Observed correlations from cross-sectional individual differences research reflect both between-person and within-person correlations

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Abstract:

The study of individual differences is often conducted using cross-sectional experiments. The cross-sectional nature of such data means that results obtained from these experiments are a mix of both between- and within-person associations. While concerns about this within-between confounding effect have been surfaced, they are often not addressed when reporting results from individual differences studies. This obfuscation threatens the interpretability and validity of cross-sectional individual differences research. We speculate that a lack of a clear and accessible description of the confounding effect may have contributed to the limited attention to this issue. In this paper, we develop a tractable description of the confounding effect and how it manifests. In doing so, we suggest a need for greater transparency in the reporting of the goals and results of cross-sectional studies and how they contribute to knowledge of individual differences.

Introduction

The study of individual differences has been a central aspect of psychological science for more than a century (Boogert et al., 2018; Cronbach, 1957; Dubois & Adolphs, 2016; Hariri, 2009; Kanai & Rees, 2011; Lebreton et al., 2019; Seghier & Price, 2018; Spearman, 1904a). One common goal in individual differences research is to understand the relationships among a set of measured dependent variables (DVs). For example, an experiment may be designed to investigate the relationship between intelligence and working memory capacity in a sample of representative individuals. Depending on the specific study, the DVs of interest can be derived from various types of measurements ranging from behavioral tasks and surveys (Eisenberg et al., 2019; Miyake et al., 2000; Unsworth, 2019) to physiological recordings (Dubois & Adolphs, 2016; Kanai & Rees, 2011; Madore et al., 2020).

Given the importance of individual differences research, it is critical to have a clear understanding of the validity of experimental designs being used to ensure that they are suitable for the goals of studies so that appropriate interpretations are generated from results. While the term ‘individual differences’ may suggest that any findings reflect purely between-person associations, this is not always true. In particular, the validity and interpretations of results obtained from cross-sectional experiments — where each DV is measured once for each individual — may be especially problematic. This is because the relationships between DVs observed in cross-sectional data actually reflect a combination of both the between- and within-person relations, a phenomenon that we will refer to as the *within-between confounding effect*.

While we provide more precise definitions for the various terminology used in this article in the next section, we introduce the confounding effect here by adapting an example from Bauer (Bauer, 2011). Suppose we conduct an experiment where multiple individuals’ negative affect and alcohol use in the past 24 hours are measured using surveys on one occasion. As is done in many cross-sectional individual difference studies, we might use a Pearson correlation or linear regression model to quantify what is, in this study, a positive relationship between negative affect and alcohol use. That is, individuals who report higher negative affect are likely to report higher alcohol use. Critically, however, we do not know whether individuals who have higher overall levels of negative affect are also more likely to have higher overall levels of alcohol use, a between-person association, or whether in moments when individuals have higher than usual levels of negative affect they are also likely to have higher than usual levels of alcohol use, a within-person association. To underscore this issue with an extreme hypothetical case, even in an experiment where all participants are identical clones of the same individual, we may still find a positive individual differences correlation between the DVs due to an observed correlation driven entirely by within-person associations (e.g. if negative affect and alcohol use covaries over the course of the day for all clones, and surveys for each clone are completed at different times of day). Thus, cross-sectional studies cannot differentiate between- and within-person associations that are conceptually distinct.

The impact of the confounding effect on the validity of conclusions obtained in a cross-sectional experiment depends on whether the goals of the experiment align with its design. Here, we consider two possible goals of an individual differences experiment: (1) ‘Between-goal’: the goal of the study is to estimate the associations of DVs at the between-person level only; in this case, a cross-sectional design is insufficient since it does not allow for separation of within- and between-person associations, and a repeated-measures design would be required. (2) ‘Mixed-goal’: the goal of the study is to estimate the associations of DVs without the need to separate within- and between-person associations; in this case, both cross-sectional and repeated-measures designs are sufficient. In this paper, we will not consider studies that specifically target within-person associations since a cross-sectional design would be clearly unsuitable for this purpose.

