How Within-Person Effects Shape Between-Person Differences: A Multilevel Structural Equation Modeling Perspective

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Abstract

Various theoretical accounts suggest that within-person effects relating to everyday experiences (assessed, e.g., via experience sampling studies or daily diary studies) are a central element for understanding between-person differences in future outcomes. In this regard, it is often assumed that the within-person effect of a time-varying predictor X on a time-varying mediator M contributes to the long-term development in an outcome variable Y . In the present work, we demonstrate that traditional multilevel mediation approaches fall short in capturing the proposed mechanism, however. We suggest that a model in which between-person differences in the strength of within-person effects predict the outcome Y mediated via mean levels in M more adequately aligns with the presumed theoretical account that within-person effects shape between-person differences. Using simulated data, we show that the central parameters of this multilevel structural equation model can be recovered well in most of the investigated scenarios. Our approach has important implications for whether or not to control for mean levels in models with within-person effects as predictors. We illustrate the model using empirical data targeting the question if the within-person association of occurrence of daily stressors (X) with daily experiences of negative affect (M) longitudinally predicts between-person difference in change in depressive symptoms (Y). Implications for other multilevel designs and intervention studies are discussed.

Keywords: multilevel mediation; Bayesian estimation; within-person dynamics; ambulatory assessment; intensive longitudinal data

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Intensive longitudinal designs (ILDs; e.g., ambulatory assessments or daily diary studies) have gained substantial momentum in psychological research. This type of data collection has been argued to come with specific advantages including a reduction of recall biases (Neubauer, Scott, et al., 2020; Schwarz, 2012), higher ecological validity compared to laboratory based research (Reis, 2012), and the possibility to examine within-person associations (Hamaker, 2012; Molenaar, 2004). There has also been a number of theoretical and empirical contributions arguing that data collected in ILDs also provide the potential to predict subsequent development in the areas of, for example, personality (Bleidorn et al., 2020; Quintus et al., 2020) or well-being and physical health (Brose et al., 2021; Charles et al., 2013; Kiecolt-Glaser et al., 2020). For instance, the TESSERA (Triggering situations, Expectancy, States/State expressions, and Reactions) framework suggests that daily experiences build the foundation for longitudinal change in personality (Wrzus & Roberts, 2017). A similar line of reasoning has also been made in conceptual work in life-span developmental psychology, which considers within-person fluctuations as a central ingredient towards understanding (between-person differences in) developmental pathways (Nesselroade, 1991). Specifically, the interplay between variables fluctuating on a short-term scale (e.g., across hours or days) has been considered as contributing to longitudinal change.

The central aim of the present work is to provide a conceptual and data-analytic model that can be used by empirical researchers in a wide range of applications in psychological research to adequately address the question how the within-person interplay between two variables predicts between-person differences in a third variable. To illustrate this general idea, consider the following example: Prior work targeted the associations among autonomy-supportive parenting, parental need fulfillment, and parental well-being (Neubauer et al., 2021). Specifically, parents were investigated in a daily-diary design and asked each day for

21 consecutive days to what extent they supported their children's autonomy today (i.e., if their children were allowed to decide for themselves what they want to do today). Parents were also asked about their own need fulfillment today (i.e., the extent to which they felt autonomous, competent, and socially included today). Results showed that days with higher provision of autonomy support were also days with higher need fulfillment. Further analyses showed that parents who reported higher need satisfaction on average showed a stronger increase in well-being from before to after the daily diary period. A question that arises from this pattern is whether and how the within-person interplay between daily autonomy support and daily need satisfaction might contribute to between-person differences in well-being: The positive association between autonomy support and need satisfaction on the one hand, and the positive association between need satisfaction and well-being on the other hand bring up the idea that the day-to-day interplay between autonomy-supportive parenting and parental need satisfaction might contribute to (between-person differences in) parental well-being.

In more general terms, we can think of this scenario as a setting involving three variables: two time-varying (Level-1) variables (further referred to as X and M; in the example: daily autonomy support and daily need fulfillment), and one time-invariant (Level-2) variable (further referred to as Y; in the example: well-being). From a theoretical and conceptual perspective, the relation between these three variables might seem straightforward. What is lacking from the extant literature is, however, a statistical model that allows to tackle this interplay. At first glance, the general pattern of relations among the variables X, M, and Y alludes to a mediation mechanism (in the example: daily autonomy support might be related to well-being via daily parental need satisfaction). We will show that a standard multilevel mediation approach is, however, not suited to test the conceptual research question if and how within-person effects shape between-person differences.

To accurately approach this question, we suggest a novel modeling approach that targets heterogeneity in Level-1 effects as predictor of a Level-2 outcome, whose effect is

mediated via the Level-2 component of a mediator measured at Level 1. With the current work, we thereby add to the literature a conceptual and data-analytic framework that aims at explaining how within-person effects shape between-person differences. We will demonstrate the application of our proposed model in a simulation study and in empirical data, targeting the question if the daily interplay of the occurrence of everyday stressors and daily negative affect predicts inter-individual differences in depressive symptoms via levels of negative affect. Before we turn to our proposed model, we first recap previous approaches to 1-1-2 multilevel mediation models and demonstrate why they fall short in targeting the research question of the present work.

Model and Assumptions

In ILDs, the repeated data collected conform to a multilevel structure, with at least two levels and repeated assessments (Level 1, within-person level) nested within study participants (Level 2, between-person level). In the present work, we target a scenario with three observed variables: The outcome variable (Y) is measured only once and therefore varies across Level-2 units only. Additionally, assume that two other variables were collected several times for each participant (i.e., at Level 1). Let us think of one of the variables as predictor (X) and the other as the mediator (M). The variables X and M , hence, vary within participants (on Level 1), but they may also exhibit between-person variation (i.e., there may be mean level differences in X and M between participants).

Assume that longitudinal data (T assessments) on the variables X and M were collected in a sample of N participants. Participant i 's observations on day t are denoted as X_{ti} and M_{ti} , respectively. Further, each participant i provided one assessment on the outcome variable (Y_i) some time after the longitudinal data had been collected. For example, these variables could be daily provision of autonomy support (X_{ti}), daily need fulfillment (M_{ti}), and well-being (Y_i). Other potential sets of variables could be daily school engagement (X_{ti}), daily

school-related motivation (M_{ti}), and educational attainment (Y_i), or experiences of daily stress (X_{ti}), daily negative affect (M_{ti}), and depressive symptoms (Y_i).

The relations among these three variables can be modeled as depicted in Figure 1, which readers familiar with the multilevel mediation literature might readily identify as a 1-1-2 mediation model.¹ In order to remain consistent with the customary terminology in mediation models, we will further refer to the effect of X predicting M on Level 1 (Level 2) as a_w (a_b), to the effect of M predicting Y on Level 2 as b , and the effect of X predicting Y on Level 2 as c' (the direct effect). Traditional approaches to multilevel modeling require that the outcome variable (Y) is located on the lowest level, a requirement that is violated in the present scenario. Therefore, we will adopt a multilevel structural equation model (MSEM; e.g., Rabe-Hesketh et al., 2007) approach in this work. MSEMs have been suggested in the context of multilevel mediation models as a more flexible way to accommodate mediation in a variety of contexts (with predictor, mediator, and outcome on either Level 1 or Level 2; Preacher et al., 2010; Preacher et al., 2011).

The variables X_{ti} and M_{ti} contain both variance on Level 1 (they vary within individuals) and on Level 2 (they vary across individuals). In the framework proposed here, a MSEM framework with a Bayesian estimator in Mplus, these variables are therefore decomposed into a latent within-person part (X_{ti}^W and M_{ti}^W) and a latent between-person part (X_i^B and M_i^B). To that end, the vector of observed data ($\mathbf{D}_{ti}$) is decomposed as specified in the following measurement model:

$$\mathbf{D}_{ti} = \mathbf{\Lambda} \cdot \boldsymbol{\eta}_{ti} \quad (1)$$

¹ In the standard terminology used in the multilevel mediation literature, the nomenclature of a multilevel mediation model specifies the level of the predictor, the mediator, and the outcome in a sequence of three digits (e.g., 1-1-1, 2-1-1).

$$\begin{bmatrix} X_{ti} \\ M_{ti} \\ Y_i \end{bmatrix} = \begin{bmatrix} 1 & 0 & 1 & 0 & 0 \\ 0 & 1 & 0 & 1 & 0 \\ 0 & 0 & 0 & 0 & 1 \end{bmatrix} \cdot \begin{bmatrix} X_{ti}^W \\ M_{ti}^W \\ X_i^B \\ M_i^B \\ Y_i^B \end{bmatrix}$$

