationship between  $X$  and  $Y$  can increase as  $W$  increases or the relationship between  $X$  and  $Y$  can decrease as  $W$  increases. A hypothesis test of moderation would say the same thing for each of these patterns: “Yes there is significant moderation.” Because each pattern tells a different story, a follow-up analysis is required to interpret these effects.

One way to understand moderation is by estimating and probing *conditional effects*. A conditional effect is the effect of one variable on another, conditioned on a third. In analysis of variance, these are called *simple effects*. In moderation analysis, researchers are typically interested in the conditional effect of  $X$  on  $Y$  at different values of  $W$ . This helps researchers better understand how the relationship between  $X$  and  $Y$  changes as  $W$  changes. Probing an interaction gives us information about the nature of this changing relationship. For example, imagine you are researching how after-school science experience ( $X$ , e.g., in a science club) predicts performance in science classes ( $Y$ ) and whether the effect differs by gender ( $W$ ). If you find an interaction between experience and gender, you know that the effect of after-school science experience is different for males than females. The next questions you might ask are “Does after-school experience help boys but not girls?”, “Does it help girls but not boys?”, “after-school experience help both boys and girls, but the effect is stronger for one gender?” Probing the interaction can answer these questions. This is done by estimating the effect of  $X$  on  $Y$  at a certain point (or points) along the moderator, and testing if this effect is significantly different from zero. Directional tests can also be used to understand not just whether an effect is different from zero, but also whether it is positive or negative. Information about where effects are positive, indistinguishable from zero, and negative, helps you understand the pattern of effects across the moderator.

### **Rationale and Summary**

Moderation hypotheses can be investigated using a variety of experimental designs; however the methods for conducting moderation analysis are not equally developed in all designs. Here, I focus on two designs: between-participant designs (e.g., participants are randomly assigned to condition; participants are observed once on each outcome of interest) and two-instance repeated-measures designs (e.g., participants experience both conditions or are measured twice over time; participants are observed twice

on each outcome of interest). Both designs are very common in psychology and other behavioral sciences. The defining difference between these two designs is that each participant is observed on each outcome only once in between-participant designs. In contrast, repeated-measures designs observe each participant multiple times (e.g., over time, in multiple situations).

Methods for testing and probing interactions in between-participant designs have been established, and it has become typical for graduate students to learn how to conduct these analyses in an introductory regression course. Easy-to-use tools have been developed to help researchers conduct moderation analyses and probe interactions in between-participant designs (e.g., Hayes, 2018; Preacher, Curran, and Bauer, 2006). However, less is known about how to test moderation effects when either the moderator or the focal predictor is a within-participant factor. Judd et al. (1996, 2001) are the only treatments of this topic in a linear regression framework. These two papers discuss moderation of the effect of a repeated-measures treatment on some outcome by a variable measured once and assumed to be constant over instance (I call this a between-participant variable).

In this article, I begin by providing a short overview of testing and probing interactions in between-participant designs. Then I review the methods outlined in Judd et al. (1996, 2001) for estimating and conducting inference on interactions between repeated-measures factors and between-participant variables using a linear regression approach. The primary purpose of this paper is to extend the methods proposed by Judd et al. (1996, 2001) in two ways. First, I will explain how to probe interactions in a two-instance repeated-measures design, a topic which has not yet been discussed in the methodology literature. Second, I extend the Judd et al. (1996) method to multiple moderator models, including additive and multiplicative moderation. Using published data, I provide a one moderator example and a two moderator example, both with repeated-measures factors as focal predictors. Throughout the article, I demonstrate how to use MEMORE (Mediation and Moderation in Repeated-Measures Designs, available at [akmontoya.com](http://akmontoya.com)), an easy-to-use tool available for SPSS and SAS, to estimate and probe interaction effects where the focal predictor is a within-participant factor, reducing the computational burden for researchers. I also include Mplus code for estimating and probing these types of models. I conclude with

alternatives and extensions of the proposed models as well as future directions, which include more than two instances, alternative models of change, and moderated mediation in two-instance repeated-measures designs.

### **Advantages of Repeated-Measures Designs**

Repeated-measures designs often boast some statistical and conceptual advantages over between-participant designs. In particular, with a repeated-measures design each person can act as their own control. For instance, imagine a study where participants read two stories, one which was sad and one which was happy. For each story they rate how likely they would be to help the main character. Helping tendency can vary a lot from person to person, with some people willing to help just about anyone and others who are the opposite. Here, a repeated-measures design would be very advantageous because participants' helping response from the sad story can be compared to their helping response from the happy story. In a within-participant design, the statistical noise due to individual differences can be avoided, making the estimate of the effect of story valence (sad vs. happy) on helping much more precise. When it is feasible for participants to be observed in all potential conditions in the study (i.e., when participating in multiple conditions is possible without concerns of carryover effects), there is a distinct statistical power advantage in using a repeated-measures design.

A particular advantage of a within-participant design is the improved ability to observe causal effects on an individual. Like in between-participant designs specific assumptions are still required. When researchers are interested in cause and effect, they are typically interested in how some treatment might change an individual person. In a between-participants design, treatments are assigned to different sets of individuals and if treatment is randomly assigned, then group differences reflect a causal effect of the treatment. Here, inferential statements are limited to what a person *would have been like* if they had been in the other treatment condition, and this causal effect cannot be directly observed on any given individual. In a within-participant design, the information about what each participant would have been like in each condition is known. However, we do not know what each person would have been like if we had observed them in each condition in a different order. To observe causal effects we must assume there



are no carryover or order effects from the first observation to the second (i.e., causal transience), and there must be an assumption of temporal stability, which means that the observed measurements do not depend on *when* they are measured (Holland, 1986). One particular advantage of the design and analysis described in this paper, is if order is counterbalanced across individuals, then order can be tested as a between-subjects moderator the causal transience assumption can be tested statistically. However, it is important to note that traditional null hypothesis testing procedures cannot provide support for the claim of causal transience as this would involve accepting the null hypothesis, but rather these tests can be used to detect violations of this assumption. Testing procedures such as equivalence testing could potentially be used to support the assumption of causal transience. If treatment order is randomized and balanced the assumption of temporal stability is not required for estimates of the average causal effect to be unbiased (Josephy, Vansteelandt, Vanderhasselt, and Loeys, 2015; Senn, 2002).

### **Moderation in Between-Participant Designs**

Before discussing moderation in repeated-measures designs, I review the principles of testing and probing interactions in between-participant designs to connect methods from this design to those for repeated-measures designs. More extensive introductions to moderation analysis in between-participant designs can be found in Hayes (2018), Jaccard and Turrisi (2003), Aiken and West (1991), and many others. In a between-participant design, each participant is measured once on all variables involved: the focal predictor, moderator, and outcome variable.

In a standard multiple linear regression, the relationship between  $X$  and  $Y$  is constant with respect to  $W$ . Researchers can test for moderation by allowing the relationship between  $X$  and  $Y$  to be a function of  $W$ ,  $f(W_i)$ . In psychology, the form for  $f(W_i)$  is typically a linear function of the moderator,  $W$  (e.g.  $f(W_i) = b_1 + b_3W_i$ ).

$$\begin{aligned}
 Y_i &= b_0 + (b_1 + b_3W_i)X_i + b_2W_i + \epsilon_i \\
 &= b_0 + b_1X_i + b_2W_i + b_3X_iW_i + \epsilon_i
 \end{aligned}
 \tag{1}$$

In this model, the outcome variable for participant  $i$ ,  $Y_i$ , is a function of both participant  $i$ 's responses on focal predictor,  $X_i$ , and their response on the moderator,  $W_i$ . The error in estimating person  $i$ 's response

on  $Y_i$  with this combination of  $X_i$  and  $W_i$  is represented by  $\epsilon_i$ . The coefficient  $b_0$  corresponds to the intercept (predicted value of  $Y$  when  $X$  and  $W$  are both zero). The relationship between  $X$  and  $Y$  is a linear function of  $W$ :  $b_1 + b_3W_i$ . The coefficient  $b_2$  can be interpreted as the relationship between  $W$  and  $Y$  when  $X$  is zero. This equation can be estimated using any multiple linear regression program. When  $b_3$  is zero, the relationship between  $X$  and  $Y$  does not depend on  $W$  (i.e.,  $(b_1 + 0W) = b_1$ ). A test on  $\hat{b}_3$  is a test of moderation.

The symmetry property of moderation states that if the relationship between the focal predictor and the outcome depends on the moderator, this also means that the relationship between the moderator and the outcome variable depends on the focal predictor. By manipulating Equation 1, it is clear that  $b_3$  also measures the degree to which the relationship between  $W$  and  $Y$  depends on  $X$ .

$$Y_i = b_0 + b_1X_i + (b_2 + b_3X_i)W_i + \epsilon_i$$

If there is evidence of moderation, the researcher's focus will shift toward the pattern of effects. There are many ways that the effect of one variable on another can depend on a third, and probing the interaction helps describe that pattern. The function  $b_1 + b_3W_i$ , which I will denote  $\theta_{X \rightarrow Y}(W)$ , is the *conditional effect of X on Y*, which is a function of  $W$ . While probing an interaction,  $\theta_{X \rightarrow Y}(W)$  is estimated and inference is conducted at different values of  $W$ . The researcher can select values of  $W$  and use the estimate of the conditional effect and its standard error to test if the relationship between  $X$  and  $Y$  is significantly different from zero at that value of  $W$ . There are two primary methods of probing an interaction: the pick-a-point approach and the Johnson-Neyman procedure.

Probing analyses are typically most informative when a test of interaction is significant, though there may be justifiable reasons why a researcher would want to probe an interaction that is not significant. A hypothetical example might be if there were a strong existing literature which supports the relationship between  $X$  and  $Y$  in one group (e.g., heterosexual couples), but there is little known about another other group (e.g., homosexual couples). If the couple's sexual orientation is the moderator, it might be worth examining the relationship between  $X$  and  $Y$  in the heterosexual group to confirm that

previous results are replicated. A brief warning: researchers who probe non-significant interactions often find themselves grappling with explaining seemingly contradictory results. For example, it may be that the relationship between  $X$  and  $Y$  is not significantly moderated by sexual orientation of the couple. However, when probed the analysis might show the relationship between  $X$  and  $Y$  is significantly different from zero for heterosexual couples, but not significantly different from zero for homosexual couples. It is important to remember that a *difference in significance does not imply significantly different*. One conditional effect may be significantly different from zero and other may not. This does not mean that these two conditional effects are significantly different from each other.

### **Pick-a-point**

The pick-a-point approach (i.e., simple-slopes analysis or spotlight analysis) is a method for probing an interaction by selecting a value of the moderator then estimating and conducting inference on the conditional effect of the focal predictor on the outcome at that specific value of  $W$ . This helps to answer the question “Is there an effect of  $X$  on  $Y$  at  $w$ ?” where  $w$  is a specific point on  $W$ .