The choices of research goals (mixed-goal vs between-goal) and experimental designs (cross-sectional vs repeated-measures) are critical for understanding the validity of an individual differences study. In practice, however, the goals of individual differences studies are not always specified, and the confounding effect is also not commonly addressed in reports of cross-sectional research. This leads to difficulty in determining whether any specific study’s conclusions and interpretations are unwarranted due to a mismatch between study goal and experimental design. For example, a report of cross-sectional correlation between attention and long-term memory that does not specify the study’s targeted goal can be interpreted as either making invalid inferences about between-person associations (between-goal) or acknowledging that the results could be the result of within-person associations, as in the clone example described above (mixed-goal). This lack of clarity about a study’s goals and/or about the suitability of the experimental approach threatens the interpretability of results from cross-sectional studies. We speculate that the lack of an accessible description of the confounding effect may have contributed to the limited attention to these issues in the literature (Borsboom, 2006).

Though the within-between confounding effect has been acknowledged as a theoretical point in various contexts (Bauer, 2011; Institute of Medicine (US) Committee on Metabolic Monitoring for Military Field Applications, 2004; Steyer et al., 2015), these discussions are usually restricted to brief qualitative descriptions inserted in a broader discussion on repeated-measures analyses, and, to the best of our knowledge, quantitative treatment of the confounding effect in cross-sectional analyses is lacking. This is in contrast to other effects, such as the attenuation of correlation (Spearman, 1904b), that are well-documented and commonly addressed in individual differences research. We recognize that discussions of related topics such as ‘ecological fallacy’ and ‘ergodicity’ have received much attention for some time and that methods such as multilevel models have been developed to separate within- and between-person associations in repeated-measures data (Curran & Bauer, 2011; Molenaar, 2004; Peugh, 2010; Robinson, 1950). Complementing those discussions, the present paper focuses on understanding the confounding effect as it affects the interpretation and validity of *cross-sectional* results. We will largely omit discussions about repeated-measures data, for which there is a wealth of

existing literature. More specifically, rather than differentiating and separating within- vs. between-person associations, as is done in many repeated-measures studies, here we focus on comparing the observed cross-sectional association and the true underlying between-person association.

In this paper, we develop both conceptual and mathematical descriptions of the within-between confounding effect in the context of cross-sectional data. In doing so, we hope the present work will serve two purposes: (1) Provide a clear and accessible description of the confounding effect to demonstrate the critical need to specify research goals in cross-sectional experiments, and (2) make recommendations and advocate for better practices in the reporting of cross-sectional research to improve transparency and interpretability. We also discuss the confounding effect in relation to measurement reliability, given the latter's central role in individual differences research (Enkavi et al., 2019; Hedge et al., 2018). We constrain our discussion to linear relationships and explore analytical methods that are based on the covariance structures of DVs (the term associations and relationships will be used interchangeably with correlation/covariance). Importantly, while we focus our discussion on examples from behavioral human research, the confounding effect has broad implications and applies to any research program using cross-sectional data (physiological, neural, etc).

What are between-person and within-person associations?

In introducing the within-between confounding effect we used the terms between- and within-person associations. In this section we provide formal definitions for these terms as used in the context of this paper. We acknowledge that the definitions for the various terms used here may differ from their uses in other contexts. While the focus of this paper is on cross-sectional experiments, understanding within-person longitudinal dynamics of measurements is critical to understanding the confounding effect. In discussing these issues, we leverage the framework of latent state-trait (LST) theory, an extension of classical test theory (CTT), where each measurement is composed of three components: trait, state, and random measurement error (Novick, 1966; Steyer et al., 2015).

Within-person association

Continuing the negative affect-alcohol use example introduced earlier, we begin by exploring the repeated measurements of a single DV, negative affect, obtained from a single individual. Figure 1A shows the true score timecourse of negative affect for this individual (solid line), where the true score is defined as the measurement that would be obtained in the absence of any measurement error. The true score at any time point can be decomposed into trait and state values. As in CTT, the trait value (horizontal dotted line) represents the long-term negative affect true score (e.g. average negative affect over a month). For simplicity, we assume in this paper that trait values are stable. The state value at any specific time (double-headed arrow) represents short-term reversible state changes in negative affect relative to the trait value (e.g. daily changes

in negative affect relative to average). The variations due to state changes are considered distinct and independent from random measurement error, which is not depicted in Figure 1A.