The latent variables (η_{ti}) can then be used in a structural model on Level 1 (within-person level; Equation 2) and Level 2 (between-person level; Equation 3). The matrices $\mathbf{B}_i$ and $\mathbf{\Gamma}$ contain the regression coefficients linking the latent variables on the within-person level and the between-person level, respectively.

$$\eta_{ti} = \alpha_i + \mathbf{B}_i \cdot \eta_{ti} + \zeta_{ti} \quad (2)$$

$$\begin{bmatrix} X_{ti}^W \\ M_{ti}^W \end{bmatrix} = \begin{bmatrix} 0 \\ 0 \end{bmatrix} + \begin{bmatrix} 0 & 0 \\ \beta_{MX_i} & 0 \end{bmatrix} \cdot \begin{bmatrix} X_{ti}^W \\ M_{ti}^W \end{bmatrix} + \begin{bmatrix} \varepsilon_{X_{ti}} \\ \varepsilon_{M_{ti}} \end{bmatrix}$$

$$\eta_i = \alpha + \mathbf{\Gamma} \cdot \eta_i + \xi_i \quad (3)$$

$$\begin{bmatrix} X_i^B \\ M_i^B \\ Y_i^B \\ \beta_{MX_i} \end{bmatrix} = \begin{bmatrix} \alpha_X \\ \alpha_M \\ \alpha_Y \\ \alpha_\beta \end{bmatrix} + \begin{bmatrix} 0 & 0 & 0 & 0 \\ \gamma_{MX} & 0 & 0 & 0 \\ \gamma_{YX} & \gamma_{YM} & 0 & 0 \\ 0 & 0 & 0 & 0 \end{bmatrix} \cdot \begin{bmatrix} X_i^B \\ M_i^B \\ Y_i^B \\ \beta_{MX_i} \end{bmatrix} + \begin{bmatrix} \omega_{X_i} \\ \omega_{M_i} \\ \omega_{Y_i} \\ \omega_{\beta_i} \end{bmatrix}$$

The decomposition of the variance of X_{ti} and M_{ti} into between-person variance and within-person variance allows an unambiguous interpretation of the effect of X on M on Level 1 (α_β) as the within-person effect, and the effect of X on M on Level 2 (γ_{MX}) as the between-person effect (Wang & Maxwell, 2015). In multilevel models, this decomposition of variance is typically obtained by centering the Level-1 variable on the person mean. However, under the default robust maximum likelihood estimator of MSEM in Mplus the predictor X_{ti} is *not* decomposed into a latent within-part and a latent between-part. For predictors that emit random effects, the whole observed variable is used as predictor on Level 1 (that is, X_{ti} , rather than X_{ti}^W ; see Asparouhov & Muthén, 2019). Consequently, the estimated effect of X on M on

the between-person level is not the between-person effect (γ_{MX}) but the context effect ($\gamma_{MX} - \alpha_\beta$). This issue can, however, be avoided by using the Bayesian estimator in Mplus, which decomposes the predictor into a latent within-part and a latent between-part also for predictors with random effects, and therefore yields the within-person and between-person effects (Asparouhov & Muthén, 2019).²

Multivariate normal distributions are assumed for the two residual vectors on Level 1 (ζ_{ti}) and Level 2 (ξ_i)

$$\zeta_{ti} \sim MVN \left(\begin{bmatrix} 0 \\ 0 \end{bmatrix}, \Psi = \begin{bmatrix} \sigma_X^2 & 0 \\ 0 & \sigma_M^2 \end{bmatrix} \right) \quad (4)$$

$$\xi_i \sim MVN \left(\begin{bmatrix} 0 \\ 0 \\ 0 \\ 0 \end{bmatrix}, \Sigma = \begin{bmatrix} v_X^2 & 0 & 0 & 0 \\ 0 & v_M^2 & 0 & 0 \\ 0 & 0 & v_Y^2 & 0 \\ 0 & 0 & 0 & v_\beta^2 \end{bmatrix} \right) \quad (5)$$

In addition to the assumptions about the distribution of residuals (Equations 4 and 5), it is assumed that the Level-1 residuals are uncorrelated with the Level-2 residuals and that the time series for the time-varying variables X_{ti} and M_{ti} are stationary with regard to means and (co)variances. Further, it is assumed that the directed effects in Figure 1 represent causal effects that are not contaminated by any third variables. The statistical model cannot determine whether the effects are indeed causal, and any interpretation of the involved effects (including the indirect effects) requires that the directionality of the effects has been correctly

² Note that an alternative approach would be to center the Level-1 predictor on the observed person mean and to enter the person mean as Level-2 predictor. In this case, the resulting coefficients would be the within- and between-person effects under both the maximum likelihood estimator and the Bayesian estimator. However, centering Level-1 predictors on the observed person means leads to biased estimates of the between-person effects because the unreliability of the person mean (which may vary across participants) is not accounted for (see Lüdtke et al., 2008). Latent person-mean centering is therefore generally recommended though it may yield estimation problems in some applications (Brose et al., 2021). In standard multilevel models, another possibility would be to add the person mean of X_{ti} as additional predictor which would also result in a pure estimate of the within-person effect of X_{ti} , but this alternative procedure has been linked to an underestimation of the random slope variance of the within-person effect (e.g., Wang & Maxwell, 2015). Alternative centering strategies (e.g., centering on the grand mean or centering on the observation of the first measurement occasion) are not suited in the present context that specifically targets within-person effects (see e.g., Wang & Maxwell, 2015, and Enders & Tofghi, 2007, for further details on centering).

specified. In empirical applications, the assumption regarding the causal order of effects is often difficult to defend when relying on observed data only. Experimental approaches (e.g., Schmiedek & Neubauer, 2020) or controlling for all relevant confounders may be needed to gain more confidence in this assumption (see also Rohrer, 2018).

Indirect Effect Estimates in 1-1-2 Mediation

Statistically testing for mediation involves targeting the indirect effect of the proposed predictor X on the outcome Y via the mediator M (MacKinnon et al., 2007). In single-level mediation models, the indirect effect is computed as the product of the effects a ($X \rightarrow M$) and b ($M \rightarrow Y$). In the present multilevel model in which X and M have been decomposed into a within-and between-person parts, there are two possibilities to compute the indirect effect: The product of a_w and b (further termed $a_w b$), and the product of a_b and b (further termed $a_b b$). The question is, however, if any of these two indirect effects is suited to address the question if and how within-person effects shape between-person differences.

Preacher (2015) argues that in multilevel mediation models in which a Level-2 variable is involved in the postulated mediation sequence, the indirect effect necessarily includes only Level-2 effects, leaving the indirect effect as $a_b b$. This argument has typically been spelled out for 2-1-1 mediation models (see also Li & Beretvas, 2013; Tofighi & Thoemmes, 2014), but the logic behind this argument also applies to the present context: the b -path represents the (between-person) effect of M on Y , that is, it represents a (presumed) causal effect of between-person differences in the mediator on between-person differences in the outcome. For example, a positive b -path from daily need satisfaction to well-being indicates that participants who report higher average need satisfaction later report higher well-being. That is, the model assumes that between-person differences in need satisfaction cause between-person differences in well-being. The a -path of the indirect effect, hence, needs to explain what causes between-person differences in the mediator (need satisfaction). The within-person effect (a_w) cannot explain between-person differences in the mediator, however.

To illustrate this point, consider an effect of $a_w = .20$. This effect indicates that on days when an individual receives (one scale point) more of the treatment X than usual (e.g., reports more autonomy-supportive parenting), M (e.g., need satisfaction) will be .20 scale points higher than typical for this individual. This effect does, however, not create any between-person differences in average M, it causes only temporary increases within the individual relative to his or her average. In other words, and to paraphrase the argument by Preacher (2015) on 2-1-1 models, between-person differences in M cannot be the result of within-person variability in X. Hence any indirect effect involving between-person differences in M cannot include within-person effects (a_w).

This line of reasoning suggests that in a 1-1-2 mediation model, the critical indirect effect that needs to be tested is the indirect effect $a_b b$. With this indirect effect estimate, the model depicted in Figure 1 can be useful in various situations. For instance, it could be applied to test if students who use effective self-regulation strategies (X) on more days during the preparation phase for an exam perform better in the exam (Y) because they reach their daily learning goals (M) on more days (Breitwieser et al., 2020). Or, in a different multilevel setting with students (Level 1) nested within classrooms (Level 2), it could be examined, if teachers of classes consisting of students with higher average levels of agreeableness (X) judge the classroom climate to be more positive (Y) because there is less (student reported) bullying occurring (M).