The ratio of the estimate  $\hat{\theta}_{X \rightarrow Y}(W)$  to its standard error can be compared to values from a  $t$ -distribution with  $n - q - 1$  degrees of freedom, where  $n$  is the total number of participants and  $q$  is the number of predictors in the regression equation used to estimate the coefficients and standard errors. By comparing to the  $t$ -distribution a  $p$ -value can be calculated, or the critical  $t$ -value can be used to calculate a confidence interval.

The pick-a-point approach can be used for either dichotomous or continuous moderators. The choice of points to probe in the pick-a-point approach is very clear when the moderator is dichotomous. However, when the moderator is continuous the choice is often arbitrary. It has traditionally been recommended to select the mean of the moderator as well as the mean plus and minus one standard deviation (Aiken & West, 1991). However, these points may or may not be within the observed range of the data. Hayes (2018) recommends probing at percentiles (e.g. 16<sup>th</sup>, 50<sup>th</sup>, and 84<sup>th</sup>) to guarantee that the probed points are always within the observed range of the data. There are also instances where certain points on the scale are particularly informative. For example, if the moderator is body mass index then

18.5, 25, and 30 might be good points to probe as they indicate the boundaries between underweight, normal, overweight, and obese. For detailed discussions of the pick-a-point approach for between-participant designs see Hayes and Matthes (2009), Aiken and West (1991), Hayes (2013), and Spiller, Fitzsimons, Lynch Jr., and McClelland (2013).

### **Johnson-Neyman Procedure**

The Johnson-Neyman procedure is another approach to probing interactions with continuous moderators (Johnson & Neyman, 1936; Johnson & Fay, 1950). This method removes the arbitrary choice of points along the moderator, and instead this method identifies important transition points (i.e., *boundaries of significance*) where the effect of the  $X$  on  $Y$  transitions from significant to non-significant or vice versa. The procedure uses the same point estimate and standard error as the pick-a-point approach. However, rather than selecting a value of  $W$ , and calculating the associated  $t$ -statistic, the Johnson-Neyman procedure selects an  $\alpha$ -value and the associated critical  $t$ -value, then solves for the values of  $W$  such that the conditional effect of  $X$  on  $Y$  is exactly significant at the pre-selected  $\alpha$ -value. This is done by setting the ratio of  $\hat{\theta}_{X \rightarrow Y}(W)$  to its standard error equal to the critical  $t$  and solving for  $W$ .

Solving for  $W$  involves finding the roots of a polynomial equation, and as such the Johnson-Neyman procedure is limited to continuous moderators (Hayes & Matthes, 2009). The solutions can be imaginary or outside of the range of the observed data. Methodologists do not recommend interpreting these solutions (Hayes & Matthes, 2009; Preacher, Curran, & Bauer, 2006; Spiller et al., 2013). By finding the transition points which lie within the observed data, this method allows the researcher to understand the patterns of significance across the entire range of the moderator, rather than at arbitrarily selected points.

### **Moderation in Two-Instance Repeated-Measures Designs**

Moderation analysis and probing have been widely described in between-participant designs, resulting in an increasing use of methods and new tools that made probing interactions easier (Hayes & Matthes, 2009; Hayes, 2013). Recent advances in probing have generalized these methods to multilevel modeling and latent curve analysis (Bauer & Curran, 2005; Preacher et al., 2006). However, the two-

instance repeated-measures design has been largely ignored when it comes to probing methods. Judd et al. (1996, 2001) described methods for estimating and testing if there is an interaction, and these methods have been used across areas of psychology to investigate questions of moderation. For example, among students with math difficulties, those with higher working memory capacity benefited more from strategy training (pre-to-posttest) than those with lower working memory capacity (Swanson, Lussier, and Orosco, 2015). In another example, dyads were assigned to complete a paired task and partners were randomly assigned to high or low power roles. Dyad members in high power positions were more likely to dehumanize the low power participant than vice versa, particularly when the high power individual offers fewer resources to the low power individual (Gwinn, Judd, and Park, 2013).

In this section I will review the methods described by Judd et al. (1996, 2001) for testing if there is moderation by some between-participant variable of the effect of a repeated-measures factor on an outcome. I refer to the repeated-measures factor as either “instance” or “condition” with the understanding that what differentiates the repeated measurements does not need to be an experimental manipulation. For example, in the dataset used in this paper, participants were measured at two time points: before and after treatment. The between-participant variable can be something that is randomly assigned (e.g., experimental condition) or it can be something that is observed, but assumed to be constant across instances of repeated-measurements (e.g., eye color). I’ve created a macro available for SPSS and SAS which both estimates the model required to test moderation and gives extensive output related to the probing methods described in this paper. This tool, MEMORE, is meant to ease the computational burden of conducting a thorough moderation analysis in the two-instance repeated-measures design. Though not described in this manuscript, MEMORE can also be used to estimate mediation models in two-instance repeated-measures designs (for instructions and examples see [akmontoya.com](http://akmontoya.com) and Montoya & Hayes, 2017).

### **Running Example**

Lasselín, Kemani, Kanstrup, Olsson, Axelsson, Andreasson, Lekander, and Wicksell (2016) investigated whether baseline inflammation moderated the effectiveness of behavioral treatment for

chronic pain. They were particularly interested in whether the treatment was less effective for individuals with higher baseline inflammation. Patients with chronic pain were recruited to the study, which involved 12 weekly sessions using one of two behavioral treatments for chronic pain: Acceptance and Commitment Therapy (ACT) or Applied Relaxation (AR). Participants reported their pain on a 6-point scale before starting treatment and after completion of the sessions. Baseline inflammation was measured by taking assays of two pro-inflammatory markers (IL-6 and TNF- $\alpha$ ). The concentrations of these markers were log transformed to improve linearity and combined using principal components analysis. The analyses in this article differ slightly from the analyses in Lasselin et al. (2016). A single outlier was dropped for all analyses in this paper. All tests are conducted at  $\alpha = .05$ . For the original analysis and other variables measured in the study, see Lasselin et al. (2016). Data for this example is available for download at [www.akmontoya.com](http://www.akmontoya.com). All analyses are described as if the data are in wide-form (each row of the dataset represents a participant) rather than long form (each row of the dataset represents a participant in a specific instance).

### **Testing the Interaction**

An interaction means that the slope which describes the relationship between the focal predictor and the outcome depends on some other variable, a moderator. Judd et al. (1996, 2001) use this idea of varying slopes to outline the following method for testing interactions between a between-participant variable and a repeated-measures treatment. This procedure begins with a model of the outcome variable  $Y$  predicted by  $W$ , the between-participant variable, where the regression weights for this model are allowed to vary by condition or instance (denoted using the subscript  $j$ ):

$$Y_{ij} = b_{0j} + b_{1j}W_i + \epsilon_{ij}$$

$Y_{ij}$  is the measure of the outcome  $Y$  for participant  $i$  in condition or instance  $j$ . The measurement of participant  $i$  on the between-participant variable is denoted  $W_i$ . Notice that this measurement does not have a subscript  $j$  because it is not measured repeatedly, but rather assumed to be constant regardless of

instance or condition. The intercept and regression weight for  $W_i$  are represented by  $b_{0j}$  and  $b_{1j}$  respectively. Note that if there are two instances, there are two  $b_{0j}$ 's:  $b_{01}$  and  $b_{02}$ . Similarly, there would be two  $b_{1j}$ 's. The intercept and slope are allowed to differ by condition. The  $\epsilon_{ij}$ 's are the errors in estimation for participant  $i$  in condition  $j$ , and are assumed to be normally distributed with mean zero, variance  $\sigma_j^2$ , correlation  $\rho$  for observations from the same participant, and correlation of 0 for observations from different participants.

In the case of two conditions there are two models, one for each outcome variable:

$$Y_{i1} = b_{01} + b_{11}W_i + \epsilon_{i1} \quad (2)$$

$$Y_{i2} = b_{02} + b_{12}W_i + \epsilon_{i2} \quad (3)$$

The coefficient  $b_{11}$  represents the relationship between  $W$  and  $Y$  in the first condition. The coefficient  $b_{12}$  represents the relationship between  $W$  and  $Y$  in the second condition. When  $b_{11} \neq b_{12}$  the relationship between  $W$  and  $Y$  depends on condition (i.e. there is an interaction between condition and  $W$ ). To test a moderation hypothesis we test if  $b_{11}$  is equal to  $b_{12}$ . By subtracting Equation 2 from Equation 3, the coefficient for  $W$  reflects the difference between  $b_{11}$  and  $b_{12}$ <sup>1</sup>.

$$Y_{i2} - Y_{i1} = b_{02} - b_{01} + (b_{12} - b_{11})W_i + (\epsilon_{i2} - \epsilon_{i1})$$

$$Y_{Di} = b_0 + b_1W_i + \epsilon_i \quad (4)$$

Regress the difference of the  $Y$  variables  $Y_{i2} - Y_{i1} = Y_{Di}$  onto  $W$ , and if  $b_1$  is significantly different from zero, we conclude there is evidence for an interaction between  $W$  and condition. This implies that  $b_{11}$  and  $b_{12}$  are not equal, which means the relationship between  $W$  and  $Y$  depends on the condition. This matches the intuitive understanding of an interaction. Equation 4 is referred to as a *simple moderation model*, where “simple” refers to their being one moderator,  $W$  (similar to “simple regression” referring to one predictor). The symmetry property holds in this model: support for the claim that the relationship between

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<sup>1</sup> The residuals in this equation ( $\epsilon_{i2} - \epsilon_{i1}$ ) are still normally distributed with constant variance. Assume that the correlation between  $\epsilon_{i1}$  and  $\epsilon_{i2}$  is  $\rho$  for all  $i = j$ , and zero for all  $i \neq j$  then  $\epsilon_{i2} - \epsilon_{i1} \sim N(0, \sigma_1^2 + \sigma_2^2 - 2\rho\sigma_1\sigma_2)$ .

condition and  $Y$  depends on  $W$  is the equivalent to saying the relationship between  $W$  and  $Y$  depends on condition.