Next, we extend our discussion to include measurements of a second DV, alcohol use, obtained from the same individual during the same time window (Figure 1B). In this example, the alcohol use and negative affect scores for this individual are positively correlated. Given that the trait scores are constant (i.e., have no variance), the observed correlation necessarily reflects the extent of association between state deviations in the two DVs. Thus, we focus on state variations in this section.

Consider an example cross-sectional measurement of both DVs from this individual. For example, a measurement of each DV would be obtained at the moment indicated by the dotted vertical line in Figure 1A and B. The single measurement marked by vertical lines in Figure 1A and B is then indicated by a black dot in Figure 1C. Generalizing from a single score on each DV to the distribution of scores on each DV, we can plot the joint distribution of the negative affect and alcohol use scores. The brown ellipse in Figure 1C shows the population joint distribution of the two DV true scores. The diagonal orientation of the ellipse reflects the alcohol use-negative affect relationship, indicating a positive correlation between the two DVs (in this paper we assume the relationships between DVs are stationary so the joint distribution is stable across time).

Of course, it is impossible to measure the DVs without some measurement error; we can also use the graphical representation to illustrate the effects of random measurement error (Figure 1D). The range of possible observed values from the example moment of measurement in Figure 1C is illustrated by the green circle centered around the true score at that moment with an example pair of observed values marked by 'x'. This error distribution is circular to indicate that error distribution of the random measurement errors for the two DVs are, by definition, not correlated. Applying this error distribution to all points in the true score joint distribution in Figure 1C we obtain the orange ellipse in Figure 1D (Spearman, 1904b). As can be seen, the diagonal orientation of this larger ellipse is attenuated, reflecting the fact that observed relations between variables are attenuated in the presence of measurement error. Thus, using this graphical representation, taking a cross-sectional measurement for both DVs from this individual corresponds to sampling a single point from this attenuated distribution. In this paper, the within-person association is defined as the attenuated correlation of the state values across DVs within each individual, which is graphically represented by the attenuated population joint distribution (Figure 1D). To avoid confusion, the term within-person association in this paper always refers to the attenuated (i.e. including random error) associations between DVs (Figure 1D), and the term within-person true score association refers to the true score associations (Figure 1C).

While it has been useful to discuss these concepts from a theoretical perspective for the purposes of clarifying the terminology, it is worth noting that, from a practical standpoint, it is difficult to separately estimate the true score state variance and random measurement error variance at the within-person level for a single DV (even with repeated-measures data).

However, it may be possible to obtain a lower-bound estimate for the true score state variance (or upper-bound estimate for random error variance) by looking at how much of the total within-person variance in a DV is explained by variance from other DVs because variance in random error is uncorrelated to anything. In addition, we emphasize that the true score is conceptually independent from the validity of a DV. For example, the true score of negative affect (as a measured manifest variable from questionnaires) in this example may not accurately reflect the individual's real underlying negative affect (as a latent variable) if the measurement instrument (questionnaires) has poor validity (Furr & Bacharach, 2008). In this paper, we always discuss variables as manifest variables rather than latent variables.

Figure 1. Within-person associations. (A) Negative affect true score timecourse (solid line) for a single individual; horizontal dotted line represents trait value and double-headed arrow represents an example state value. Vertical dotted line intersection with true score time course represents true score value at an example moment of cross-sectional measurement. (B) Alcohol use true score time course for the same individual with the same annotations as panel A. (C) Within-person true score association distribution of negative affect and alcohol use for a single individual; black dot corresponds to example cross-sectional measurement from panels A and B (vertical dotted lines); trait values shown as vertical and horizontal dotted lines. (D) Within-person association distribution of negative affect and alcohol use for a single individual shown as the orange distribution (includes measurement error). Random measurement error distribution for example time point shown as circular green distribution with 'x' showing an example measurement. a.u. = arbitrary units.