Critically, however, this indirect effect does not inform us about the central research question of the present work: Note that the effect a_b is the between-person effect of X on M, which is not generally equivalent to the within-person effect a_w that we intend to target in the present work (Molenaar, 2004). Consequently, the indirect effect $a_b b$ does not help in targeting the conceptual research question if and how within-person effects shape between-person differences. Both indirect effect estimates from 1-1-2 mediation models are, hence, not suited to address the question set up in the present work. We therefore need to seek for

alternatives on how the within-person effect of X on M may be transmitted to create (between-person) differences in Y.

Between-Person Differences in Within-Person Effects and Mean Levels: Considering Within-Person Experiments

To illustrate our proposed solution – which is targeting between-person differences in the strength of within-person effects as central predictor of a Level-2 outcome, as introduced later – consider an experimental design with a within-person manipulation of X. For this scenario, we focus on the two time-varying variables X_{ti} and M_{ti} . We use this hypothetical experimental design to illustrate how (between-person differences in) causal within-person effects can create between-person differences in the variable M.

Assume that X_{ti} is a dichotomous variable that assesses if participant i has received a treatment on day t ($X_{ti} = 1$) or not ($X_{ti} = 0$). The variable X_{ti} is experimentally manipulated: on each day, participants are randomly assigned to either receive the treatment or not. Defining $X_{.i}$ as individual i 's mean in X_{ti} ($X_{.i} = \frac{1}{T} \sum_{t=1}^T (X_{ti})$), the multilevel model equations for X predicting M can be written as:

Level 1:

$$M_{it} = \beta_{0i} + \beta_{1i} \cdot (X_{it} - X_{.i}) + \varepsilon_{it} \quad (6)$$

Level 2:

$$\beta_{0i} = \gamma_{00} + \gamma_{01} \cdot X_{.i} + v_{0i} \quad (7)$$

$$\beta_{1i} = \gamma_{10} + v_{1i} \quad (8)$$

Note that the Level-1 predictor is centered on the person mean to yield an unbiased estimate of the pure within-person effect (Wang & Maxwell, 2015). Combining the Level-1 and Level-2 equation yields:

$$M_{it} = \gamma_{00} + v_{0i} + \gamma_{01} \cdot X_{.i} + \gamma_{10} \cdot X_{it} + v_{1i} \cdot X_{it} - \gamma_{10} \cdot X_{.i} - v_{1i} \cdot X_{.i} + \varepsilon_{it} \quad (9)$$

$$\begin{bmatrix} \varepsilon_{it} \\ v_{0i} \\ v_{1i} \end{bmatrix} \sim MVN \left(\begin{bmatrix} 0 \\ 0 \\ 0 \end{bmatrix}, \begin{bmatrix} \sigma_\varepsilon^2 & 0 & 0 \\ 0 & \sigma_{v0}^2 & \sigma_{01} \\ 0 & \sigma_{01} & \sigma_{v1}^2 \end{bmatrix} \right) \quad (10)$$

If the model in Equation (9) is the data-generating model, between-person differences in the effect of X on M (σ_{v1}^2) will lead to between-person differences in mean levels of M_{it} . To demonstrate this, we examine the expected values on M_{it} for two individuals $i = 1$ and $i = 2$ who differ in the within-person effect v_{1i} ($v_{1i=1} = s_1$ and $v_{1i=2} = s_2$, with $s_1 \neq s_2$) in two scenarios: for occasions when no treatment is provided ($X_{it} = 0$), and when the average amount of the treatment for this person is provided ($X_{it} = X_i$). This difference-in-differences approach is necessary to compare the expected difference between these two individuals after the within-person experiment (when both have received the treatment on $X_i \cdot T$ occasions) to the same individuals only at occasion when they have received no treatment ($X_{it} = 0$).

The expectancies of M_{it} in these four scenarios are (see Appendix for details):

$$E(M_{it} | X_{ti} = 0, i = 1) = \gamma_{00} + E(v_{0i} | i = 1) + E(X_{.1}) \cdot [\gamma_{01} - \gamma_{10}] - s_1 \cdot E(X_{.1}) \quad (11)$$

$$E(M_{it} | X_{ti} = 0, i = 2) = \gamma_{00} + E(v_{0i} | i = 2) + E(X_{.2}) \cdot [\gamma_{01} - \gamma_{10}] - s_2 \cdot E(X_{.2}) \quad (12)$$

$$E(M_{it} | X_{ti} = X_i, i = 1) = \gamma_{00} + E(v_{0i} | i = 1) + E(X_{.1}) \cdot \gamma_{01} \quad (13)$$

$$E(M_{it} | X_{ti} = X_i, i = 2) = \gamma_{00} + E(v_{0i} | i = 2) + E(X_{.2}) \cdot \gamma_{01} \quad (14)$$

It follows that the expected difference in M_{it} between two individuals when the individuals received the treatment on $X_i \cdot T$ occasions and the expected difference between these two individuals at occasions when they did not receive a treatment ($X_{ti} = 0$) is:

$$(14) - (13) - [(12) - (11)] = \gamma_{10}(E(X_{.2}) - E(X_{.1})) + s_2 \cdot E(X_{.2}) - s_1 \cdot E(X_{.1}) \quad (15)$$

To illustrate the implications, consider an exemplary within-person experiment in which each participant gets exposed to a stressor on n_{1i} days ($X_{ti} = 1$) and not exposed to a stressor on the other n_{0i} days ($X_{ti} = 0$; with random assignments of stressor and non-stressor

days). Negative affect (M_{ti}) is assessed at the end of each day. $E(X_{.i})$ is equal to $n_{1i} / (n_{1i} + n_{0i})$, that is, the proportion of days on which the individual was exposed to a stressor. In case of a perfectly balanced design (all participants receive the treatment on the same proportion of days, i.e., n_1 and n_0 are constant for all participants), the difference in differences between the two participants (see Equation 15) reduces to

$$(s_2 - s_1) \cdot \frac{n_1}{n_1 + n_0} \quad (16)$$

In other words, two individuals who differ in their response to the exposure of a stressor by one unit (e.g., $s_2 = 1$ vs. $s_1 = 0$) are expected to report mean levels in negative affect that are by $\frac{n_1}{n_1 + n_0}$ larger than the mean levels of these individuals that are observed at occasions without the stressor (without the treatment). Figure 2 illustrates this difference-in-differences approach: Two participants who differ in their within-person effect of X on M differ in mean M after the experiment (filled circles) more strongly than they differ in the observations of the no-treatment condition only (shaded circles). The difference in differences (the length of the purple line) equals $(s_2 - s_1) \cdot \frac{n_1}{n_1 + n_0}$.

In unbalanced designs (when individuals differ in $E(X_{.i})$), the expected difference in differences is additionally a function of individuals' expectations in X, that is, the proportion of days on which an individual has received the treatment. It is therefore important to remove the variance accounted for by $E(X_{.i})$ by statistically controlling for mean levels in X. If $E(X_{.i})$ is correlated with v_{1i} (i.e., if participants with higher stress reactivity tend to receive more or less of the treatment than individuals with lower stress reactivity), failure to account for between-person differences in the amount of treatment received can lead to an overestimation or underestimation of the causal effect of between-person differences in within-person effects on mean levels in M, depending on the direction of the association between $E(X_{.i})$ and v_{1i} .

Predicting Between-Person Differences from Between-Person Differences in Within-Person Effects

The previous elaborations have demonstrated, that the indirect effect $a_b b$ – the central indirect effect in 1-1-2 mediation models (Preacher, 2015) – does not provide relevant information to test if and how the within-person interplay in two variables shapes between-person differences in a third variable but rather addresses a research question fundamentally different from the one set up in the present work. Further, the demonstrations in the previous section highlight that between-person differences in the within-person a_w -path, v_β^2 , create between-person differences in M – differences that may be linked to between-person differences in the outcome Y. We therefore suggest that between-person differences in the within-person association between X and M are central for testing the conceptual idea that within-person processes contribute to between-person differences in Y. Specifically, we argue that between-person differences in the within-person association between X and M can account for between-person differences in the outcome Y via process-related average levels (average levels in M). We propose that an adjustment to the model introduced above (Figure 1) can reflect this conceptual model and can be applied to address the question if and how within-person effects account for between-person differences. Specifically, we propose that adding directed paths from the latent variable representing between-person differences in the within-person effect (β_{MX_i}) to the between-person part of the mediator (M_i^B) and the outcome (Y_i^B) represents the postulated mechanism.³

The changes to the model only affect the between-person part of the MSEM; the additional parameters included are highlighted in red in Figure 3. These changes affect the structural model on the between-person part (see Equation 3) and the residual variance-covariance matrix on the between-person level Σ , which need to be changed as follows

$$\eta_i = \alpha + \Gamma \cdot \eta_i + \xi_i \quad (17)$$

³ This model has no direct correspondence in the conventional three-digit notation of multilevel mediation models, but it could be understood as a (1 $\rightarrow$ 1)-2-2 mediation model.