**Estimation with MEMORE.** In Lasselin et al. (2016), the researchers were interested in whether the effect of behavioral treatment for chronic pain on pain intensity depended on baseline inflammation. MEMORE can be used to estimate this regression. One advantage of MEMORE is that the researcher does not need to create the difference score as an additional variable. Rather, after installing MEMORE, the researcher can input the two outcome variables using a MEMORE command line. If the variables for pre-treatment pain and post-treatment pain are stored as `PrePain` and `PostPain`, respectively, and the variable for pre-treatment inflammation is saved as `inflamm` then the MEMORE command for this analysis in SPSS would be

```
MEMORE y = PrePain PostPain /w = inflamm /model = 2 /wmodval1 = 0.85 /jn =1.
```

The command MEMORE calls the MEMORE macro. Instructions for installing the MEMORE macro can be found at [akmontoya.com](http://akmontoya.com). The variables in the `y` list are used as the outcome variables. The difference score is taken in the order that the variables are inputted, in this case `PrePain - PostPain`. The variable in the `w` argument is the moderator, in this case `inflamm`. When there is only one moderator the researcher can use either `Model = 2` or `Model = 3`, the results will not differ. The two additional commands will be explained in the probing section. See model templates and documentation at [akmontoya.com](http://akmontoya.com) for more detail and instructions for SAS.

See Figure 1 for SPSS output. Note that the `inflamm` variable is mean-centered such that zero is the sample average. The first section of the output has information about the model, variables, calculated variables, and sample size. This section should always be looked over carefully to make sure the intended model is estimated.

The second section of the output includes the results of estimating Equation 4. Overall model information includes estimated  $R^2$  and a test of overall variance explained. The second part of this section is a table of all the regression coefficients and their associated significance tests and confidence intervals.



Each row corresponds to the results for the predictor denoted in the left column, where “constant” denotes the intercept. The estimated regression equation is

$$\hat{Y}_{pre} - \hat{Y}_{post} = \hat{Y}_D = 0.20 - 0.40W_i \quad (5)$$

The estimate of the intercept,  $\widehat{b}_0 = .20$ , means that when the inflammation score is 0 (the sample mean for inflammation) then the expected difference in pain is 0.20. Pain is expected be 0.20 units lower after treatment, but this effect is not significantly different from zero,  $t(38) = 1.06, p = .30$ . Additionally, for each unit increase in inflammation, there is a 0.40 unit decrease in the *difference in pain*,  $t(38) = -2.12, p = .04$ . The difference was constructed by subtracting post-treatment pain from pre-treatment pain, so lower scores reflect greater post-treatment pain relative to pre-treatment pain. If the treatment is considered effective when post-treatment pain is low relative to pre-treatment pain, then the treatment is less effective for individuals with higher baseline inflammation.

This information alone is very useful, but the researchers may have additional questions. Is the treatment still effective for people with high inflammation, just less so? How much is inflammation related to pre-treatment or post-treatment pain? These questions can be answered by probing the interaction. Next, I will discuss how to probe an interaction between a repeated-measures factor and between-participant variable.

### **Probing the Interaction**

Just as in between-participant designs, the simple-slopes and Johnson-Neyman procedures can be used to probe moderation effects in two-instance repeated-measures designs, though they have not been described in this context before. There are two relationships which can be probed: the effect of condition on the outcome variable at different values of  $W$ , and the effect of  $W$  on the outcome variable in different conditions. I will treat each of these separately, describing both the simple-slopes method and Johnson-Neyman procedure where they apply.

**Probing effect of condition on the outcome.** Researchers may be interested in estimating and conducting inference on the effect of condition at specific values of the between-participant variable  $W$

(e.g., estimating the expected difference, pre-treatment to post-treatment, in pain for an individuals with a specific inflammation score). How well does this treatment work for those who have relatively high inflammation, or for those with relatively low inflammation? A test of interaction examines if the difference in pain differs for those with varying levels of inflammation. However, a test of interaction does not estimate the treatment effect for individuals with a specific score on inflammation. Do people with high inflammation show little difference in pain from treatment, because inflammation reflects a high physical irritation which may not be relieved by behavioral interventions? Do people with low inflammation benefit from the intervention? These questions can be tested using probing, by estimating the effect of condition at specific values of the between-participant variable using the simple-slopes method. Alternatively, regions of significance can be defined using the Johnson-Neyman method. This analysis would show where along the between-participant variable there are significant effects of condition on the outcome and where these effects are not statistically significant.

**Simple slopes.** The simple-slopes method relies on choosing a point on the between-participant variable  $W$ , say  $w$ , then estimating the effect of condition on the outcome at the specific value  $W = w$ . Based on Equation 4, the best estimate of the effect of condition on the outcome at a specific value of  $W$  is  $\hat{\theta}_{C \rightarrow Y}(W) = \hat{b}_0 + \hat{b}_1 W$ , where  $C$  denotes condition and  $\hat{\theta}_{C \rightarrow Y}(W)$  denotes the estimated effect of condition on the outcome variable  $Y$  as a function of  $W$ . This is the estimate of the difference in the outcome variables between conditions at a specific value of  $W$ . The variance of  $\hat{\theta}_{C \rightarrow Y}(W)$  can be estimated as<sup>2</sup>

$$\widehat{var}(\hat{\theta}_{C \rightarrow Y}(W)) = \widehat{var}(\hat{b}_0) + W^2 \widehat{var}(\hat{b}_1) + 2W \widehat{cov}(\hat{b}_0, \hat{b}_1).$$

The estimated variance of  $\hat{\theta}_{C \rightarrow Y}(W)$  is a function of the chosen value of the moderator,  $W$ , the estimated variance of  $\hat{b}_0$ ,  $\widehat{var}(\hat{b}_0)$ , the estimated variance of  $\hat{b}_1$ ,  $\widehat{var}(\hat{b}_1)$ , and the estimated covariance between  $\hat{b}_0$  and  $\hat{b}_1$ ,  $\widehat{cov}(\hat{b}_0, \hat{b}_1)$ . The estimates of the variances and covariances of the regression coefficients are

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<sup>2</sup> Under the assumptions of linear regression  $\hat{b}_0$  and  $\hat{b}_1$  are normally distributed. If  $X$  and  $Y$  are normally distributed,  $aX + bY$  is normally distributed with variance  $a^2 var(X) + b^2 var(Y) + 2ab cov(X, Y)$ .

available through most statistical packages that estimate regression models. However, typical programs used to conduct regression analysis will not calculate  $\hat{\theta}_{C \rightarrow Y}(W)$  or  $\widehat{Var}(\hat{\theta}_{C \rightarrow Y}(W))$  without additional work by the researcher.

The ratio of the estimate of  $\theta_{C \rightarrow Y}(W)$  to its standard error is  $t$ -distributed with  $n - q - 1$  degrees of freedom, where  $n$  is the number of observations and  $q$  is the number of predictors in the regression model. In the case of Equation 4,  $q = 1$ . Specific values of  $W$  can be plugged into the equation for  $\hat{\theta}_{C \rightarrow Y}(W)$  and its  $\widehat{Var}(\hat{\theta}_{C \rightarrow Y}(W))$ . The ratio  $\frac{\hat{\theta}_{C \rightarrow Y}(W)}{\sqrt{\widehat{Var}(\hat{\theta}_{C \rightarrow Y}(W))}}$  can be calculated and compared to a critical  $t$ -statistic with the appropriate degrees of freedom. Alternatively, a  $p$ -value can be calculated from the calculated  $t$ -statistic.

*Simple slopes with MEMORE.* In the chronic pain example, probing the effect of instance on the outcome at values of the between-participant moderator means estimating the effect of treatment on pain at different values of baseline inflammation. Suppose the researchers are particularly concerned with knowing if the treatment is still effective for those with high inflammation. They could choose to probe the effect of treatment on pain at the 80<sup>th</sup> percentile of inflammation, which corresponds to a score of 0.85 on the inflammation measure. Based on the regression results, an estimate of the effect of treatment on pain levels conditional on inflammation can be calculated at 0.85. The estimates of the intercept and regression coefficient for inflammation can be drawn from the regression results in Equation 5 or Figure 1.

$$\hat{\theta}_{C \rightarrow Y}(0.85) = 0.20 - 0.40(0.85) = -.14$$

This means that individuals who score 0.85 on inflammation are expected to have post-treatment pain levels 0.14 points higher than their pre-treatment pain levels. But is this difference statistically significant? First the variance of the estimate of the conditional effect must be estimated. The variances of each of the regression coefficients are just the squares of their standard errors, which can be extracted from the results in Figure 1. The covariance between  $\widehat{b}_0$  and  $\widehat{b}_1$  is exactly zero.

$$\widehat{Var}(\hat{\theta}_{C \rightarrow Y}(0.85)) = 0.19^2 + 0.85^2 0.19^2 + 2(0.85)(0) = 0.062$$

$$\frac{\hat{\theta}_{C \rightarrow Y}(0.85)}{\sqrt{\widehat{Var}(\hat{\theta}_{C \rightarrow Y}(0.85))}} = \frac{-0.14}{\sqrt{0.062}} = -0.56$$

The probability that a  $t$ -statistic with 38 degrees of freedom is as extreme or more extreme than 0.56 is  $p = .58$ . All these calculations can be done in MEMORE by including the `wmodval1 = 0.85` argument in the command line. Figure 2 denotes the specific section of the output which corresponds to this command, including a table similar to the table in the previous section with estimates of the conditional effect of condition on the outcome at the requested values of the moderator and accompanying information for inference. In addition, MEMORE probes at the mean as well as plus and minus one standard deviation from the mean of the moderator by default (see Figure 1; heading: “Conditional Effect of X on Y at values of the moderator(s)”). See documentation for additional probing options. The obvious follow-up question after probing at this specific point is then, for what values of inflammation is there a statistically significant effect of treatment on pain?

**Johnson-Neyman Procedure.** Just as in between-participant moderation, the ratio of  $\hat{\theta}_{C \rightarrow Y}(W)$  to its standard error can be used to calculate the point(s) along the range of  $W$  where the ratio is exactly equal to the critical  $t$ -value. These points mark the boundaries of significance for the relationship between condition and the outcome. By solving for these points, the Johnson-Neyman technique defines the pattern of significance for the relationship between condition and the outcome across the entire range of  $W$ .