Between-person association

In the previous section, we presented scores and distributions of a single individual. We now consider alcohol use and negative affect true score timecourses for a group of individuals recruited from a larger population to participate in an experiment. Three example individuals' true score timecourses and trait values for the two DVs are shown in Figure 2A and B. As in the

previous section, we can use a graphical representation to view the relationship between the trait values of the by plotting the population joint distribution of the alcohol use and negative affect trait values (green ellipse in Figure 2C); the trait values of each individual in Figure 2A and B are depicted as single points in this population distribution. Note that Figure 2C shows the joint distribution of individuals' trait values, a between-person distribution, while Figure 1C shows the joint distribution of true scores (trait + state) for a single individual (i.e. each point in Figure 2C here represents the center of a within-person distribution shown in Figure 1C). Following from these two figures, the between-person association is defined in this paper as the correlation of the two DV trait values (graphically represented by the green population joint distribution of trait values in Figure 2C), while the within-person association is defined as the attenuated correlation of the repeated measures of two DV scores (graphically represented by the orange joint distribution of true scores in Figure 1D).

Figure 2. Between-person association. (A) Negative affect true score timecourses for three example individuals; trait values for each individual shown as horizontal dashed lines. (B) Alcohol use true score timecourses for the same three individuals in panel A. (C) Joint distribution of between-person differences in trait values of negative affect and alcohol use. The trait value of the three individuals from panels A and B are indicated as dots of corresponding color. a.u. = arbitrary units.

Finals comment on definitions

We end this section by acknowledging that the terminology used herein may have different meanings in other contexts. As an example, some might use within-person associations to describe moment-to-moment covariance between fMRI activity and reaction time on a Stroop task while still labeling the data as cross-sectional because the data can be collected from a single scan session (i.e. correlations between the trial-by-trial level fluctuations of these variables from a single scan session). However, using the definitions of this paper, we would instead describe this example as a repeated-measures experiment with each trial as a separate cross-sectional measure of the DVs (single trial reaction time and fMRI activity). The use of between/within may also be confusingly similar and may have slight differences to terms such as group/individual or inter/intra individual in other contexts. We will not attempt to reconcile these different terminologies; instead, we hope that we will have illustrated the concepts of this paper clearly enough so that these issues can be discussed in whatever language is necessary given the circumstances of each research program.

Qualitative description of confounding effect in bivariate case

With the terminology clarified and mapped to useful graphical representations, we turn to our examination of the confounding effect. Continuing with the same example, we can break down the cross-sectional data collection process into two steps: First, recruiting each participant for the experiment can be represented as sampling once from the between-person association distribution (green distribution from Figure 2C in Figure 3A), where each participant can be identified with a pair of alcohol use-negative affect trait values (black dots in Figure 3C). Second, obtaining cross-sectional measurements for the DVs can be represented as sampling once from each participant's within-person association distribution (orange distributions from Figure 1C layered on in Figure 3B). For simplicity, we assume in this paper that all individuals have the same within-person associations between DVs (allowing that the true score timecourses may still be different). This two-step sampling process to obtain cross-sectional measurements can also be equivalently represented as sampling once from a combined distribution that we refer to as the cross-sectional distribution (purple distribution; Figure 3C). It is important to note here that while the within- and between-person distributions are plotted, cross-sectional data only allow for estimates of the cross-sectional distribution (i.e. the between- and within-person distributions are not separable or independently estimated), hence the confounding effect.

We illustrate the confounding effect using this graphical representation to compare three hypothetical combinations of underlying between- and within-person associations: (1) positive within- and between-person correlations between the DVs (Figure 3C); (2) positive between-person correlation and zero within-person correlation (Figure 3D); (3) zero between-person correlation and positive within-person correlation (Figure 3E). Critically, the cross-sectional distributions (purple ellipses) are identical in all the three cases, illustrating that it is not possible to distinguish among the three cases with cross-sectional data.

Figure 3. Graphical representation of confounding effect in the bivariate case. (A) Recruiting participants for an experiment is represented as sampling from the between-person association distribution, with each participant having a pair of trait values. The four hypothetical participants shown are chosen at the edge of the distribution for illustrative purposes and do not reflect actual random sampling processes. (B) Taking cross-sectional measurements for the DVs from each individual is represented as sampling once from each individual's within-person distribution; example measurements are marked by 'x.' (C) Two-step sampling processes for cross-sectional experiments can be equivalently represented as sampling once from a cross-sectional distribution. (D,E) Different combinations of underlying within-person and between-person associations that result in cross-sectional distributions that are indistinguishable from that in panel C.

