Are Items more than Indicators? An examination of psychometric homogeneity, item-specific effects, and consequences for structural equation models

Kenneth McClure*

University of Notre Dame, Department of Psychology, Notre Dame, IN, USA

kmccclur5@nd.edu

Ross Jacobucci

University of Notre Dame, Department of Psychology, Notre Dame, IN, USA

rjacobuc@nd.edu

Brooke Ammerman

University of Notre Dame, Department of Psychology, Notre Dame, IN, USA

bammerm1@nd.edu

*Corresponding Author: kmccclur5@nd.edu

Abstract

Concerns of measurement error often motivate researchers to aggregate item information, using simple heuristics (e.g., sum scores) or latent variable methods, to mitigate unwanted effects such as parameter bias and attenuation. These approaches are often invoked without acknowledging that many scales in practice likely fail to possess the necessary properties for these models to be sufficient (i.e., positive conditional association and vanishing conditional dependence). We argue that measures which are not psychometrically homogeneous likely contain item specific effects particularly when examined in conjunction with external variables. We demonstrate this using a clinical empirical example assessing risk factors for suicidal ideation and show that measures constructed in alignment with principles of psychometric homogeneity are most appropriately modeled at the scale (or subscale) level while other measures should be considered at the item level. As a result, latent variable applications to such instruments are susceptible to interpretational confounding. The effects of interpretational confounding on R^2 , root mean square error, and model parameters are evaluated in a small simulation study. We conclude that item specific effects are not uncommon in practice and impact both explanatory and predictive research. Our findings suggest that classical approaches to addressing measurement error are insufficient to fully capture the breadth of instruments implemented in practice. Careful consideration of both the scale construction process and roles of scale items in the broader psychological theory are necessary prior to the application of traditional measurement methods.

Keywords: Measurement, Interpretational Confounding, Structural Equation Modeling, Predictive Validity

Translational Abstract

Psychological measurement primarily consists of multi-item scales and, while met with opposition from methodologists, is frequently scored as a simple sum of the items. Alternatively, structural equation modeling is used to mitigate the impacts of measurement error. Both approaches reflexively defer to composite scores without considering the role of the individual items on the scale. Traditional frameworks emphasize reliable measurement of the general underlying psychological construct and rely on a specific conceptualization of these phenomena. In this paper, we argue that unreliability is not inherently problematic as it allows the possibility for items to provide unique information when considered in a predictive context. We demonstrate this in a clinical example predicting suicidal ideation from multiple risk factors and show that scales not explicitly developed to preserve classic conceptualizations of measurement may be more appropriately examined as a collection of items instead of a composite. Further, through a small simulation, coercing these predictors into overly restrictive measurement error models likely yields undesirable consequences from both explanatory and predictive perspectives. We conclude that the conceptualization and construction of the measures commonly used in psychology have downstream implications when used to explain or predict outcomes.

Introduction

In psychology, theoretical constructs are often quantified via multi-item scales using observed scores as proxies for concepts which are not directly measurable (e.g., Jöreskog & Sorbom, 1979). Depression, for example, may be assessed via a series of statements thought to be characteristic of depressed mood or anhedonia. The resulting scores are then used to inform inferences concerning the broader construct. In other words, these items tap observable behaviors hypothesized to reflect a theoretical domain of depression which is not itself accessible. This understanding of construct-item relationships represents a reflective theory of measurement in which items serve as imperfect indicators of a latent trait (e.g., Bollen & Lennox, 1991; Borsboom, Mellenbergh, & Heerden, 2003; Edwards & Bagozzi, 2000) and is common to both classical test theory and domain sampling approaches (Lord, 1955; Lord & Novick, 1968). Further, a large portion of the rationale for the use of scale/sum scores, and more broadly structural equation modeling (SEM), is the ever-present influence of measurement error. While the reduction or elimination of measurement error is an oft-cited motivation behind these strategies when conducting inference, it is also noted as a motivation for predictive goals particularly when nonlinear relationships are possible (Carrol et al., 2006; Jacobucci & Grimm, 2020) as well as assessments of incremental validity (Hayes, 2021; Westfall & Yarkoni, 2016).