By setting the absolute value of the ratio of  $\hat{\theta}_{C \rightarrow Y}(W)$  to its standard error equal to value of the critical  $t$ -value, and solving for  $W$ , these points can be found using basic algebra. The critical  $t$ -value will be denoted as  $t_{\frac{\alpha}{2}, df}^*$ . Ratios greater than  $t_{\frac{\alpha}{2}, df}^*$  will be significant at level  $\alpha$ .

$$t_{\frac{\alpha}{2}, n-q-1}^* = \left| \frac{\hat{\theta}_{C \rightarrow Y}(W)}{\sqrt{\widehat{Var}(\hat{\theta}_{C \rightarrow Y}(W))}} \right|$$

$$t_{\frac{\alpha}{2}, n-q-1}^* = \left| \frac{\widehat{b}_0 + \widehat{b}_1 W}{\sqrt{\widehat{var}(\widehat{b}_0) + W^2 \widehat{var}(\widehat{b}_1) + 2W c\widehat{ov}(\widehat{b}_0, \widehat{b}_1)}} \right|$$

Squaring both sides eliminates the absolute value sign.

$$t_{\frac{\alpha}{2}, n-q-1}^{*2} = \frac{(\widehat{b}_0 + \widehat{b}_1 W)^2}{\widehat{var}(\widehat{b}_0) + W^2 \widehat{var}(\widehat{b}_1) + 2W c\widehat{ov}(\widehat{b}_0, \widehat{b}_1)}$$

Rearrange terms to get a quadratic form,

$$0 = \left( b_0^2 - t_{\frac{\alpha}{2}, n-q-1}^{*2} \widehat{var}(\widehat{b}_0) \right) + \left( 2\widehat{b}_1 \widehat{b}_0 - 2t_{\frac{\alpha}{2}, n-q-1}^{*2} c\widehat{ov}(\widehat{b}_0, \widehat{b}_1) \right) W + \left( \widehat{b}_1^2 - t_{\frac{\alpha}{2}, n-q-1}^{*2} \widehat{var}(\widehat{b}_1) \right) W^2.$$

The quadratic formula can be used to show that the solutions to this equation are

$$W_{JNk} = \frac{-\left( 2\widehat{b}_1 \widehat{b}_0 - 2t_{\frac{\alpha}{2}, n-q-1}^{*2} c\widehat{ov}(\widehat{b}_0, \widehat{b}_1) \right) \pm \sqrt{\left( 2\widehat{b}_1 \widehat{b}_0 - 2t_{\frac{\alpha}{2}, n-q-1}^{*2} c\widehat{ov}(\widehat{b}_0, \widehat{b}_1) \right)^2 - 4 \left( \widehat{b}_0^2 - t_{\frac{\alpha}{2}, n-q-1}^{*2} \widehat{var}(\widehat{b}_0) \right) \left( \widehat{b}_1^2 - t_{\frac{\alpha}{2}, n-q-1}^{*2} \widehat{var}(\widehat{b}_1) \right)}}{2 \left( \widehat{b}_1^2 - t_{\frac{\alpha}{2}, n-q-1}^{*2} \widehat{var}(\widehat{b}_1) \right)}$$

In its mathematical form, there are always two solutions to this problem; however, these two solutions are not always interpretable. Just as in the between-participant case, solutions can be imaginary or fall outside of the range of the observed data, neither of which should be interpreted. Even when transition points are found within the range of the data, it is important to note how much of the data is above or below these points in order to determine how much to trust them. Without data surrounding the Johnson-Neyman points, there is no evidence that the observed trend continues outside of the range of the observed data, and thus no evidence that these points are either meaningful or interpretable.

The equation above looks fairly tedious to implement by hand, and computing these values by hand could result in rounding errors. Regardless, there is a closed form solution for these points, and these points of transition can be found using MEMORE.

*Johnson-Neyman with MEMORE.* In the chronic pain example, it may be useful to find the scores on inflammation such that the treatment is effective at reducing pain, based on a statistically significant difference. MEMORE calculates the Johnson-Neyman points of transition and prints a table of probed values to help researchers understand what ranges of the moderator show significant (and non-significant)

effects of condition on the outcome. Including the  $jn = 1$  in the command line calls for the Johnson-Neyman procedure. Figure 1 is an example of the output with the chronic pain data. The critical  $t$ -statistic for 38 degrees of freedom and  $\alpha = .05$  (the default) is 2.02, so the two solutions for the transition points are

$$JN_1 = -0.63; JN_2 = 11.79.$$

The second point is well out of the observed range of inflammation (Range = -1.73 to 2.14); however, the first point is within the observed range of the data. The first part of the “Johnson-Neyman Procedure” section in the MEMORE output includes the Johnson-Neyman points of transition as well as the percentage of the data which falls above that point. MEMORE does not print any Johnson-Neyman solutions which are outside of the observed range of data. Since one of the solutions, 11.79, was out of the range of the data, MEMORE only printed one Johnson-Neyman solution. The point -0.63 is the transition of two regions, significant to non-significant. The second part of this section of the output is a table of probed values that helps give researchers a sense of the pattern of effects across the range of the moderator. The table indicates that points above -0.63 are significant and those below are non-significant.

A helpful way to use the Johnson-Neyman results is to graph the conditional effect of treatment on pain across the range of the moderator, inflammation. If a confidence interval is included around this line, it is easy to tell that the Johnson-Neyman transition points are the points where the confidence interval around the conditional effect intersects zero on the  $Y$ -axis, marking no significant effect of treatment on condition (See Figure 2). This kind of visualization can be helpful in understanding an interaction. Because a negative effect of treatment on condition means that post-treatment pain is higher than pre-treatment pain ( $Y_D < 0$ ), scores below zero indicate an ineffective treatment or even a treatment which has the opposite effect than hoped. Positive scores mean that pain after treatment is lower than pain before treatment. Based on Figure 2, those with inflammation scores below -0.63 are expected to show significant improvement (reduction) in pain after the treatment.

**Probing effect of moderator on the outcome.** Estimating the relationship between the between-participant variable  $W$  and the outcome in each of the different conditions is much simpler than probing

the effect of condition on the outcome. In Equations 2 and 3,  $b_{11}$  and  $b_{12}$  represent the relationship between  $W$  and  $Y$  in the first and second conditions, respectively. There is no need to condition on a specific value of a variable then derive the variance of the conditional estimate. By estimating Equations 2 and 3 separately,  $\hat{b}_{11}$  and  $\hat{b}_{12}$  and their corresponding hypothesis tests are conditional estimates of the relationship between  $W$  and  $Y$  and tests of whether these relationship are different from zero. This is equivalent to the simple-slopes method and can conveniently be conducted in any regression program.

Probing the effect of  $W$  on  $Y$  is automatically conducted in MEMORE (see Figure 1; heading: “Conditional Effect of Moderator(s) on  $Y$  in each Condition”). Consider two individuals who are one unit different from each other on baseline inflammation. The individual with higher baseline inflammation is expected to have 0.0293 units higher in pain levels at baseline. This effect is not significantly different from zero,  $t(38) = 0.20$ ,  $p = .84$ . However, in Condition 2 (i.e., post-treatment measurement period), a one unit difference in baseline inflammation is related to a 0.43 unit difference in post-treatment pain levels, where higher baseline inflammation is related to higher post-treatment pain levels. This effect is significantly different from zero,  $t(38) = 2.22$ ,  $p = .03$ . This is particularly interesting because pre-treatment pain and baseline inflammation were both measured in the same visit. One might expect that pre-treatment inflammation would be more related to pain before treatment than after. However, this shows clear evidence that the relationship is stronger and more positive for post-treatment pain, suggesting there may be some aspect of the treatment that is less effective for participants with high baseline inflammation. The Johnson-Neyman method cannot be conducted because condition is not a continuous variable.

This completes our discussion of probing interactions between a repeated-measures factor and a between-participant variable in two-instance repeated measures designs. Next we move to discussion of models which incorporate multiple moderators. In this section, I use additional potential moderators from Lasselin et al. (2016) as an example.

### **Multiple Moderator Models**

Judd et al. (1996, 2001) introduced the regression-based approach to testing interactions between a repeated-measures factor and a single between-participant variable. However, they did not generalize beyond a single moderator. When I refer to multiple moderators I mean two or more distinct variables which could impact the relationship between  $X$  and  $Y$ , rather than the same moderator measured multiple times. Extensions to multiple moderator models are very important as currently researchers often test a variety of single moderator models separately or conduct subgroup analyses (e.g., Blanco, Barberia, & Matute, 2015; Buunk, Ybema, Van Der Zee, Schaufeli, & Gibbons, 2001). Using separate models or subgroups analysis results in issues of confounding of interaction effects, and does not allow simultaneous testing of multiple moderators and thus can result in imprecise theories about moderation. Testing multiple moderators all together is more parsimonious, and gives the most detailed picture of how the moderators together interact with the focal predictor and each other to predict the outcome. Lasselin et al. (2016) posited that the degree to which effectiveness of treatment depends on inflammation might itself depend on whether the participants received AR or ACT (a three way interaction), but did not have the tools available to test this hypothesis. We will test that question in this section.

Multiple moderator models can be described in two types. The first type is additive moderation which involves multiple two-way interactions (See Table 1 for a comparison of simple, additive, and multiplicative moderation models). Just as in the simple moderation model (Equation 4) the researcher needs two observations of the outcome (one in each instance) as well as a single observation on each of the moderators. The moderator variables can be randomized (e.g., type of therapy) or observed variables (e.g., baseline inflammation). In additive moderation the moderators are not allowed to interact with each other. In multiplicative moderation, interactions among moderators are included. For example if there were two moderators, all two-way interactions would be included, and the three-way interaction between  $X$ ,  $W_1$ , and  $W_2$  would be included (Hayes, 2018). In this section, I generalize the simple moderation model for two-instance repeated-measures designs to models with two moderators, though these methods can be generalized to any number of moderators. MEMORE will estimate and test additive and



multiplicative moderation with up to five moderators. I provide an example using the data from Lasselin et al. (2016) with multiple moderators.

### Additive Moderation

Just as in Judd et al (1996, 2001) the additive model begins with a model for each outcome in each condition. Additive moderation suggests that the effect of each moderator,  $W_1$  and  $W_2$ , depends on condition, but the effect of each moderator does not depend on the other moderator.

$$Y_{i1} = b_{01} + b_{11}W_{1i} + b_{21}W_{2i} + \epsilon_{i1}$$

$$Y_{i2} = b_{02} + b_{12}W_{1i} + b_{22}W_{2i} + \epsilon_{i2}$$

The effect of each moderator is allowed to vary by condition, but the effect of each moderator is not a function of the other moderator (i.e., the moderators do not interact with each other). Taking the difference between these equations allows us to test a moderation hypothesis.

$$Y_{i2} - Y_{i1} = b_{02} - b_{01} + (b_{12} - b_{11})W_{1i} + (b_{22} - b_{21})W_{2i} + \epsilon_{i2} - \epsilon_{i1}$$

$$Y_{Di} = b_0 + b_1W_{1i} + b_2W_{2i} + \epsilon_i \tag{6}$$

Equation 6 contains coefficients of greatest interest  $b_1 = b_{12} - b_{11}$  and  $b_2 = b_{22} - b_{21}$ , as these reflect the degree to which condition moderates the effect of each moderator. By symmetry, this is also how each moderator impacts the effect of condition on the outcome. Hypothesis tests on the estimates of  $b_1$  and  $b_2$  will indicate if the effects of  $W_1$  and  $W_2$  on  $Y$  differ by condition, respectively. Again, the symmetry argument applies: if  $\hat{b}_1$  and  $\hat{b}_2$  are significantly different from zero, this indicates that the effect of condition depends on  $W_1$  and  $W_2$ , respectively. This method can be generalized to any number of moderators.