Mathematical description of confounding effect in bivariate case

Following the graphical presentation, we can consider a mathematical characterization of the confounding effect in the bivariate case with cross-sectional data on two DVs, X and Y (e.g. alcohol use and negative affect). We build on existing work in the psychometrics literature that explored the effects of error correlation in the CTT framework, which is different from LST in that there is no distinction between measurement error and state variations (Saccetti et al., 2020; Williams, 1974; Zimmerman & Williams, 1977). We extend these results to derive an equation to characterize the confounding effect and present the results using notations consistent with the definitions of this paper.

Let $\hat{X}$ and $\hat{Y}$, the observed values of the DVs, be composed of trait values, X_T and Y_T , state values, X_S and Y_S , and random measurement error, ϵ_X and ϵ_Y , such that:

$$\hat{X} = X_T + X_S + \epsilon_X \quad \hat{Y} = Y_T + Y_S + \epsilon_Y \quad (a)$$

We can define the reliabilities of the DVs as the ratio of trait variance to total variance:

$$R_X = \frac{\text{var}[X_T]}{\text{var}[X_T] + \text{var}[X_S] + \text{var}[\epsilon_X]} \quad R_Y = \frac{\text{var}[Y_T]}{\text{var}[Y_T] + \text{var}[Y_S] + \text{var}[\epsilon_Y]} \quad (b)$$

The reliability as defined here can also be interpreted graphically as the relative sizes of the within-person and between-person distributions in each DV dimension in Figure 3. We acknowledge that the reliability as defined here may be different from alternative definitions where the state variance is included in the numerator. Using these variables, we can express the confounding effect as:

$$\text{corr}(\hat{X}, \hat{Y}) = \text{corr}(X_T, Y_T) \cdot \sqrt{R_X R_Y} + \text{corr}(\hat{X}_S, \hat{Y}_S) \cdot \sqrt{(1 - R_X)(1 - R_Y)} \quad (c)$$

where $\text{corr}(\hat{X}, \hat{Y})$ is the observed cross-sectional correlation, $\text{corr}(X_T, Y_T)$ is the between-person association correlation (of trait values), and $\text{corr}(\hat{X}_S, \hat{Y}_S)$ is the within-person association correlation (of state values with measurement error) where $\hat{X}_S$ and $\hat{Y}_S$ represent state values with random measurement error:

$$\hat{X}_S = X_S + \epsilon_X \quad \hat{Y}_S = Y_S + \epsilon_Y \quad (d)$$

$\text{corr}(\hat{X}_S, \hat{Y}_S)$ can also be expressed as the attenuated correlation of the within-person true score correlation (relationship between Figure 1C and 1D):

$$corr(\hat{X}_S, \hat{Y}_S) = \frac{corr(X_S, Y_S)}{\sqrt{var[X_S + \epsilon_X]var[Y_S + \epsilon_Y]}} \quad (e)$$

These equations are consistent with the graphical representations of the confounding effect in the previous section (Figure 3). The observed correlations from a cross-sectional experiment reflect the weighted sums of between- and within-person correlations, with the weights being functions of the measurement reliabilities. As such, and critically, it is impossible to draw any conclusions about specific between- or within-person associations from just cross-sectional data because the observed correlation can arise from an infinite combination of within- and between-person associations and reliabilities. Without more information about the reliabilities and the true between or true within-person association, we cannot confidently make inferences from the cross-sectional association.