$$\begin{bmatrix} X_i^B \\ M_i^B \\ Y_i^B \\ \beta_{MX_i} \end{bmatrix} = \begin{bmatrix} \alpha_X \\ \alpha_M \\ \alpha_Y \\ \alpha_\beta \end{bmatrix} + \begin{bmatrix} 0 & 0 & 0 & 0 \\ \gamma_{MX} & 0 & 0 & \gamma_{M\beta} \\ \gamma_{YX} & \gamma_{YM} & 0 & \gamma_{Y\beta} \\ 0 & 0 & 0 & 0 \end{bmatrix} \cdot \begin{bmatrix} X_i^B \\ M_i^B \\ Y_i^B \\ \beta_{MX_i} \end{bmatrix} + \begin{bmatrix} \omega_{X_i} \\ \omega_{M_i} \\ \omega_{Y_i} \\ \omega_{\beta_i} \end{bmatrix}$$

$$\xi_i \sim MVN \left(\begin{bmatrix} 0 \\ 0 \\ 0 \\ 0 \end{bmatrix}, \Sigma = \begin{bmatrix} v_X^2 & 0 & 0 & v_{\beta X} \\ 0 & v_M^2 & 0 & 0 \\ 0 & 0 & v_Y^2 & 0 \\ 0 & 0 & 0 & v_\beta^2 \end{bmatrix} \right) \quad (18)$$

The three new parameters in this model are the directed effects from β_{MX_i} to M_i^B ($\gamma_{M\beta}$) and from β_{MX_i} to Y_i^B ($\gamma_{Y\beta}$), as well as the covariance between β_{MX_i} and X_i^B ($v_{\beta X}$).

This model can be used to explain how within-person effects predict between-person differences in future outcomes. An implication of this model is that between-person differences in the outcome Y can be explained by between-person differences in the within-person effect of X on M that cause differences in Y (partially) via differences in M. To describe this model, consider an example in which daily stress is the predictor, daily negative affect is the mediator, and change in depressive symptoms from before to after the daily diary phase is the outcome. In this example, the structural model would imply that - holding between-person differences in average stress levels constant - participants who have a stronger within-person association between stress and negative affect (participants with a larger stress reactivity; Bolger & Zuckerman, 1995) report higher levels of average negative affect ($\gamma_{M\beta}$) and consequently, stronger increases in depressive symptoms (γ_{YM}): Participants whose negative affect increases more strongly on days with a stressor increase in depressive symptoms because they experience more negative affect on average across the daily-diary period. The presumed causal association between β_{MX_i} and M_i^B is also visualized in Figure 2: All else being equal, participants who exhibit a more positive within-person association of X_{ti}^W and M_{ti}^W have higher average levels in M_i^B than individuals with a less positive association.

Importantly, average levels of experienced stress (X_i^B) are controlled for in this model. That is, the effect of the within-person association between stress and negative affect on average negative affect ($\gamma_{M\beta}$) is adjusted for between-person differences in X_i^B . Note that controlling for the average level in X_i^B is essential to obtain unbiased estimates of $\gamma_{M\beta}$ when β_{MX_i} and X_i^B are correlated. Specifically, if participants who experience more stress are also more likely to exhibit high stress reactivity (a pattern of results that may be expected; see Sliwinski et al., 2009) and γ_{MX} is constrained to zero (i.e., the effect of X_i^B on M_i^B is not estimated, i.e., average X is not controlled for), $\gamma_{M\beta}$ will be overestimated, and, as a consequence, the indirect effect, $\gamma_{M\beta} \cdot \gamma_{YM}$, and the total effect of β_{MX_i} ($\gamma_{M\beta} \cdot \gamma_{YM} + \gamma_{Y\beta}$) will be overestimated as well.

The model proposed in Figure 3 has important implications for research targeting the predictive validity of between-person differences in within-person effects. One particularly important feature is that predicting Y_i^B from β_{MX_i} while controlling for M_i^B can have deleterious consequences for the estimate of the total effect of β_{MX_i} on Y_i^B . Specifically, the total effect of β_{MX_i} on Y_i^B ($totE_\beta$) is the sum of the direct effect ($\gamma_{Y\beta}$) and the indirect effect via M_i^B ($\gamma_{M\beta} \cdot \gamma_{YM}$).

$$totE_\beta = \gamma_{Y\beta} + \gamma_{M\beta} \cdot \gamma_{YM} \quad (19)$$

It follows that in a model in which the directed path from β_{MX_i} to M_i^B is not estimated ($\gamma_{M\beta} = 0$), but the effect of M_i^B on Y_i^B is controlled for, the total effect of β_{MX_i} on Y_i^B will be downward biased, with the bias amounting to $-\gamma_{M\beta} \cdot \gamma_{YM}$. If the effect of M_i^B on Y_i^B is also not controlled for (that is, $\gamma_{M\beta} = 0$ and $\gamma_{YM} = 0$), the total effect of β_{MX_i} to M_i^B will, however, be estimated accurately as $\gamma_{Y\beta}$.

The current trend in models that integrate dynamic aspects of affective experiences into models predicting future outcomes seems to be that mean levels in affective experiences are controlled for (Brose et al., 2021; Charles et al., 2013; Dejonckheere et al., 2019; Rackoff & Newman, 2020; Rush et al., 2019). In fact, Dejonckheere et al. (2019) found that indicators of affect dynamics often add little information in predicting future outcomes in well-being after controlling for mean levels of affective experiences. While we do not refute this possibility and the pattern of findings and the data-generating model may be very different for different dynamic aspects of affective experiences, we emphasize here that *under the assumption that data were generated in accordance with the model in Figure 3 and when investigating the predictive utility of between-person differences in within-person effects*, controlling for mean levels in affective experiences leads to a bias in the estimated effect if the indirect effect of the within-person effects on the outcome via mean levels in affect is not accounted for. We will return to the important conceptual implications of this issue when we present the results from the empirical example.

Estimation

The model depicted in Figure 3 can be estimated using MSEM with the Bayesian estimator in Mplus. A detailed introduction into Bayesian statistics or the Bayesian estimator in Mplus is beyond the scope of this work, and we refer to standard introductory texts for more detail (e.g., Gelman et al., 2014; McElreath, 2016; Muthén & Asparouhov, 2012). In a nutshell, the Bayesian estimator in Mplus uses Markov Chain Monte Carlo (MCMC) estimation to construct the posterior distribution of all model parameters. As typical for Bayesian estimation, the analysis requires that prior distributions for all model parameters are specified. In our analyses, we used the default priors implemented in Mplus, which are flat priors for all parameters, but we note that when prior information on the likely range of the parameters is available, weakly informative priors may facilitate model convergence (Lüdtke et al., 2018). By default, Mplus terminates the sampling process after the chains (by default:

2) have converged as indicated by a maximal potential scale reduction (PSR) close to 1. This default behavior may, however, be problematic because it may result in early termination of the estimation at a local minimum. To circumvent this problem, a pre-defined number of iterations can be set and convergence of the two chains can be checked after the estimation has terminated via statistical criteria such as the obtained PSR (and/or the effective sample size, see Zitzmann & Hecht, 2019) and visual inspection of the trace plots and autocorrelation plots of the draws from the posterior distribution (see also Hamaker et al., 2018). Central elements of the summary results provided by the Mplus output are, for each parameter, a point estimate (the median of the posterior distribution of this parameter), and the 95% credible interval of this parameter.

Simulation Study

Method

To examine the performance of the model introduced above (see Figure 3), we employed a simulation study. Specifically, we simulated data from a model with the population parameters as listed in Table 1. Hence, there were $3 \times 2 \times 4 \times 3 = 72$ conditions in total. For each condition, 200 data sets were created in R. These 14,400 data sets were then analyzed in Mplus version 8.3 for Linux. The MplusAutomation package was used as an interface between R and Mplus (Hallquist & Wiley, 2018). We set a fixed number of iterations (here: 20,000 per chain), the first 50% of which were discarded as burn-in. A thinning factor of 20 was used to reduce the autocorrelation of the draws from the posterior and, hence, only every 20th iteration was retained to construct the posterior distributions. The median and 95% credible interval were therefore based on the 1000 iterations that were retained. The model was then estimated as depicted in Figure 3. For parameter estimation, we focused on the following three Level-2 regression coefficients: $\gamma_{M\beta}$, $\gamma_{Y\beta}$, and γ_{YM} .