A particularly useful application of additive moderation is when there is a single conceptual moderator, but it is multicategorical (more than two groups). In this case, the multicategorical moderator can be coded into  $k-1$  variables, where  $k$  is the number of groups, using a coding system such as indicator or Helmert coding. Each of these variables can be included as a separate moderator, and the test of  $R^2$  would be a test of omnibus moderation (Hayes & Montoya, 2017).

Multiple moderator models can be probed at different sets of values of the moderators or in difference conditions, just as a single moderator model can be probed. In additive moderation the conditional effect of each moderator remains relatively simple, whereas the conditional effect of condition becomes more complex. For example, in the case of two moderators the conditional effect of  $W_1$  would be

$$\theta_{W_1 \rightarrow Y}(C) = b_{1c}$$

and the variance would be  $var(b_{1c})$ . Still the effect of  $W_1$  depends the condition  $C$ , but it does not depend on the other moderator  $W_2$ . The conditional effect of condition would be

$$\theta_{C \rightarrow Y}(W_1, W_2) = b_0 + b_1 W_1 + b_2 W_2.$$

The standard errors for these effects become more complex as the number of moderators increase.

However, they are easily derived using matrix algebra, and these methods generalize to any number of moderators. Consider the coefficients from the model as a vector  $\vec{b}$  where

$$\vec{b} = [b_0 \quad b_1 \quad b_2].$$

Additionally, consider the covariance matrix of the parameter estimates to be  $\Sigma$  where

$$\Sigma = \begin{bmatrix} var(\widehat{b_0}) & cov(\widehat{b_0}, \widehat{b_1}) & cov(\widehat{b_0}, \widehat{b_2}) \\ cov(\widehat{b_0}, \widehat{b_1}) & var(\widehat{b_1}) & cov(\widehat{b_1}, \widehat{b_2}) \\ cov(\widehat{b_0}, \widehat{b_2}) & cov(\widehat{b_1}, \widehat{b_2}) & var(\widehat{b_2}) \end{bmatrix}$$

The parameter  $\theta_{C \rightarrow Y}(W_1, W_2)$  is a linear combination of the parameters where the vector which defines that combination can be called  $\vec{l}$ , where

$$\vec{l} = [1 \quad W_1 \quad W_2]$$

such that

$$\theta_{C \rightarrow Y}(W_1, W_2) = \vec{b} \vec{l}' = [b_0 \quad b_1 \quad b_2] \begin{bmatrix} 1 \\ W_1 \\ W_2 \end{bmatrix} = b_0 + b_1 W_1 + b_2 W_2$$

I use the prime symbol to mean transpose. The variance of  $\theta_{C \rightarrow Y}(W_1, W_2)$  is

$$var(\theta_{C \rightarrow Y}(W_1, W_2)) = \vec{l}' \Sigma \vec{l}.$$

The estimate of the variance of the conditional effect of condition is calculated by using the estimate of  $\Sigma$ . This is a general procedure for finding the standard error of any conditional effect, and only requires that the researcher identify  $\tilde{l}$ , the contrast vector which identifies the conditional effect of interest. Researchers can use MEMORE to calculate these effects automatically using the `wmodval1`, `wmodval2`, and `wmodval3` arguments (see documentation at [akmontoya.com](http://akmontoya.com)).

### **Multiplicative Moderation**

Multiplicative moderation is when the moderators interact with each other as well as the repeated-measures factor. This means that the model of  $Y$  in each condition includes interaction terms, adding complexity to the model. A model for the outcome in each condition is defined:

$$Y_{i1} = b_{01} + b_{11}W_{1i} + b_{21}W_{2i} + b_{31}W_{1i}W_{2i} + \epsilon_{i1}$$

$$Y_{i2} = b_{02} + b_{12}W_{1i} + b_{22}W_{2i} + b_{32}W_{1i}W_{2i} + \epsilon_{i2}$$

In these models we can think of the relationship between  $W_1$  and  $Y$  in each condition as a function of  $W_2$ , or the relationship between  $W_2$  and  $Y$  as a function of  $W_1$ . The new terms  $b_{31}$  and  $b_{32}$  represent the degree to which  $W_1$  and  $W_2$  interact in their respective conditions. The researcher may be interested in whether the interaction between  $W_1$  and  $W_2$  differs across conditions (i.e., is there a three-way interaction between condition,  $W_1$ , and  $W_2$ ?). This could be tested by examining if  $b_{31} = b_{32}$ . To test this hypothesis, the difference between the equations can be used to define coefficients which estimate the parameters of interest.

$$Y_{i2} - Y_{i1} = b_{02} - b_{01} + (b_{12} - b_{11})W_{1i} + (b_{22} - b_{21})W_{2i} + (b_{32} - b_{31})W_{1i}W_{2i} + \epsilon_{i2} - \epsilon_{i1}$$

$$Y_{Di} = b_0 + b_1W_{1i} + b_2W_{2i} + b_3W_{1i}W_{2i} + \epsilon_i \quad (7)$$

Estimating Equation 7 using a linear regression program would yield estimates of each of these coefficients along with inferential statistics. As mentioned above, a test on  $b_3$  would be a test of whether there is a three-way interaction between condition,  $W_1$ , and  $W_2$ . As additional moderators are added, the same method could be used to test higher order interactions which include a repeated-measures factor.

In all previous analyses, tests on  $b_1$  and  $b_2$  were tests of two-way interactions. Now they are tests of *conditional two-way interactions*. The coefficient  $b_{11}$  is the effect of  $W_1$  on  $Y_1$  conditional on  $W_2$  being zero, and  $b_{12}$  is the effect of  $W_1$  on  $Y_2$  conditional on  $W_2$  being zero. The difference between  $b_{11}$  and  $b_{12}$ ,  $b_1$ , is the degree to which the conditional effect of  $W_1$  on  $Y$  conditional on  $W_2$  being zero differs across conditions. Because the degree to which  $W_1$  affects  $Y$  in any given condition is allowed to depend on  $W_2$ , there is no single effect of  $W_1$  on  $Y$  in a specific condition. So,  $b_1$  reflects a conditional two-way interaction (between condition and  $W_1$ ) conditional on  $W_2$  being zero. Similarly,  $b_2$  reflects the degree to which the effect of  $W_2$  on  $Y$  conditional on  $W_1$  being zero differs across conditions.

I've described how to test a three-way interaction between a repeated-measures factor and two between-participant variables. This method can be generalized to any number of moderators. In addition to the test of interaction, probing can be used to better understand the pattern of effects. Especially with higher order interactions, understanding the pattern of effects throughout the range of the moderators can be very difficult by just examining the coefficients. Both Johnson-Neyman and simple-slopes probing methods can be generalized to higher order interactions, though the simple-slopes approach is often more interpretable as the researcher can choose specific sets of values for the moderators and estimate the effect of condition on the outcome. Generalizations of the Johnson-Neyman procedure to multiple moderator models involve either higher dimensional spaces (Hunka & Leighton, 1997) or regions of significance for interactions (Hayes, 2018), both of which can be very difficult to interpret. Because of this I focus on using the pick-a-point procedure in multiple moderator models.

When moderation is multiplicative, probing becomes even more important because the effect of each moderator will depend on the value of the other moderators. For example, in the case of two moderators the conditional effect of  $W_1$  would be

$$\theta_{W_1 \rightarrow Y}(C, W_2) = b_{1c} + b_{3c}W_2$$

and the variance would be

$$var(\theta_{W_1 \rightarrow Y}(C, W_2) = var(b_{1c}) + W_2^2 var(b_{3c}) + 2W_2 cov(b_{1c}, b_{3c}).$$

Now the effect of  $W_1$  is conditional on both the condition  $C$  and the other moderator  $W_2$ . Additionally the conditional effect of condition would be

$$\theta_{C \rightarrow Y}(W_1, W_2) = b_0 + b_1 W_1 + b_2 W_2 + b_3 W_1 W_2.$$

Using the methods outlined in the previous section we can identify that  $\vec{l}$  is

$$\vec{l} = [1 \quad W_1 \quad W_2 \quad W_1 W_2]$$

such that

$$\theta_{C \rightarrow Y}(W_1, W_2) = \vec{b} \vec{l}' = [b_0 \quad b_1 \quad b_2 \quad b_3] \begin{bmatrix} 1 \\ W_1 \\ W_2 \\ W_1 W_2 \end{bmatrix} = b_0 + b_1 W_1 + b_2 W_2 + b_3 W_1 W_2$$

This also means that

$$\text{var}(\hat{\theta}_{C \rightarrow Y}(W_1, W_2)) = \vec{l}' \Sigma \vec{l}$$

where  $\Sigma$  is

$$\Sigma = \begin{bmatrix} \text{var}(\widehat{b}_0) & \text{cov}(\widehat{b}_0, \widehat{b}_1) & \text{cov}(\widehat{b}_0, \widehat{b}_2) & \text{cov}(\widehat{b}_0, \widehat{b}_3) \\ \text{cov}(\widehat{b}_0, \widehat{b}_1) & \text{var}(\widehat{b}_1) & \text{cov}(\widehat{b}_1, \widehat{b}_2) & \text{cov}(\widehat{b}_1, \widehat{b}_3) \\ \text{cov}(\widehat{b}_0, \widehat{b}_2) & \text{cov}(\widehat{b}_1, \widehat{b}_2) & \text{var}(\widehat{b}_2) & \text{cov}(\widehat{b}_2, \widehat{b}_3) \\ \text{cov}(\widehat{b}_0, \widehat{b}_3) & \text{cov}(\widehat{b}_1, \widehat{b}_3) & \text{cov}(\widehat{b}_2, \widehat{b}_3) & \text{var}(\widehat{b}_3) \end{bmatrix}$$

These calculations are done in MEMORE using the `wmodval1`, `wmodval2`, and `wmodval3` arguments (see documentation at [akmontoya.com](http://akmontoya.com)).

### Example of Additive Moderation with MEMORE

Lasselin et al. (2016) expressed concerns that perhaps the effects were stronger for participants who went through ACT therapy compared to AR therapy, as was found in Kemani et al. (2015). By adding in *type of therapy* as a moderator, this hypothesis can be tested. In this analysis, I will use additive moderation. The MEMORE command for this analysis in SPSS would be

```
MEMORE y = PrePain PostPain /w = inflame therapy /model = 2.
```

The additional moderator `therapy` is included by adding it to the `w` list in the MEMORE command, and additive moderation is indicated by using Model 2 (See documentation for details and SAS commands).

Figure 3 contains the output for the command specified above. The `therapy` variable is coded so ACT

is 0 and AR is 1. The first section of the output gives information about the model: which variables are assigned to which role, order of subtraction for the outcome variable, and sample size.