A number of points are worth highlighting. (1) If the within-person correlation is zero, Equation (c) reduces to the correlation attenuation equation (Spearman, 1904b), which also can be seen in Figure 3D. (2) The within-person association term in Equation (c) approaches zero if just one of the reliabilities approaches one (graphically represented by squeezing the orange distributions flat with zero width/height in either dimension in Figure 3); this means that the observed cross-sectionally measured correlation will be minimally affected by within-person associations if just one of the DVs involved is highly reliable (e.g. age). (3) As defined in Equations (b), both the within-person state and random measurement error variances can modulate the reliabilities and thereby affect the relative within- and between-person weightings in the confounding effect. (4) Both the mathematical and graphical representations (Figure 3) illustrate that the confounding effect does not depend on the number of participants sampled in an experiment. (5) While we have only discussed examples where the within- and between-person correlations are both positive (Figure 3), equation (c) shows that the within- and between-person correlations can theoretically have opposite signs (e.g. Simpson's paradox). (6) While measurement reliabilities are viewed as upper bound estimates for observed correlations from the perspective of Spearman's correlation attenuation, this is no longer the case when there are non-zero within-person associations; conversely, observed correlations also cannot be used as lower bound estimates for reliabilities. One consequence of this is that, contrary to the view that DVs in individuals differences research should have high reliability, a DV with low reliability may still be useful in the mixed-goal case as long as the low reliability is driven by a large state variance rather than measurement error. In line with other work that emphasizes the importance of measurement reliabilities in individual differences research (Draheim et al., 2021; Hedge et al., 2018), here we show that the magnitude of reliabilities is also critical in understanding the confounding effect in cross-sectional studies. Note that the reliability as defined in this context is better reflected by external reliability estimates (e.g. test-retest) rather than internal reliability estimates (e.g. split-half) because of the impact of state variations.

Multivariate simulations

In the preceding sections our discussion of the confounding effect concentrated on the bivariate case for simplicity. In practice, many individual differences studies involve multiple DVs. Given that many commonly used analytic approaches (e.g., ANOVA, Regression, SEM) rely on covariance matrices and that the elements of covariance matrices are all bivariate covariances, the confounding effect will materialize in all cross-sectional covariance-based multivariate analyses. In this section, we simulate cross-sectional multivariate experiments to explore the impact of the confounding effect on the interpretations of results. It is beyond the scope of this paper to cover all multivariate analytical approaches. Instead, we focus on examining the observed (sample) correlation matrices, which is the basis for many analytical methods.

For concreteness, we frame the simulated experiments around an example research question: “What is the structure of individual differences in executive function?” (Friedman & Miyake, 2017; Miyake et al., 2000). Each experiment consists of simulated data for six DVs from six distinct tasks that putatively index executive function (Stroop task, go/no-go task, etc) from 100,000 individuals; a large number of individuals were simulated because sampling variability is not a focus of this paper. As in prior sections, we assume identical within-person associations for all individuals; thus, each simulated experiment was parameterized by one between-person correlation matrix and one within-person correlation matrix. Reliabilities were fixed at 0.5 for all DVs and random measurement error was, for simplicity, fixed at zero. Data were simulated using multivariate normal distributions.

Data from three experiments were simulated, with the underlying covariance matrices and resulting observed cross-sectional covariance results for each experiment shown in separate rows in Figure 4. The three experiments were simulated using different combinations of underlying between-person correlation matrix (left column) and within-person correlation matrix (middle column): (1; top row) three-factor within-person and unitary between-person structures, (2; middle row) unitary within-person and three-factor between-person structures, and (3; bottom row) dual-factor within-person and between-person structures. In all three experiments, the observed correlation matrix had a three-factor structure (right column), confirming the confounding effect. Note that in the third simulation, a three-factor structure was obtained even when neither the within- nor the between-person structures had a three-factor structure. As such the confounding in the multivariate case extends beyond anything seen in the simple bivariate case. Further, while we fixed the measurement reliability to 0.5 here, differences in reliabilities in real experiments will undoubtedly also impact the observed correlation structure and should be carefully considered. These simulations show that, even though a three-factor structure for executive function is observed cross-sectionally, experimenters cannot (and should not) obtain any specific conclusions about the underlying within- or between-person structures.

Figure 4. Simulated multivariate cross-sectional experiments. Three simulated cross-sectional individual differences experiments where six DVs are measured. Each row represents a separate simulated experiment. Each experiment is parameterized by a between-person correlation matrix (left column) and a within-person correlation matrix (middle column). The observed cross-sectional correlation matrix for each is shown on the right column.

Discussion

In this paper, we illustrate that observed associations from cross-sectional individual differences experiments cannot separate between- and within-person associations and highlight the danger that this confounding effect poses to data interpretability. Moreover, a lack of clarity about the goals of studies (between-goal vs mixed-goal) often makes it difficult to evaluate whether a cross-sectional design is appropriate. If the goal of a study is to isolate between-person associations, then a cross-sectional study would be insufficient since the results likely would be confounded by within-person associations; rather, a repeated-measures study is required to isolate the associations of interest. In contrast, when the goal of a study is to describe data without separating between- and within-person associations, then a cross-sectional study would be sufficient. In this discussion, we explore some relevant issues and offer some recommendations for better design, reporting, and reading of individual differences research.