Additionally, we targeted the indirect effect of β_{MX_i} on Y_i^B via M_i^B , and the total effect of β_{MX_i} on Y_i^B , computed as the sum of the indirect effect and the direct effect of β_{MX_i} . Key outcome

variables were absolute bias, relative bias, and coverage rate. *Absolute bias* was computed as the difference between the estimated parameter in repetition r ($\hat{\theta}_r$) and the true parameter value (θ_r): $\hat{\theta}_r - \theta_r$. *Relative bias* was calculated as the proportion of absolute bias relative to the parameter's true value: $(\hat{\theta}_r - \theta_r) / \theta_r$. Note that relative bias is only defined for conditions in which the true parameter does not equal zero. *Coverage rate* refers to the proportion of repetitions in which the true parameter fell into the 95% credible interval of the parameter estimate. Simulation and analysis scripts can be found in the accompanying OSF repository (<https://osf.io/c2xhu/>).

Results

Model estimation was unsuccessful in four (out of 14,400) data sets, all of which were in the condition with the lowest number of participants ($N = 40$) and measurement occasions ($T = 8$). These data sets were discarded. All remaining models converged successfully (PSR < 1.02 for all). All five parameters of interest could be recovered well (see Online Supplement for detailed results). With a small sample size ($N = 40$ and $T = 8$), the total and direct effects of β_{MX_i} on Y_i^B were slightly overestimated, while the indirect effect was negatively biased (see Figure 4). With larger sample sizes, these biases disappeared and with a Level-2 sample size of $N = 100$ or higher, these biases were eliminated and 95% coverage rates ranged from .910 to .985.

In additional models, we examined the impact of model misspecifications on parameter estimates. Specifically, we examined the consequences of three conditions: In *Misspecification 1*, the effect of the slope variable (β_{MX_i}) on M_i^B was not estimated ($\gamma_{M\beta} = 0$). In *Misspecification 2*, the effect of M_i^B on Y_i^B was additionally set to zero (i.e., $\gamma_{M\beta} = 0$ and $\gamma_{YM} = 0$). Finally, in *Misspecification 3*, we set the effect of X_i^B on M_i^B to zero ($\gamma_{MX} = 0$). Results showed that these misspecifications led to expected biases in parameter estimates (see Online Supplement for further details).

Together, findings from the simulation study demonstrate that the true parameters of the model in Figure 3 can be recovered well when the number of Level-2 units is 100 or larger, and that misspecifications of the model can lead to both positive and negative bias in the estimated total effect estimate depending on the nature of the misspecification.

Empirical Example: Predicting Change in Depressive Symptoms from the Within-Person Association of Stress and Negative Affect

Prior work has linked between-person differences in stress reactivity, that is, in the within-person coupling of the occurrence of minor stressors (daily hassles) and affective well-being, to between-person differences in psychological adjustment. For example, Charles et al. (2013) reported results suggesting that between-person differences in stress reactivity predict change in psychological distress 10 years later. Similarly, stress reactivity has been linked to other outcomes as well, such as subsequent sleep quality (Ong et al., 2013) or mortality (Mroczek et al., 2015). However, prior work differs in whether or not between-person differences in stressor exposure (X) and/or affective well-being (M) were used as covariates. This inconsistent approach towards modeling between-person differences in an outcome as a function of between-person dynamics may have led to both, an underestimation of the reported effect (see Misspecification 1), or an overestimation of the effect (see Misspecification 3). Furthermore, none of the previous studies has considered indirect effects of stress reactivity (β_{MX_i}) on outcomes (Y_i^B) via average affective well-being (M_i^B). All in all, not exploring the role of average stress and average negative affect in the hypothesized model may lead to inconsistent findings in the literature. To illustrate how the model introduced in this work can be applied to empirical data, we utilized data from the COGITO study, a 100-day intensive longitudinal study with 101 younger adults (20-31 years) and 103 older adults (65-80 years).

Method

The COGITO study is a laboratory-based, intensive longitudinal study with a pre-post assessment enveloping the repeated assessment of cognitive performance measures and self-reports over approximately 100 days. Participating adults visited the laboratory each day to work on a battery of cognitive tests and to provide information on various aspects of their day and related subjective experiences. Before and after this longitudinal phase of the study, participants also completed additional cognitive tasks and comprehensive self-report measures. The present analyses utilize data from the pre- and post-assessment (depressive symptoms) and the intensive longitudinal assessments (negative affect and the occurrence of daily stressors). For more details on the COGITO study see Schmiedek et al. (2010).

Depressive symptoms. Both at pre-test and post-test, participants completed the German version (Hautzinger, 1988) of the Center for Epidemiological Studies Depression scale (Radloff, 1977), which captures depressive symptoms over the last two weeks. Responses to the 16 items were averaged to a measure of depressive symptoms at baseline, McDonald's $\omega = .87$, and at post-test, $\omega = .90$, respectively.

Negative affect. On each day during the intensive longitudinal phase of the study participants reported on their negative affect today. Ten items were administered, only five of which were used in the present work. We chose these five items to stay consistent with prior use of this data that has discarded the remaining five items due to low within-person fluctuations; see Brose et al. (2011) for details on the psychometrics of the negative affect scale in the COGITO study.

Daily stressors. Participants were asked each day whether they experienced one or more of seven events on this day: argument with someone; disagreement over something without clarification; some work-related event; an event related to a friend; a health-related event; an event in leisure; and an event related to finances. These categories were delineated from the Daily Inventory of Stressful Events (Almeida et al., 2002). A dichotomous variable

was created for each person and day indicating if at least one of these seven stressful events had occurred today (coded as 1) or not (coded as 0).

Data Analyses. Before the analyses, we first centered the dichotomous Level-1 predictor (occurrence of at least one stressor) on the manifest person mean, and the person mean (i.e., an individual's proportion of days on which at least one stressor was reported) on the grand mean. We note that latent person mean centering of dichotomous predictors is possible in Mplus (Asparouhov & Muthén, 2019), but when applying this approach to our data in Mplus version 8.3, results suggested that the model has clearly not converged successfully (PSR = 10.626). This issue could also not be resolved by more thinning or changing the prior distributions of the model parameters. We therefore report the results based on the manifest person mean centering below.

Another aspect that we added in this model compared to the models introduced above is that we controlled for depressive symptoms prior to the intensive longitudinal phase. By controlling for baseline levels in depressive symptoms, the outcome variable in this model becomes *change* in depressive symptoms from before to after the 100 days of daily data collection (Castro-Schilo & Grimm, 2018). We allowed baseline levels of depressive symptoms to correlate with the two predictors (average stress and between-person difference in stress reactivity), and also controlled for the effect of baseline depressive symptoms on mean negative affect. Hence, baseline depression was modeled to predict both average negative affect and depressive symptoms at the post assessment (see Figure 5). Mplus input and output files can be found online (<https://osf.io/c2xhu/>).

As explicated above, the introduced model rests on the assumption that the directed effects represent causal effects, an assumption that needs to be defended in each individual application of the model. In the present scenario, we build on several lines of research to justify the assumption of causal effects. First, prior experimental work showed that inducing stress leads to increases in negative affect (e.g., Henze et al., 2017), suggesting that directed

effects from stress to negative affect are tenable. Second, longitudinal research has shown that average daily negative affect has been associated with subsequent changes in psychological distress (Brose et al., 2020). Similarly, longitudinal research has linked stress reactivity to subsequent changes in depressive symptoms (Charles et al., 2013). Notably, longitudinal studies cannot unequivocally prove causal effects since they cannot rule out third-variable explanations. They may, in combination with plausible theoretical pathways (see, e.g., Kiecolt-Glaser et al., 2020), and when controlling for potentially relevant confounders (e.g., as in the present model baseline depressive symptoms) be considered as tentative support for an assumed causal effect. Finally, the presumed causal link between stress reactivity and average negative affect is built on the considerations explicated in the section on the within-person experimental design. These considerations rest on the assumption of stationarity, which also implies that averages in M do not affect stress reactivity. Although this remains an assumption of the model that cannot be tested in the present scenario, we add that this is a standard assumption of multilevel modeling approaches. Hence, this is not a unique assumption that needs to be made for our proposed model, but one that needs to be made in all standard applications of multilevel models (and MSEM) in general. We turn to this issue in more detail in the Discussion section.

Results

The two MCMC chains converged successfully, as indicated by a maximal PSR of 1.005. Trace plots and autocorrelation plots of the parameters were inspected visually and revealed no irregularities. Both, a greater proportion of stressor days, $b = .435$, and higher stress reactivity, $b = .703$, were associated with higher average negative affect – these effects were statistically meaningful⁴ after controlling for baseline depressive symptoms. Higher negative affect, in turn was associated with a stronger increase in depressive symptoms from

⁴ We deemed a parameter as statistically meaningfully different from zero if its associated 95% credible interval did not contain zero.

baseline to the post assessment, $b = .095$. Only the direct effect of stress reactivity, $b = .228$, but not average stress, $b = .102$, on change in depressive symptoms was statistically meaningful.