The second section of the output is the results from estimating Equation 6 with the data from Lasselin et al. (2016). This table is just like the table from the single moderator analysis, but now it has multiple predictors. The estimated regression equation is

$$\hat{Y}_D = 0.43 - 0.38W_1 - 0.50W_2 \quad (8)$$

where inflammation is  $W_1$  and therapy type is  $W_2$ . Pain after treatment is expected to be 0.43 units lower than pain before treatment for those who are average on inflammation ( $W_1 = 0$ ) and in the ACT condition ( $W_2 = 0$ ), but this effect is not significant,  $t(37) = 1.69, p = .10$ . As inflammation increases by one unit, the difference between pain before and after treatment decreases by 0.38 units (i.e., treatment becomes less effective) and this effect is just significant,  $t(37) = -2.03, p = .05$ . Finally, it seems that participants in the AR condition have 0.50 units less difference on pain from pre- to post-treatment, but this effect is not significant,  $t(37) = -1.34, p = .19$ . If this effect were significant it would indicate that AR therapy was less effective than ACT therapy at reducing pain as suggested by Kemani et al. (2015).

For both the additive and multiplicative moderation models, MEMORE probes the effect of condition at a variety of sets of values of the moderators (see Figure 3; heading: “Conditional effect of ‘X’ on Y at values of moderator(s)”). These results show that the treatment is most effective when inflammation is low ( $W_1 = -1.01$ ) and with ACT therapy ( $W_2 = 0$ ). For this group, pain levels are expected to decrease 0.81 units over the course of treatment ( $t(37) = 2.64, p = .01$ ). However, there is no significant reduction in pain when inflammation is high or with AR therapy.

In this section, I’ve described how to estimate and test moderation in two-instance repeated-measures designs with multiple between-participant moderators. Though, throughout the paper I’ve described methods for testing these models using regression, there are alternative statistical approaches to answering these types of questions. I now turn to some short descriptions of these alternatives and where to learn more about them.

## Alternatives

The methods in this paper proposed for testing moderation are not the only possible methods for testing a moderation hypothesis in a two-instance repeated-measures design. Two particularly important methods require mention: structural equation modeling and multilevel modeling. Judd et al. (1996) directly compare the regression methods for testing an interaction described in this paper to structural equation modeling. Multilevel modeling requires additional multiple observations of each person in each condition, but if this type of data is available, then methods for testing and probing interactions in multilevel models have been discussed in depth in other papers (Bauer & Curran, 2005; Preacher, Curran, & Bauer, 2006), and could be used.

### Structural Equation Modeling

Judd et al. (1996) compared the approach described in this paper to a very basic structural equation modeling (SEM) approach where the moderator is allowed to predict each of the outcomes, and the residuals in these models are allowed to covary (See Figure 4). Note the correspondence between Figure 4 and Equations 2 and 3. In a structural equation modeling approach, the researcher would estimate the model in Figure 4, then fix the two paths  $b_{11}$  and  $b_{12}$  to be equal, then use a model comparison approach to test if the model with free paths fits significantly better than the model where the paths are fixed. In a structural equation modeling approach this would be done by using a  $\chi^2$  goodness-of-fit statistic to compare the two models. Note, though, that the null hypothesis in this structural equation model is the same as in the methods proposed in this paper. The concern is with whether a model where  $b_{11} = b_{12}$  describes the data sufficiently, or would it be better to allow the relationship between  $W$  and  $Y$  to vary by condition, thus allowing  $b_{11} \neq b_{12}$ .

The SEM analyses could be conducted in a variety of structural equation modeling programs like LISREL, Mplus, AMOS, EQS, etc. Some of these programs only use the variance-covariance matrix of the variables to estimate the paths involved. Judd et al. (1996) found that there was little to no difference

in statistical power and type I error between the methods proposed in this paper and using a structural equation modeling approach.

In order to probe the interactions, the intercept is needed, and so the means of the variables are also needed. Many programs have the functionality to accept either the raw data or the mean vector of all the variables in the dataset. Either way, this additional information would be needed to probe the interaction. Mplus allows the researcher to define additional parameters which can be combinations of existing parameters, and will then include inferential tests on these new parameters in the output. By choosing specific values of the moderator to probe at, and defining additional parameters using these values, the simple-slopes method is easy to implement in Mplus. The Johnson-Neyman procedure is not implemented in any of the existing structural equation modeling programs, so this method of probing would not be available in a structural equation modeling approach. Below is an example of Mplus code which would estimate all the parameters of interest including the conditional effect of therapy on pain at an inflammation level of 0.85. Though there is not a way to get the exact Johnson-Neyman transition points in Mplus, there is a fairly simple way to get plots which align with the Johnson-Neyman procedure. I've also included code which creates the Johnson-Neyman plots (Similar to Figure 2). The parameter estimates are identical, but the standard errors are slightly different because Mplus uses asymptotic variance estimates (denominator of  $N$ ) and OLS uses sample variance estimates (denominator of  $N-1$ ).

```
TITLE: Mplus code for the two-instance repeated-measures
moderation, single moderator
DATA:
  file is 'LasselinData_short.dat';
VARIABLE:
  names are inflame prepain postpain;
  !usev are inflame prepain postpain;
MODEL:
  prepain on inflame (b11);
  postpain on inflame (b12);
  [prepain] (b01);
  [postpain] (b02);
  prepain WITH postpain;
```



```

MODEL CONSTRAINT:
  new (pathdiff thetaCW85);
  pathdiff=b11-b12;
  thetaCW85=b01-b02+(b11-b12)*.85;

!JN Plot code;
LOOP(inflame, -2, 2, .1);
PLOT(thetaCW);
thetaCW = b01-b02+(b11-b12)*inflame;

PLOT:
TYPE = PLOT2;

```

One advantage of structural equation modeling is the superior methods available for dealing with missing data. Particularly with the methods described in this paper, the use of the difference score means that if individuals have missing data on either observation of the outcome, they are not included in the analysis when regression analysis is used. Structural equation modeling allows for methods like Full Information Maximum Likelihood, which can handle missing data without using any imputation methods. This means that individuals with some data can still contribute to the overall estimates.

An additional advantage of structural equation modeling approaches is the ability to include latent variables. Often the outcome variable or the moderator is not just one variable, but the combination of many. For example, Lasselin et al. (2016) conducted principal components analysis to create the measure of inflammation. This could have been integrated into the complete analysis by creating a latent variable which was indicated by each of observed inflammation variables, in which case the whole structural equation model, including a latent variable for inflammation, could have been estimated simultaneously. In the case where the moderator is a latent variable and the only predictor in the model, using a structural equation modeling approach will likely result in better power and more accurate standard errors for coefficient estimates, since regression methods assume there is no measurement error in the predictor variables. This is not necessarily the case with additive moderation, as measurement error in multiple

variables can have varying effects on estimation. In the case of multiplicative moderation, latent moderators will result in latent interactions, an area of research which is still in development (e.g., Cham, West, Ma, & Aiken, 2012; Marsh, Wen, Hau, & Nagengast, 2013).

### **Multilevel Modeling**

Multilevel modeling approaches to two-instance repeated-measures designs are only possible if there are multiple replicates per condition. For example, in many cognitive psychology studies, participants see a variety of visual cues which are from two different conditions. When there are many trials per condition, there are many observations of each participant in each condition. With only one observation per participant per condition, the multilevel models will not have enough degrees of freedom to estimate the parameters of interest. The basic premise of the multilevel model, however, is quite similar to those models that were described in this paper. The multilevel model used to assess interaction in a two-condition within-participant design would be

$$Y_{ij} = b_{0i} + b_{1i}X_{ij} + \epsilon_{ij}$$

where

$$b_{0i} = \gamma_{00} + \gamma_{01}W_i + u_{0i}$$

$$b_{1i} = \gamma_{01} + \gamma_{11}W_i + u_{1i}$$

Here  $i$  denotes individual and  $j$  indexes repeated measurements of that same individual.  $X_{ij}$  denotes the condition for person  $i$  during replicate  $j$ . The coefficients with subscripts  $i$  are random by person. This is one of the great advantages of multilevel models, in that by including random coefficients, the dependencies among observations from the same person can be taken into account. The above equations may look very different from those used throughout this article, however when they are combined the resulting equation is quite similar to the equation used throughout this article, but also including person-specific errors.

$$Y_{ij} = (\gamma_{00} + \gamma_{01}W_i + u_{0i}) + (\gamma_{01} + \gamma_{11}W_i + u_{1i})X_{ij} + \epsilon_{ij}$$

The above model would be quite unstable with only two observations per person. A more stable model would be to fix the random coefficient for  $X_{ij}$ ; however, this would then not allow for  $W$  to moderate the effect of  $X$  on  $Y$ . But if there are many replicates per person in each condition, these models should prove to be superior to the methods proposed in this paper. Bauer and Curran (2005) and Preacher, Curran, and Bauer (2006) provide excellent introductions to moderation analysis in multilevel models and computational tools for both testing and probing interactions using simple-slopes and the Johnson-Neyman procedure.

### **Extensions**

There are a variety of extensions for the methods proposed in this article which should be useful throughout experimental psychology and other scientific fields. In this section I address a few of these which I expect to be of particular interest. Some extensions are resolved below, but others are potentially fruitful future directions of research.

#### **When $W$ is Expected to Change across Instances**

Throughout this article I have addressed how to conduct moderation analysis when the moderator is a between-participant variable (measured once); however, researchers may wonder what to do if they believe that their moderator changes across instance. In this case, the researcher might measure the moderator twice, one in each instance. The original paper by Judd et al. (1996) addressed “moderation” in this case. Looking more closely, however, the authors revised their approach to testing moderation when the moderator is measured repeatedly, and in their 2001 paper they discussed this analysis as *mediation*. The hypothesis of moderation implies that the moderator impacts the relationship between instance and the outcome. This means the moderator should have temporal precedence over instance, and instance should not affect the moderator. If the moderator varies across instances, that means that instance is affecting the moderator. In this case it is difficult to discuss how the moderator affects the relationship between instance and the outcome, when it is clear that instance is affecting the moderator. Judd et al.

(2001) address how to assess mediation when the mediator is measured in each instance, and Montoya and Hayes (2017) update this approach to the more modern path-analytic approach, providing a computational tool, MEMORE (Model 1), for conducting inference on the indirect effect in these cases.