Confounding effect in real experiments

Pushing beyond the conceptual and mathematical basis of the confounding effect illustrated in this paper, a reader may reasonably seek further clarity about the implications for real experiments. One may wonder how much within-person associations actually contribute to observed cross-sectional results. In addition, one might ask whether it would be acceptable to use cross-sectional data to make inferences about between-person associations if the magnitude of within-person associations is small or if the DVs used have high measurement reliability (i.e., are very stable as indicated by test-retest reliability). These questions are just as important for designing new studies as they are for interpreting existing studies.

Acknowledging that the magnitude of within-person associations will depend on the specific study, we consider this issue from a broader standpoint. Empirical evidence suggests that the confounding effect is not negligible and that observed cross-sectional correlation structures are unlikely to be due to purely between-person associations. The fact that entire research fields are dedicated to repeated-measures studies indicates that covariance structure commonly exists on both between- and within-person levels (Curran & Bauer, 2011; Fisher et al., 2018; Ram & Gerstorf, 2009; Schmiedek et al., 2020; West, 2018). Furthermore, recent data demonstrate that many commonly used DVs display a wide range of reliabilities, which can directly modulate the confounding effects (Enkavi et al., 2019). These observations suggest that the confounding effect may often play a nontrivial role in the results obtained from cross-sectional individual differences studies.

More importantly, we believe that the choice of experimental design should be determined based on the conceptual goals of each experiment and that using empirical estimates of the within-person associations as justification for the use of cross-sectional designs should be avoided for several reasons. (1) From a purely theoretical perspective, a cross-sectional design is only valid if the goals of a study do not seek to differentiate within- and between-person associations, regardless of the magnitude of the within-person association. (2) From a practical standpoint, it is difficult to estimate the within-person association unless a repeated-measures experiment is conducted; even then, the usefulness of such data may be limited due to factors such as sampling and experimental variability (Enkavi et al., 2019). (3) Even minimal amounts of within-person variance and covariance will undermine the use of cross-sectional results to make inferences about between-person associations; this may be particularly true for conclusions made based on statistical methods that involve thresholds, which may be sensitive to even small contributions from within-person associations (e.g. SEM model comparisons using p-values and some choice of alpha).

Within-person association stationarity assumption

In illustrating the confounding effect, we assumed stationarity in the within-person associations for simplicity and only considered how random measurement error impacts the measurement process of a DV. In reality, a variety of real-world factors will influence the

measurements. While it is outside the scope of this paper to address this topic fully, here we briefly discuss an example of how one source of influence, experimental parameters, may impact the observed cross-sectional correlations.

The experimental parameters discussed here encompasses all aspects of experimental design such as length of breaks between separate cognitive tasks and brightness of the laptop display. Critically, choices of experimental parameters may causally affect the measurement of DVs, and these influences may have important and unintentional consequences for the results and interpretations of a cross-sectional experiment. For example, cross-sectional experiments that separate a large battery of tasks into multiple sessions/days may introduce unintended within-person associations into the data by artificially causing DVs measured in the same session to be more correlated than DVs measured in different sessions. Consider the first multivariate simulation experiment (top row Figure 4), if each pair of correlated DVs were measured in a separate day, the within-person structure could be entirely caused by this session structure and have nothing to do with the natural dynamics of the DVs. Even within the same session, effects such as fatigue may introduce within-person associations to neighboring tasks such that the measurement of a DV will have a causal effect on the true score dynamics of other DV measurements that occur later. These influences from real-world factors should be carefully considered when designing and interpreting cross-sectional individual differences research.

Cross-sectional vs repeated-measure? Between-goal vs mixed-goal?

In this paper we make distinctions at both the levels of study goals (between vs mixed) and design (cross-sectional vs repeated-measures) mostly from a purely measurement standpoint, with a focus on illustrating that cross-sectional approaches are only sufficient for mixed-goal studies and not between-goal studies due to the confounding effect. Here, we go beyond the discussion of sufficiency and briefly consider the practical and applied aspects of these choices.