We further examined the indirect effect of stress reactivity via average negative affect and the total effect estimate of stress reactivity. Both effects were statistically meaningful: indirect effect = .063 [.018, .135]; total effect = .292 [.114, .484].

Had we not considered the indirect effect in estimating the total effect of stress reactivity on change in depressive symptoms, the total effect would have been estimated as about 21.5% lower (the ratio of the indirect effect to the total effect). In contrast, if the effect of negative affect on change in depression had not been estimated either (i.e., if we had not controlled for the average level of the mediator), the total effect of stress reactivity on change in depression would have been estimated in a more similar range as the total effect in the full model. If, on the other hand, the effect of the proportion of stressor days on negative affect had not been estimated, the effect of stress reactivity on negative affect would hardly have changed since the stress reactivity and the proportion of stressor days were only weakly associated in this sample, $r = -.151$ [-.347, .064]. Note, however, that, because the covariance between stress reactivity and the proportion of stressor days was negative, a small decrease in the estimated indirect effect and total effect would be expected if the effect of average stress to negative affect was not estimated.

To demonstrate this pattern, we estimated three more models, in which the effect of stress reactivity on average negative affect was not estimated (Model 1a; see also Misspecification 1 in the simulation study), in which both the effect of stress reactivity on average negative affect and the effect of average negative affect on change in depression were not estimated (Model 1b; Misspecification 2), or in which the effect of the proportion of stressor days on negative affect was not estimated (Model 1c; Misspecification 3).

In Model 1a, the estimated effect of stress reactivity on change in depressive symptoms was $b = .231$, and hence, close to the estimated direct effect estimated in the full model ($b = .228$). In Model 1b, the estimated effect of stress reactivity on change in depressive symptoms was $b = .296$, and hence, close to the estimated total effect in the full model ($b = .292$). Results in Model 1c also confirmed expectations: the effects of stress reactivity on negative affect, $b = .597$, the indirect effect, $b = .055$, and the total effect, $b = .276$, of stress reactivity on change in depression were lower than the estimated parameters in the full model.

Discussion

In recent years, theoretical and conceptual work has accumulated suggesting that everyday experiences may shape change in various psychological outcomes over a longer time span (Bleidorn et al., 2020; Nesselroade, 1991; Wrzus & Roberts, 2017). This prior work has laid the foundation for understanding if and how within-person processes might shape phenomena such as between-person differences in personality (Quintus et al., 2020) or health and well-being (Charles et al., 2013). In addition, the more and more widespread implementation of measurement burst studies (Sliwinski, 2008) has resulted in rich data sets that allow to tackle these important research questions. However, thus far, no data-analytic framework has been introduced that allows to examine the conceptual question, if and how within-person processes may be transformed into between-person differences. We showed that previous accounts in the framework of multilevel mediation fall short for this purpose. The present work emphasizes the crucial role of between-person differences in within-person effects in this regard: We suggest that these differences can contribute to between-person differences in future outcomes by affecting the mean levels of a Level-1 mediator.

Implications for Other Hierarchical Designs and Interventions

Throughout this work, we focused on the application of the proposed model to intensive longitudinal data. We hasten to add, however, that this model can also be applied in

other settings with hierarchically nested data, and the implications of this model extend to these designs as well. For example, in educational contexts, students are typically considered as nested within higher organizational units (e.g., classrooms or schools). The introduced model can aid in understanding how differences between classrooms can emerge from within-class associations. For example, teachers of classes in which students show a stronger association of victimization (X) and externalizing behavior (M) might be more prone to burn out (Y) compared to teachers of classes in which this association is less strong: If students respond to experiences of victimization (e.g., bullying) with more externalizing behavior, this may be associated with more exhaustion of the teacher of this classroom (compared to a classroom with equal amounts of bullying, but in which bullying is less strongly associated with students' externalizing behavior). The framework introduced in this study therefore has widespread implications for other research designs beyond ILDs.

Understanding heterogeneity in Level-1 effects across Level-2 units as a potential cause for Level-2 outcomes opens up exciting avenues for interventions as well. Going back to the student-classroom example, the effect of an intervention reducing bullying might be particularly effective for reducing teacher exhaustion in classes with a strong classroom coupling between bullying and externalizing behavior: If the link between victimization and externalizing behavior is particularly strong, reducing bullying should result in a stronger decrease of the classroom's average externalizing behavior, and consequently, a reduction in this classroom's teacher's exhaustion. Similarly, in ILDs, our approach highlights that between-person differences in within-person effects may provide very useful information to target individuals with the highest need to intervene: By identifying participants with potentially maladaptive within-person effects (e.g., high stress reactivity), it may be possible to identify risk factors that would remain hidden if only the average levels (e.g., average stress and average negative affect) were investigated. Note, however, that the promises of Level-1 effects as diagnostic instruments (aiding to identify which Level-2 units might profit from

which intervention to a particular degree) hinges on the reliability of the estimation of these couplings which may be rather low in many settings (Liu et al., 2019; Neubauer, Voelkle, et al., 2020).

Between-person differences in within-person effects might also be the target of interventions and the active ingredient in explaining the effect of interventions on between-person differences in clinically relevant outcomes. For example, results from a recent study on adults with binge eating disorders (Smith et al., 2020) suggest that there may be a de-coupling between momentary negative affect and subsequent binge eating disorder 6 months after treatment termination. Further, those patients who showed a stronger decrease in the within-person coupling of negative affect and binge eating also exhibited better treatment outcomes. Similarly, mindfulness-based therapeutic approaches or emotion regulation trainings focus on decreasing participants' responses to events in their daily lives (Wenzel et al., 2021). These accounts emphasize that within-person effects might in fact be malleable by interventions and account for between-person differences in the effectiveness of treatments on long-term outcomes.

Furthermore, the model introduced in this work can serve as a useful data-analytic framework for studies using interventions in individuals' everyday lives. So called micro-randomized trials (Klasnja et al., 2015) are using study designs that provide interventions on a within-person level. That is, interventions are provided on some, but not all measurement occasions, which allows to examine causal effects in study participants' daily lives (see also Schmiedek & Neubauer, 2020, for a related approach). For instance, participants might receive notifications on their smartphone to increase physical activity on some measurement occasions (vs. other notifications or no notifications on other measurement occasions; see Klasnja et al., 2019). Micro-randomized trials examine the effect of the implemented intervention on a proximal outcome (here: step count after the notification), but they can also target more distal outcomes (e.g., physical health outcomes). As shown in the present work,

between-person differences in the effectiveness of the intervention (the within-person effect of the intervention on the proximal outcome) provide relevant information to address the question, how the within-person intervention can impact distal outcomes: Those individuals who benefit more from the intervention on the daily level (for whom the effect of the intervention on the proximal outcome is stronger) will show a more positive long-term development in the distal outcome.

Implications for the Role of Average Experiences

If the core aim in a study is to investigate whether between-person differences in within-person effects ($X \rightarrow M$) predict future outcomes, researchers are often confronted with the question which, if any, covariates to include in the model. In this context the average levels of momentary experiences of X and M are often included to demonstrate that these more complex parameters explain variance in the outcome above and beyond more parsimonious parameters such as mean levels of time-varying variables. This approach has been fueled further by recent work arguing that more complex parameters obtained in ILDs often add limited information in the prediction of future outcomes above and beyond mean levels (Dejonckheere et al., 2019). The model introduced in this work has important implications for considering whether or not mean levels in experiences are controlled for when predicting between-person differences in future outcomes. Specifically, we argue that if mean levels in both the Level-1 predictor (X) and the Level-1 mediator (M) are controlled for, the effect of between-person differences in within-person effects on the outcome (Y) is biased if part of this effect is actually mediated via average levels in the Level-1 mediator.

In terms of the empirical example utilized, if (part of) the reason that individuals with higher stress reactivity show stronger increases in depressive symptoms is that they experience higher negative affect on average, the estimated effect of stress reactivity on depression would be biased if the effect of average negative affect on depression was controlled for. The consequences of controlling for mediators for the estimation of the true

causal effects has been documented and elaborated upon previously (e.g., Rohrer, 2018; Rosenbaum, 1984), but the implications for predicting between-person differences from within-person effects have not been laid out yet. For the context of the present work, it is therefore important to emphasize that controlling for mean levels in the Level-1 mediator (M) in models in which between-person differences in within-person effects ($X \rightarrow M$) predict future outcomes can lead to biased estimates of the true effect of within-person effects.