### **Including Covariates**

In the analysis described in this paper it is unclear how one should include additional covariates or even if those covariates *should* be included. An important aspect of this analysis is that it is a within-person analysis. If there is an effect of a covariate on the outcome and it does not vary across conditions, this covariate will cancel out when taking the difference score. Lasselin and colleagues (2016) were also interested in how age might impact pain levels. In the equations below I denote Age as  $A$ . Consider new versions of Equations 2 and 3 which now include age as a covariate:

$$Y_{i1} = b_{01} + b_{11}W_i + b_{21}A_i + \epsilon_{i1}$$

$$Y_{i2} = b_{02} + b_{12}W_i + b_{22}A_i + \epsilon_{i2}$$

In each equation, age is controlled for. However, if the relationship between age and the outcome is the same across conditions (i.e.,  $b_{21} = b_{22}$ ), then when the difference score is taken age will cancel out and is not required in the final model.

$$Y_{i2} - Y_{i1} = b_{02} - b_{01} + (b_{12} - b_{11})W_i + (b_{22} - b_{21})A_i + (\epsilon_{i2} - \epsilon_{i1})$$

$$Y_{Di} = b_0 + b_1W_i + b_2A + \epsilon_i$$

Therefore, if researchers are concerned about controlling for an additional variable, but they do not believe that the effect of that variable depends on condition, then that variable is not needed. If instead they believe that the effect of that variable depends on condition and they want to control for it, then age or any other covariate of interest should be treated as an additional moderator.

### **More than Two Instances**

This paper focused on two-instance repeated-measures designs; however, there may be instances when there are more than two conditions. Hayes and Montoya (2017) describe how to test moderation and probe moderation in a between-participant design when the focal predictor is multicategorical. In the within-participant case, including additional conditions involves taking contrasts of the conditions rather

than difference scores (Judd et al., 2001). Using a structural equation modeling approach, contrasts of interest can be defined and a likelihood ratio test can be used to test the significance of the effect. Once these contrasts are defined, probing the conditional effects is a simple generalization of the work presented in this paper; however, no published research has addressed this concern nor are there computational tools to do so. When there are more than two conditions, there is also the opportunity to probe the omnibus test of group differences, an issue which is still unresolved in the within-participant case.

### **Alternative Models of Change**

The analytical approach described in this article relies on difference scores to describe change for each individual. Difference scores can be useful in modeling change; however, they can be insensitive to phenomena like regression toward the mean or ceiling and floor effects (Cronbach and Furby, 1970; Campbell and Kenny, 1999; Jamieson, 1995). Many researchers have suggested abandoning the use of difference scores in favor of alternative methods (e.g., Bonate, 2000; Cronbach and Furby, 1970; Lord, 1963; Twisk and Proper, 2004; but see Thomas & Zumbo, 2012; Rogosa, 1995; Zumbo, 1999). Some alternative models include that of the residualized change scores, where the second measurement is regressed on the first, and the residuals from this model are then regressed onto the predictors of interest. This method could also be used to test and probing moderation, where instead of predicting the difference score, the residualized change score would be predicted by the moderator.

$$Y_{2i} = i_{Y_2} + bY_{1i} + u_{Y_{2i}}$$

$$\hat{u}_{Y_{2i}} = b_0 + b_1W_i + e_{Y_{2i}}$$

This model of change corrects for the initial measurement and expected regression toward the mean (Campbell and Kenny, 1999). Another alternative is an autoregressive model (also known as ANCOVA), where the second measure is predicted by the first as well as other predictors.

$$Y_{2i} = b_0 + b_1Y_{1i} + b_2W_i + e_{Y_{2i}}$$

It is worth noting that the autoregressive model is equivalent to the difference score model when  $b_1 = 1$  (Brogan and Kutner, 1980). Each of these models represents a different model of change, and it is likely that different methods will perform better or worse depending on what the true model of change is (if there is such a thing). Indeed much of the simulation work in this area has found that the performance of these different models depends on the generating model and no method works optimally for all types of data (Jamieson, 1995; Kisbu-Sakarya, MacKinnon, and Aiken, 2013; Petscher and Schatschneider, 2011). Much of the simulation work focuses on study designs where individuals are randomly assigned to one of two conditions and measured twice (before and after treatment). The statistical methods described in this paper could be used for that type of study, but also for studies involving continuous moderators. Future research could examine how these different models of change perform in investigating questions of moderation where the moderator is continuous and not randomly assigned.

### **Moderated Mediation**

The integration of mediation and moderation in between-participant designs has become a flourishing topic over the past decade (Edwards & Lambert, 2007; Fairchild & MacKinnon, 2009; Hayes, 2015; Preacher, Rucker, & Hayes, 2007). Perhaps one of the most promising future directions from this research is the integration of mediation and moderation in two-instance repeated-measures designs. Recent work has proposed path-analytic approaches for estimating and testing indirect effects in two-instance repeated-measures designs (Mackinnon, 2008; Montoya and Hayes , 2017; Valente & MacKinnon, 2017). By combining methods for mediation and moderation, we can estimate and test the moderation of mechanisms in two-instance repeated-measures designs and probe the indirect effects using methods similar to those proposed in this paper.

### **Conclusion**

Previous research on moderation in between-participant designs has established both how to test for interactions/moderation and how to probe these moderation effects to better understand the pattern of effects across the moderator. Easy-to-use computational tools are available as additions to popular statistical software (e.g., SPSS and SAS) or available online and have made the adoption of these methods

widespread throughout psychology and other academic research fields. Previous research on moderation analysis in two-instance repeated-measures designs had previously only established how to test for an interaction, and been limited to a single moderator. This paper generalized the analysis to include two probing methods, simple-slopes and the Johnson-Neyman procedure, as well as generalized the models to any number of moderators. MEMORE (available at [akmontoya.com](http://akmontoya.com)) can conduct the inferential tests for moderation, probe using both methods described in this paper, and test models with up to five moderators which may either be additive or multiplicative. With these additional innovations and tools in two-instance repeated-measures moderation analysis, researchers can now conduct their analyses more thoroughly and accurately, with better understanding of the nature of the interactions they are investigating.

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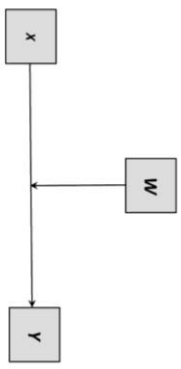
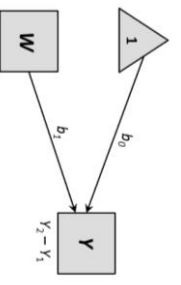
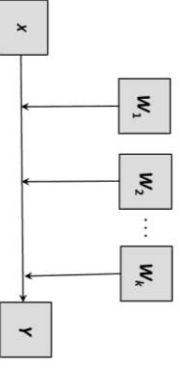
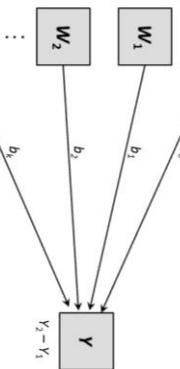
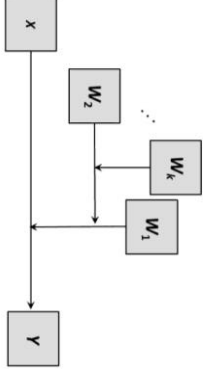
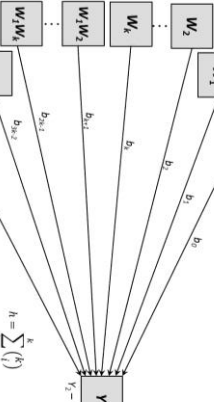
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**Table 1**  
Comparison of three types of moderated mediation models for two-instance repeated-measures designs.

Model	Equation Number	Model Number in MEMORE	Number of Moderators	Conceptual Diagram	Path Diagram
Simple Moderator Model	4	2 or 3	1		
Additive Moderator Model	6	2	2+		
Multiplicative Moderator Model	7	3	2+		

\*\*\*\*\* MEMORE Procedure for SPSS Version 2.0 \*\*\*\*\*

Written by Amanda Montoya

Documentation available at akmontoya.com

\*\*\*\*\*

Model:  
2

Variables:  
Y = PrePain PostPain  
W = inflame

Computed Variables:  
Ydiff = PrePain - PostPain

Sample Size:  
40

\*\*\*\*\*

Outcome: Ydiff = PrePain - PostPain

Model Summary

	R	R-sq	MSE	F	df1	df2	p
	.3254	.1059	1.4211	4.5014	1.0000	38.0000	.0404

Model

	coeff	SE	t	p	LLCI	ULCI
constant	.1997	.1885	1.0594	.2961	-.1819	.5813
inflame	-.3999	.1885	-2.1216	.0404	-.7815	-.0183

Degrees of freedom for all regression coefficient estimates:  
38

\*\*\*\*\*

Conditional Effect of 'X' on Y at values of moderator(s)

inflame	Effect	SE	t	p	LLCI	ULCI
-1.0135	.6050	.2683	2.2552	.0300	.0619	1.1481
-.0008	.2000	.1885	1.0611	.2954	-.1816	.5816
1.0119	-.2050	.2683	-.7642	.4495	-.7481	.3381

Degrees of freedom for all conditional effects:  
38

Values for quantitative moderators are the mean and plus/minus one SD from the mean.

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Conditional Effect of 'X' on Y at requested values of modederator(s)

inflame	Effect	SE	t	p	LLCI	ULCI
.8500	-.1402	.2475	-.5667	.5743	-.6412	.3608

Degrees of freedom for all conditional effects:  
38

\*\*\*\*\* JOHNSON-NEYMAN PROCEDURE \*\*\*\*\*

Moderator value(s) defining Johnson-Neyman significance region(s) and percent of observed data above value:

Value	% Abv
-.6260	65.0000

} Output generated from including wmodval1 = 0.85

} Output generated from including jn =1

```

Conditional Effect of 'X' on Y at values of moderator
inflamm Effect SE t p LLCI ULCI
-1.7349 .8935 .3773 2.3680 .0231 .1296 1.6574
-1.5311 .8120 .3446 2.3565 .0237 .1144 1.5096
-1.3273 .7305 .3131 2.3329 .0250 .0966 1.3644
-1.1235 .6490 .2834 2.2900 .0277 .0753 1.2227
-.9197 .5675 .2560 2.2168 .0327 .0493 1.0857
-.7158 .4860 .2317 2.0972 .0427 .0169 .9551
-.6260 .4500 .2223 2.0244 .0500 .0000 .9001
-.5120 .4045 .2117 1.9106 .0636 -.0241 .8330
-.3082 .3230 .1972 1.6377 .1097 -.0763 .7222
-.1044 .2414 .1895 1.2741 .2104 -.1422 .6251
.0994 .1599 .1894 .8443 .4038 -.2236 .5434
.3032 .0784 .1970 .3981 .6928 -.3204 .4773
.5070 -.0031 .2114 -.0145 .9885 -.4310 .4249
.7108 -.0846 .2313 -.3656 .7167 -.5529 .3838
.9146 -.1661 .2555 -.6499 .5196 -.6834 .3512
1.1184 -.2476 .2829 -.8752 .3870 -.8203 .3251
1.3223 -.3291 .3126 -1.0528 .2991 -.9619 .3037
1.5261 -.4106 .3440 -1.1935 .2401 -1.1071 .2859
1.7299 -.4921 .3768 -1.3062 .1993 -1.2548 .2706
1.9337 -.5736 .4105 -1.3975 .1704 -1.4046 .2573
2.1375 -.6551 .4449 -1.4724 .1492 -1.5559 .2456

```

} Output generated from including jn =1

```

Degrees of freedom for all conditional effects:
38

*****
Conditional Effect of Moderator(s) on Y in each Condition

Condition 1 Outcome:
PrePain

Model Summary
R R-sq MSE F df1 df2 p
.0324 .0010 .8616 .0398 1.0000 38.0000 .8429

Model
coeff SE t p LLCI ULCI
constant 4.3250 .1468 29.4691 .0000 4.0279 4.6221
inflamm .0293 .1468 .1996 .8429 -.2678 .3264

Degrees of freedom for all conditional effects:
38

-----

Condition 2 Outcome:
PostPain

Model Summary
R R-sq MSE F df1 df2 p
.3383 .1145 1.5002 4.9118 1.0000 38.0000 .0327

Model
coeff SE t p LLCI ULCI
constant 4.1253 .1937 21.3019 .0000 3.7333 4.5174
inflamm .4292 .1937 2.2163 .0327 .0371 .8213

Degrees of freedom for all conditional effects:
38

***** ANALYSIS NOTES AND WARNINGS *****

Level of confidence for all confidence intervals in output:
95.00

```

Figure 1. MEMORE SPSS output for simple moderator model generated from command on Page 15.