What value is there in studies with the mixed-goal compared to studies that seek to understand individual differences by separating within- and between-person associations? One possibility is to view these differing goals as a decision between explanation vs. prediction (Yarkoni & Westfall, 2017), with the critical distinction here involving experimental designs instead of analytical approaches. A cross-sectional design may be sufficient when building a sample-level prediction model, while a repeated-measures design is required to decompose the effects to explain the between- and within-person components. Even so, some may wonder whether it is better to always use a repeated-measures design even in the prediction (or mixed-) goal case, given that repeated-measures data can be used to obtain the same results that would be obtained with cross-sectional data while also supporting a broader range of analyses. While this may be true, there are many practical considerations that need to be balanced when choosing between repeated-measures vs cross-sectional designs (e.g. financial cost, compatibility with research question, participant recruiting challenges, etc) such that it may not always be most optimal to use repeated-measures designs. We emphasize that the purpose of this paper is not to rank-order the two experimental design types, nor is it to criticize cross-sectional approaches. We

believe both approaches are useful as long as there is clarity about how the research goals and inferences match the design used.

Recommendations for better practices

We conclude by making some suggestions for better practices in designing, reporting, and reading individual differences research. For each suggestion, we also include an example of the types of phrases that should be included when reporting and discussing results:

- (1) The goals of a study and the target associations of interest (between vs mixed) should guide the choice of experimental design. A potentially helpful thought experiment may be to ask whether the results of the study would be useful if all the participants are clones and/or the observed associations are due entirely to within-person associations. Furthermore, explicit statements about the goals of a study and the reasoning behind the experimental design should be made in reports of the study to avoid any confusion in interpretation of the results. While the term “individual differences” is commonly used, it encompasses two conceptually distinct goals and lacks precision. Thus, it may be beneficial to always provide further clarification when the term “individual differences” is used unless one intends to use it as an umbrella term to refer to both goals (e.g. avoid using article titles such as “Individual differences in alcohol use and negative affect”). Example phrase in introduction: “The goal of this study is to understand the relationships between alcohol use and negative affect without dissociating within- and between-person associations so a cross-sectional design was chosen.”
- (2) Cross-sectional designs cannot serve as substitutes for repeated-measures designs even in cases where minimal within-person associations are suspected. If a cross-sectional design is used, the confounding effect should be acknowledged and its implications discussed. If a repeated-measures design is used, the data should be analyzed using appropriate analytical methods (such as multilevel modeling) (Grimm et al., 2016; Peugh, 2010). Example phrase in discussion: “While we have observed a negative linear relationship between negative affect and alcohol use from this cross-sectional data, we cannot conclude whether this is due to associations at the within-person or between-person levels.”
- (3) Cross-sectional experiments should consider and report how both between- and within-person associations are relevant to the research questions of interest, even when there is no need (based on study goals) to separate the two types of associations. While this may seem counterintuitive, this is important because the two types of associations are mechanistically distinct and will both be present in the results. Critically, given that experiments are almost always theoretically motivated and linked to specific mechanisms that are being probed, there is a need, in both the experimental planning and result-reporting stages, to account for all the possible processes that could contribute to observed results. Let us consider the executive function simulations as an example: even in a hypothetical cross-sectional experiment where we do not intend to separate between-

and within-person associations, we would still consider how an observed three-factor structure relates to existing executive attention theories and findings at both the between- and within-person levels. Example phrase in preregistration: “We hypothesize that combinations of two possible correlation structures may be observed in our cross-sectional experiment examining executive function structure: a unitary within-person structure caused by within-person state fluctuations in motivation and a three-factor between-person structure caused by long-term trait level associations between the DVs.”

With increasing interest to better understand individual differences methodology (Bastian et al., 2020; Dubois & Adolphs, 2016; Enkavi et al., 2019; Hedge et al., 2018), this paper connects with the common goal from these efforts to improve scientific clarity and transparency.

Code availability

The code for generating the figures and multivariate simulations are available at [<https://doi.org/10.5281/zenodo.5854792>].

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