We therefore caution against a default according to which mean levels in time-varying mediators are included as covariates in models with between-person differences in within-person effects as the central predictor *without a clear and conceptual explication of these variables' roles in the model* (see also Brose et al., 2021): Are mean levels included because the interest lies in the unique effect of between-person differences in within-person effects? Or are they included as potential mediators? In the former case, controlling for mean levels can be appropriate. In the latter case, the indirect effect of between-person differences in within-person effects needs to be considered in determining the total effect of this predictor.

A Note on Model Assumptions

The model introduced in this work comes with a number of assumptions that may not hold in some empirical applications. One particularly important assumption refers to the causal order of variables. As in any mediation model, the direction of effects cannot be unequivocally determined using observational data. Experimental manipulation of the presumed causes remain the *via regia* towards determining causality. Of course, data resulting from experimental designs can be analyzed using the model introduced in the current article. As introduced above, the time-varying predictors can be manipulated on the within-person level (e.g., in microrandomized trials or in within-person encouragement designs; Klasnja et al., 2015; Schmiedek & Neubauer, 2020), as can the predictor (X) and mediator (M) on the between-person level. It is theoretically also possible to experimentally manipulate the strength of the within-person effect (see Smith et al., 2020; Wenzel et al., 2021). Designs that

cross manipulations of multiple predictors would be the ideal way to defend the assumption of causality. However, in practice, the combination of manipulation of all predictors in the model is likely unrealistic (both practically and ethically) in most empirical settings.

Using longitudinal data can help in strengthening the argument for possibly causal effects. In this context, measurement burst designs (Sliwinski, 2008), in which intensive longitudinal measurement bursts (e.g., daily diary studies, ambulatory assessments) are repeated with the same individuals across time, might be useful to approach the lead-lag association between within-person effects and mean levels of outcomes: For instance, is stress reactivity associated with an increase in negative affect at the following burst? Or do participants with higher negative affect at one burst show an increase in stress reactivity at the next burst? Or is there a reciprocal association between stress reactivity and average negative affect across bursts? Alternatively, individuals might however also develop a blunted stress response when they experience high levels of negative affect across an extended time period, which would result in a negative association between mean levels of negative affect and subsequent stress reactivity. It is important to add two aspects when it comes to the benefits and limitations of longitudinal studies in this context: First, relating to the issue of causality, temporal precedence of the antecedent before the outcome is a necessary, but not sufficient condition to establish causality because third variables might still account for the observed covariance across time. Second, relating to the assumption of stationarity (no directed effect from mean M to the effect for $X \rightarrow M$), the process is assumed to be stationary *within a measurement burst*. That is, even if mean negative affect at one burst is associated with change in stress reactivity to the next burst, the assumption of stationarity within a burst is not necessarily violated. If the effect of mean negative affect on the presumed predictor (stress reactivity) occurs on a slower time frame (across months) but mean levels have no immediate effects on stress reactivity within the time frame of a measurement burst (e.g., from one day to the next), the stationarity assumption of the model is not violated.

As a final note on causality, a mediation model based on observational data can yield results consistent with a presumed causal model, but it cannot prove it. Building on prior theoretical and empirical work is always necessary to justify the (strong) assumptions of causality in mediation models. The model introduced in the present article is no exception from this general rule to mediation models. As recently advocated by Grosz et al. (2020), there is a need to more explicitly discuss the implied causality among variables also in observational, nonexperimental data. The model in our empirical example is based on the assumption of causal effects of stress reactivity on negative affect and on change in depression. In this specific example, the alternative of experimental manipulation of stress reactivity to test its causal effects on long-term change in depression does not seem feasible (neither practically nor ethically). We therefore think it is important to explicate these underlying causal structures of the model and discuss the findings based on observational data in light of these assumptions. Overall, a bouquet of various approaches is needed to uncover the true data generating, causal model. To that end, a combination of approaches, comprising the inclusion of potential confounders and – overall – the causal network of the variables in the model (including controlling for potential third variable confounders), as well as experimental studies targeting at least part of the presumed causal effects in the model is required (see also Rohrer, 2018).

Predictors and Outcomes of Between-Person Differences in Within-Person Effect

In the present work, within-person effects were used as a predictor of later between-person differences in an outcome. Prior work has, however, often considered the links of between-person differences in, for instance, personality and within-person effects treating personality as predictor of within-person effects. In this context, one prominent example is the personality trait Neuroticism that has been considered a predictor of mean levels in stress, negative affect, and the within-person effect stress reactivity (e.g., Bolger & Schilling, 1991; Bolger & Zuckerman, 1995). Hence, whereas the approach suggested in the present work

started from the perspective of 1-1-2 mediation models, this prior work has taken a 2-1-1 perspective (with between-person differences in personality shaping within-person effects). Notably, these two approaches are not mutually exclusive, but they supplement each other: Between-person differences in one construct (e.g., Neuroticism) might shape within-person effects, which in turn shape the development of between-person differences across a longer time span. To illustrate this, consider results from a recent measurement burst study reporting that changes in Neuroticism are coupled with changes in stress reactivity (Wrzus et al., 2021). This finding suggests that stress reactivity is not only an outcome of between-person differences in Neuroticism (Bolger & Schilling, 1991; Bolger & Zuckerman, 1995), but that changes in this within-person effect might also shape changes in Neuroticism as well. This again highlights the potential and usefulness of targeting between-person differences in within-person effects – as outcomes (e.g., Bolger & Zuckerman, 1995), but also as predictors of future outcomes (e.g., Charles et al., 2013).

Promises and Limitations

With the present work, we introduce a mediation model with a path that, to the best of our knowledge, has not been considered in previous work using data from ILDs: a directed effect of between-person differences in within-person effects to average levels in the Level-1 mediator, which, in turn, may explain the effect of within-person effects on some distal outcome variable *Y*. This proposed model employs a one-step approach in that between-person differences in within-person effects and their association with outcomes (*M*, *Y*) are estimated in one common model, which is in general preferable to a two-step approach (estimating between-person differences in within-person effects in one step, and their associations with outcomes in the second step; see Brose et al., 2021; Skrondal & Laake, 2001). We are confident that considering this model in future work can substantially contribute towards understanding how daily experiences translate into between-person differences and how they shape human development. Nevertheless, we caution that a

meaningful interpretation of the model's results rest on whether or not the underlying assumptions (in particular stationarity and correct specification of causality) are tenable.

Reflections of model assumption help to determine if the introduced model is sensible in a specific research setting. For instance, consider research in the framework of environmental sensitivity (Pluess, 2015). This theoretical account assumes that individuals differ in their responses to changes in the environment (for better and for worse): For some individuals, their experiences (the outcome M , e.g., affective well-being) depend strongly on, for example, the occurrence of stressors or uplifts (predictor X), whereas others are less strongly affected by these environmental influences. It has been proposed that individual differences in sensitivity of the central nervous system might underlie differences in responsivity to changes in the environment (e.g., Pluess, 2015). Assuming that this form of sensitivity is a (physiological) property of the individual that is not affected by individuals' momentary experiences (at least not across a time course of days or weeks), a directed effect from responsivity (which can be operationalized as the within-person effect of X predicting M ; Sicorello et al., 2020) to average experiences (individual's mean in M) in constant environments (controlling for mean in X) can be assumed. In this example, reverse causal effects from average M to the effect of X on M (from daily experiences to responsivity) are theoretically less plausible and the model introduced in this article is reasonable.

In contrast, consider the process giving rise to the release of cortisol in the human body. In response to challenges to the environment (e.g., stressors), cortisol is released via the hypothalamic–pituitary–adrenal (HPA) axis into the bloodstream. Rising cortisol levels, in turn, have a negative feedback effect on the HPA axis and thereby reduce the release of further cortisol. That is, there is a causal effect of the output (cortisol) on the process giving rise to the output (the regulation of the HPA axis) and therefore the stationarity assumption of the model introduced here is violated. In consequence, our model would not be an appropriate choice in this case. In such settings, adding a directed effect from between-person differences

in within-person effects to mean levels in M is not defensible because their association is reciprocal. Therefore, it would be more appropriate to control for mean levels in M when the interest lies in effect of between-person differences in within-person effects on a distal outcome. These considerations demonstrate that the model we introduce is not a general approach that will be useful in all scenarios – its usefulness rests on the tenability of the underlying model assumptions that need to be evaluated in empirical applications on a case-by-case basis, informed by prior empirical and theoretical work. We are confident, however, that the model can be a useful tool to better understand the dynamics of everyday life and how they relate to long-term development in a variety of settings.