Output explores a model which allows the effect of treatment on pain (PrePain vs. PostPain) to be moderated by baseline inflammation (inflamm).

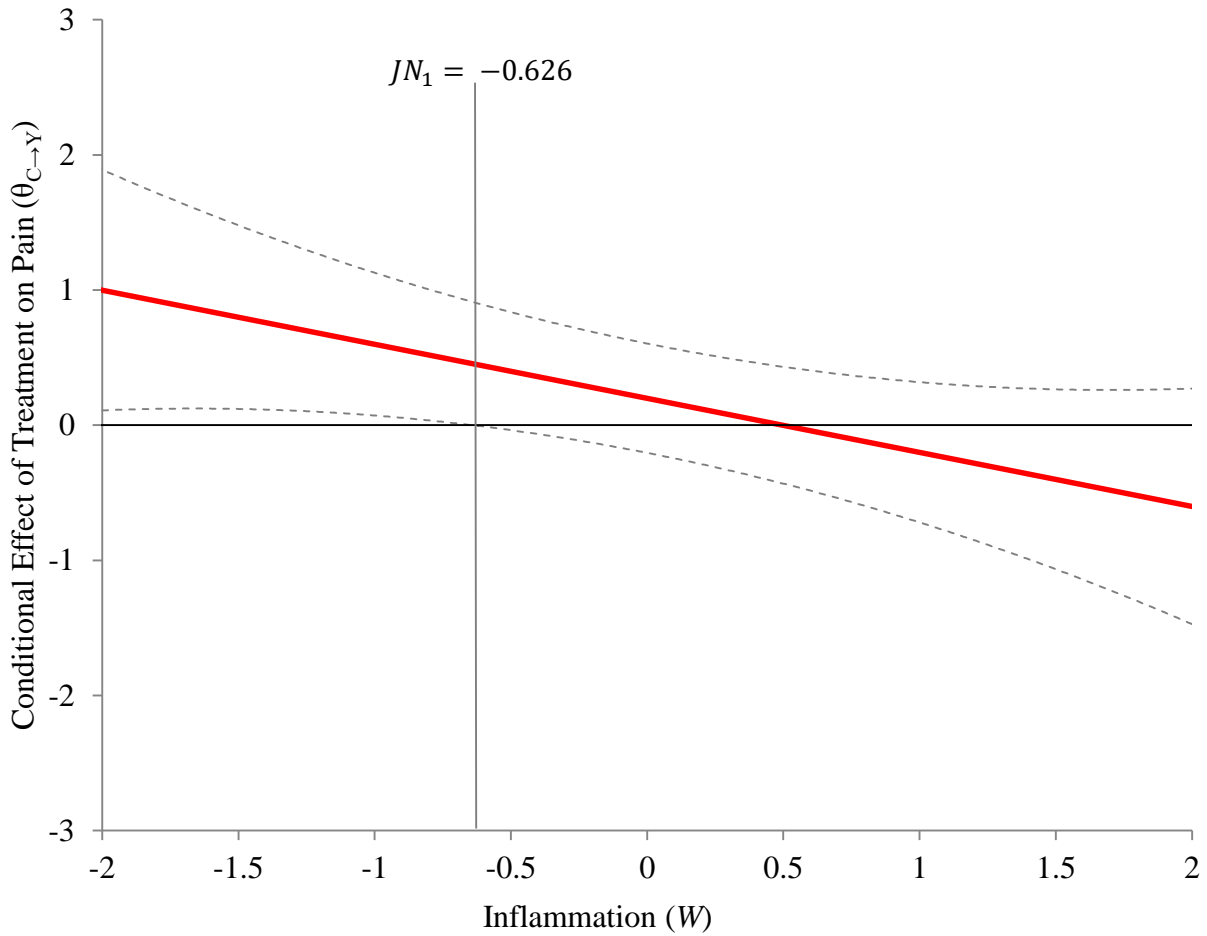


Figure 2. Graph of the Conditional Effect of Treatment ( $C$ ) on Pain ( $Y$ ) as a linear function of inflammation ( $W$ ) including Johnson-Neyman transition point ( $JN$ ). The  $JN$  point is where the confidence interval around the condition effect intersects zero on the  $Y$ -axis.



\*\*\*\*\* MEMORE Procedure for SPSS Version 2.0 \*\*\*\*\*

Written by Amanda Montoya

Documentation available at akmontoya.com

\*\*\*\*\*

Model:

2

Variables:

Y = PrePain PostPain

W1 = inflame

W2 = therapy

Computed Variables:

Ydiff = PrePain - PostPain

Sample Size:

40

\*\*\*\*\*

Outcome: Ydiff = PrePain - PostPain

Model Summary

	R	R-sq	MSE	F	df1	df2	p
	.3839	.1474	1.3918	3.1983	2.0000	37.0000	.0523

Model

	coeff	SE	t	p	LLCI	ULCI
constant	.4268	.2519	1.6945	.0986	-.0836	.9372
inflame	-.3799	.1871	-2.0300	.0496	-.7591	-.0007
therapy	-.5047	.3761	-1.3418	.1878	-1.2669	.2574

Degrees of freedom for all regression coefficient estimates:

37

\*\*\*\*\*

Conditional Effect of 'X' on Y at values of moderator(s)

	inflame	therapy	Effect	SE	t	p	LLCI	ULCI
	-1.0135	.0000	.8118	.3070	2.6445	.0119	.1898	1.4339
	-1.0135	1.0000	.3071	.3461	.8874	.3806	-.3941	1.0083
	-.0008	.0000	.4271	.2519	1.6957	.0983	-.0833	.9375
	-.0008	1.0000	-.0776	.2786	-.2786	.7821	-.6420	.4868
	1.0119	.0000	.0424	.3232	.1312	.8963	-.6125	.6974
	1.0119	1.0000	-.4623	.3275	-1.4116	.1664	-1.1259	.2013

Degrees of freedom for all conditional effects:

37

Values for quantitative moderators are the mean and plus/minus one SD from the mean.

Values for dichotomous moderators are the two values of the moderator.

\*\*\*\*\*

```

*****
Conditional Effect of Moderator(s) on Y in each Condition

Condition 1 Outcome:
  PrePain

Model Summary
      R      R-sq      MSE      F      df1      df2      p
      .0595      .0035      .8827      .0657      2.0000      37.0000      .9365

Model
      coeff      SE      t      p      LLCI      ULCI
constant      4.3660      .2006      21.7650      .0000      3.9596      4.7725
inflamm      .0329      .1490      .2208      .8264      -.2691      .3349
therapy      -.0911      .2996      -.3042      .7626      -.6981      .5158

Degrees of freedom for all conditional effects:
  37

-----

Condition 2 Outcome:
  PostPain

Model Summary
      R      R-sq      MSE      F      df1      df2      p
      .3750      .1406      1.4952      3.0266      2.0000      37.0000      .0606

Model
      coeff      SE      t      p      LLCI      ULCI
constant      3.9392      .2611      15.0878      .0000      3.4102      4.4682
inflamm      .4128      .1940      2.1282      .0400      .0198      .8058
therapy      .4136      .3899      1.0608      .2957      -.3764      1.2036

Degrees of freedom for all conditional effects:
  37

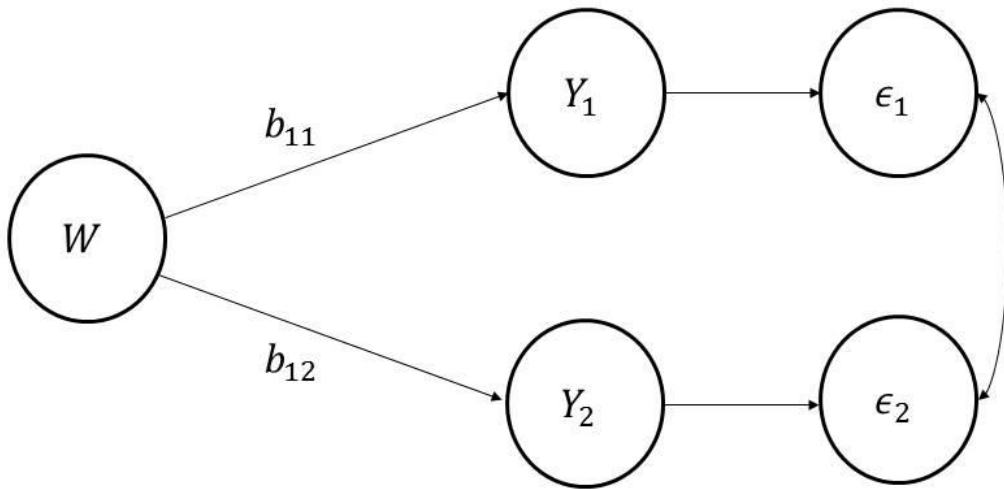
***** ANALYSIS NOTES AND WARNINGS *****

Level of confidence for all confidence intervals in output:
  95.00

```

Figure 3. MEMORE SPSS Output for additive moderator model generated from command on Page 28.

Output explores a model which allows the effect of treatment on pain (PrePain vs. PostPain) to be moderated by type of treatment (therapy) and baseline inflammation (inflamm).



*Figure 4.* Path diagram representing structural equation model for testing moderation in two-instance repeated-measures design.