Conclusions

In multiple areas of psychological research, a central question is how between-person differences emerge, change, and are malleable to interventions. Daily life experiences may be central to answer these questions, and current trends in the collection of intensive longitudinal data, in particular measurement burst studies, provide a very promising basis to tackle these important questions. In the present work, we propose a data-analytic framework to approach the idea that daily life experiences might affect subsequent between-person differences. We demonstrate that traditional multilevel mediation models fall short in explaining this phenomenon and suggest that between-person difference in within-person effects (random slopes in the multilevel modeling terminology) play a crucial role in this regard. Our modeling approach has important implications with regard to the role of mean levels of daily experiences, which may serve as mediators of the effect of within-person dynamics on subsequent between-person differences.

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Table 1

Population values of the model parameters in the simulation study

Parameter	Population value(s)
$u_X^2, \sigma_X^2, \sigma_M^2, \sigma_Y^2, u_\beta^2$	1
u_M^2, u_Y^2	Chosen such that the total Level 2 variance in M and Y = 1
$[\alpha_X], [\alpha_M], [\alpha_Y]$	0
$\gamma_{MX} (= a_b)$	.15
$\alpha_\beta (= a_w)$	0
$\gamma_{YM} (= b)$	.30
$\gamma_{YX} (= c')$	.10
$\gamma_{Y\beta}$	.20
$\gamma_{M\beta}$	0 / .20 / .50
$u_{\beta X}$	0 / .50
Level 2 sample size (Number of participants)	40 / 100 / 200 / 500
Level 1 sample size (Number of repeated assessments)	8 / 20 / 100

Figure 1. A general 1-1-2 mediation model with random slope for the 1-1 link.

Figure 2. Displays data from two hypothetical participants in the within-person experiment. The within-person effect of X is smaller for participant $i = 1$ than for participant $i = 2$. Filled dots represent participants' means in M across all $n_1 + n_0$ observations. The shaded dots represent the means in M of these individuals only at occasions when they did not receive the treatment and the grey bar represents the difference in mean M between these two participants only at occasions without the treatment. The purple line represents the difference in differences, that is the difference between the two participants in means across all observations minus the difference between these two participants in means of observations at no treatment occasions only.

Figure 3. A statistical model explaining how between-person differences in within-person effects (β_{MX_i}) predict between-person differences in an outcome (Y_i^B) via between-person differences in a mediator (M_i^B). Parameters that have been added compared to the model in Figure 1 are highlighted in red.

Figure 4. Results from simulation study. Error bars indicate 95% bootstrap confidence intervals.

Figure 5. Depicts unstandardized estimates and associated 95% credible intervals in brackets.

Arrows in bold are the central effects of the mediation model. NA = negative affect; Depr = depressive symptoms.

Appendix

Expectancies for M_{it}

In the hypothetical within-person experiment, the time-varying outcome M_{it} is regressed on the time-varying predictor X_{it} (with $X_{it} = 0$ if no treatment was given to participant i on occasion t ; and with $X_{it} = 1$ if the treatment was given to participant i on occasion t), yielding the following regression equation (see also Equation (9))

$$M_{it} = \gamma_{00} + v_{0i} + \gamma_{01} \cdot X_{.i} + \gamma_{10} \cdot X_{it} + v_{1i} \cdot X_{it} - \gamma_{10} \cdot X_{.i} - v_{1i} \cdot X_{.i} + \varepsilon_{it} \quad (A1)$$

In the following, the expected differences in M_{it} are delineated for two individuals who differ in their within-person effect v_{1i} . Let v_{1i} be s_1 for $i = 1$ and v_{1i} be s_2 for $i = 2$. The expectancies in M_{it} for these two individuals can be expressed as follows:

$$E(M_{it} | i = 1) = \quad (A2)$$

$$\gamma_{00} + E(v_{0i} | i = 1) + \gamma_{01} \cdot E(X_{.i} | i = 1) + \gamma_{10} \cdot E(X_{it} | i = 1) + s_1 \cdot E(X_{it} | i = 1) - \gamma_{10} \cdot E(X_{.i} | i = 1) - s_1 \cdot E(X_{.i} | i = 1) =$$

$$\gamma_{00} + E(v_{0i} | i = 1) + \gamma_{01} \cdot E(X_{.1})$$

$$E(M_{it} | i = 2) = \quad (A3)$$

$$\gamma_{00} + E(v_{0i} | i = 2) + \gamma_{01} \cdot E(X_{.i} | i = 2) + \gamma_{10} \cdot E(X_{it} | i = 2) + s_2 \cdot E(X_{it} | i = 2) - \gamma_{10} \cdot E(X_{.i} | i = 2) - s_2 \cdot E(X_{.i} | i = 2) =$$

$$\gamma_{00} + E(v_{0i} | i = 2) + \gamma_{01} \cdot E(X_{.2})$$

The difference between (A3) and (A2) is the expected difference between these two individuals across all observations (both with and without the treatment). This difference is:

$$\gamma_{00} + E(v_{0i} | i = 2) + \gamma_{01} \cdot E(X_{.2}) - [\gamma_{00} + E(v_{0i} | i = 1) + \gamma_{01} \cdot E(X_{.1})] = \quad (A4)$$

$$E(v_{0i} | i = 2) - E(v_{0i} | i = 1) + \gamma_{01} \cdot [E(X_{.2}) - E(X_{.1})]$$

Next we compute the expectancies of the same two individuals for occasions without the treatment ($X_{it} = 0$). The expectancies can be expressed as follows:

$$E(M_{it} | i = 1, X_{it} = 0) = \quad (A5)$$

$$\gamma_{00} + E(v_{0i} | i = 1, X_{it} = 0) + \gamma_{01} \cdot E(X_{.i} | i = 1, X_{it} = 0) + \gamma_{10} \cdot E(0 | i = 1) +$$

$$s_1 \cdot E(0|i = 1) - \gamma_{10} \cdot E(X_{.i}|i = 1, X_{it} = 0) - s_1 \cdot E(X_{.i}|i = 1, X_{it} = 0) =$$

$$\gamma_{00} + E(v_{0i}|i = 1) + \gamma_{01} \cdot E(X_{.1}) - \gamma_{10} \cdot E(X_{.1}) - s_1 \cdot E(X_{.1})$$

$$E(M_{it}|i = 2, X_{it} = 0) = \tag{A6}$$

$$\gamma_{00} + E(v_{0i}|i = 2, X_{it} = 0) + \gamma_{01} \cdot E(X_{.i}|i = 2, X_{it} = 0) + \gamma_{10} \cdot E(0|i = 2) + s_2 \cdot E(0|i = 2) - \gamma_{10} \cdot E(X_{.i}|i = 2, X_{it} = 0) - s_2 \cdot E(X_{.i}|i = 2, X_{it} = 0) =$$

$$\gamma_{00} + E(v_{0i}|i = 2) + \gamma_{01} \cdot E(X_{.2}) - \gamma_{10} \cdot E(X_{.2}) - s_2 \cdot E(X_{.2})$$

The difference between (A6) and (A5) is the expected difference between these two individuals only in the occasions without the treatment. This difference is:

$$\gamma_{00} + E(v_{0i}|i = 2) + \gamma_{01} \cdot E(X_{.2}) - \gamma_{10} \cdot E(X_{.2}) - s_2 \cdot E(X_{.2}) - [\gamma_{00} + E(v_{0i}|i = 1) + \gamma_{01} \cdot E(X_{.1}) - \gamma_{10} \cdot E(X_{.1}) - s_1 \cdot E(X_{.1})] = \tag{A7}$$

$$E(v_{0i}|i = 2) - E(v_{0i}|i = 1) + \gamma_{01} \cdot [E(X_{.2}) - E(X_{.1})] - \gamma_{10} \cdot [E(X_{.2}) - E(X_{.1})] - s_2 \cdot E(X_{.2}) + s_1 \cdot E(X_{.1})$$

Lastly, we compute the expected difference in differences of these two individuals in M_{it} across all observation and in M_{it} in occasions without the treatment ($X_{it} = 0$) only

$$(A4) - (A7) = \tag{A8}$$

$$E(v_{0i}|i = 2) - E(v_{0i}|i = 1) + \gamma_{01} \cdot [E(X_{.2}) - E(X_{.1})] - [E(v_{0i}|i = 2) - E(v_{0i}|i = 1) + \gamma_{01} \cdot [E(X_{.2}) - E(X_{.1})] - \gamma_{10} \cdot [E(X_{.2}) - E(X_{.1})] - s_2 \cdot E(X_{.2}) + s_1 \cdot E(X_{.1})] =$$

$$\gamma_{10} \cdot [E(X_{.2}) - E(X_{.1})] + s_2 \cdot E(X_{.2}) - s_1 \cdot E(X_{.1})$$