Item scores, on the other hand, often represent overly narrow domains which inadequately cover the breadth of the target construct and limit generalization (Crocker & Algina, 2008). Despite these apparent drawbacks, two recent modeling advances, network models (e.g., Borsboom & Cramer, 2013) and machine learning, challenge the current zeitgeist of using summed/scale scores through the focus on using item level data. Network models examine the partial correlation structure between variables (e.g., item scores) and emphasize the

unique pairwise relations intrinsic to the network. This provides an explanatory framework for assessing the connectedness between granular predictors and potentially inform interventions (McNally, 2016). Machine learning, on the other hand, is a broad collection of methods which emphasizes predictive accuracy involving algorithmic procedures that often combine small effects in a non-linear data driven fashion (e.g., Breiman, 2001; Friedman, 2001). Both of these approaches highlight the role of item scores as potential variables of interest in psychological research. Further, predictive aims are becoming increasingly common in psychology research (Yarkoni & Westfall, 2017) which is more in line with the use of items as opposed to scales (McDonald, 1996).

While the elimination of measurement error through theoretically based dimension reduction seems like an effective strategy across domains, this seems to stand in opposition to the more granular use of items in specific domains. On one hand, item scores are more susceptible to measurement error than composite scale scores (Brown, 1910; Crocker & Algina, 2008; Spearman, 1910) increasing the risk of parameter bias and reduced power; on the other, however, scale scores (e.g., sum scores, factor scores) segregate construct and item effects combining the latter with measurement error. This is a strength from a purely explanatory perspective, as it facilitates the examination of construct relationships, but undesirable from a predictive perspective as all non-error variance may be informative when external variables enter the mix. In other words, when items convey information relevant to an outcome beyond that of the latent construct both predictive and explanatory performance of the model may be compromised.

As detailed above, there are conflicting reports on whether and when scale/sum scores and item scores should be used. While the majority of literature focuses on the benefits of using scale scores; namely, increased reliability, parsimonious interpretation, and alignment with

theoretical formulations, there are still a number of scenarios in which the use of item scores is preferential. Further, items and scales are not all constructed from a unified conceptualization of their target construct. Many clinical scales contain items indicative of a constitutive approach to measurement (Kendler, 2017a) and are likely to contain item specific effects beyond that of the overall scale (e.g., the Patient Health Questionnaire (PHQ-9); Kroenke et al., 2001); others acknowledge an indexical approach (Kendler, 2017a) and integrate principals from classical test theory (CTT) and the behavior domain perspective (e.g., Lord, 1955; Lord & Novick, 1968) to iteratively construct the measure. We demonstrate below that coercing items from the former into restrictive latent variable models consistent with the latter yields deleterious effects for parameter estimates and accuracy metrics.

While the use of sum/scale scores has benefits in specific circumstances, they rely on a number of assumptions which often lay at odds conceptually with the field in practice especially in areas of clinical research. In fact, the use of sum scores corresponds to imposing a heavily constrained factor model that can rarely be justified in practice (McNeish & Wolf, 2020). To demonstrate this, we examine the role of items under traditional conceptualizations of measurement and contrast this with the scale development in practice. We follow this with an empirical example predicting suicidal ideation from multiple depression scales to highlight the importance of item conceptualization in prediction. Finally, we conduct a simulation study to demonstrate that SEM is ill-equipped to address measurement error in prediction settings in its current state.

Predictive Validity and Measurement Error

Measurement error is well known to permeate traditional regression models and bias parameter estimates. This bias is most commonly discussed in terms of attenuation where the

expected value of estimated regression coefficients is nearer to zero in the presence of measurement error in the predictors than would be in its absence (Aiken & West, 1991; Bollen, 1989; Carroll et al., 2006; Cohen, Cohen, Aiken, & West, 2003; Fuller, 1987). In the single predictor case, this attenuation is reflected in the validity coefficient shown below. A scale's ability to predict a criterion assessed at a later time point, refers to the predictive validity of that scale. The scope of predictive validity in CTT, however, is generally discussed in terms of a correlation coefficient between predictor and criterion scores (ρ_{xy}). Under traditional CTT assumptions it can be shown that score reliabilities limit this validity coefficient. Specifically, for a criterion Y the validity coefficient can be calculated as:

$$\rho_{XY} = \rho_{T_X T_Y} \sqrt{\rho_{XX'}} \sqrt{\rho_{YY'}} \quad (1)$$

where $\rho_{XX'}$ and $\rho_{YY'}$ are the reliabilities of predictor and criterion scores respectively. From this, we can see that any time scores are not perfectly reliable, the predictive validity of observed scores is less than the true predictive validity (i.e., since reliability is bounded above by 1). It appears that by maximizing score reliability, predictive validity will also be maximized; however, this is not the case (Tucker, 1946; Loevinger, 1954). Part of the confusion in understanding the influence of reliability on prediction is that equations are often stated in terms of correlations, not variances/covariances. To see how factors beyond reliability influence prediction, we restate Equation 1 as:

$$\frac{\sigma_{XY}^2}{\sigma_X^2 \sigma_Y^2} = \frac{\sigma_{T_X T_Y}^2}{\sigma_{T_X}^2 \sigma_{T_Y}^2} \frac{\sigma_{T_X}^2}{\sigma_X^2} \frac{\sigma_{T_Y}^2}{\sigma_Y^2} \quad (2)$$

From this formulation it is clear that the variability of the observed scores (σ_X^2) plays a crucial role in any subsequent predictive task. When X corresponds to a single measurement of the target construct (i.e., a single-item scale), the intuitive attenuation present in equation 1 holds and

prediction is maximized when reliability is perfect; however, when multiple items are used the relationship between reliability and predictive validity becomes more complicated.

Historically dubbed “The Attenuation Paradox” (Loevinger, 1954), maximizing reliability does not translate to optimizing validity. Tucker (1946) provides the intuition for this paradox as follows: consider a test of items that are equivalent. Then, these items have equal reliabilities, are equally difficult and assess the same construct. If the reliability is increased to one (i.e., all observed score variance is true score variance), then all interitem correlations also increase to one. Thus, the variance of the true score is equal to the variance of each item. Consequently, a person endorsing any of these items must also endorse all other test items and failure to endorse an item guarantees the remainder of items are not endorsed (i.e., respondents receive a perfect score or a zero score). This results in a rank-order of scale scores equivalent to the rank-order of responses on any item. On the other hand, a scale with imperfect reliability produces scale scores with increased variability (i.e., there are now possible scores between perfect and zero) which translates to a more informative rank-ordering on the latent trait.

While perfect reliability is nearly impossible to reach in practice, a potential inverse relationship between reliability and predictive validity can be seen. Tucker (1946) demonstrates that when items are equivalent, the highest levels of predictive validity occur when item scores possess less than perfect reliability. The predictive validity of a scale with the underlying ability as the criterion is a function of item discrimination, item difficulty, and scale length. For items of median difficulty (i.e., the probability of endorsement with average ability is 0.50), Tucker (1946) shows that the predictive validity of a single item scale is maximized when item discrimination is zero (i.e., the item characteristic curve is a vertical line) and the inter-item correlation is one. When scales contain multiple items, however, predictive validity is

maximized with item correlations of ~ 0.5 for a ten-item scale and ~ 0.2 for a one hundred item scale (Tucker, 1946; see the top panels of Figure 1). Additionally, using the underlying relationship between the factor analytic model and the normal ogive item response model (Bock, Gibbons, & Muraki, 1988; Embretson & Reise, 2013) we can further understand these results in terms of factor loadings (see the bottom panel of Figure 1). For multi-item scales in general, improving reliability increases predictive validity up to a point; beyond this, however, increasing reliability is detrimental for prediction. This process represents a tension between maximizing scale variance while contributing unique true score variance from each item. Of course, these results rely on strict psychometric assumptions unlikely to be met in practice (e.g., McNeish, 2018); however, they demonstrate that maximizing reliability will generally not maximize the predictive validity of a scale even under ideal conditions and increasing scale length beyond a threshold may adversely impact prediction.

Latent Variable Models and Prediction:

Intuitively, the addition of random error into the mix obscures signal and attenuates these coefficients; however, when multiple predictors are considered simultaneously, as often is the case in psychology, the magnitude and direction of regression coefficient estimates are more complex and are impacted by the covariance structure of the true score variables (i.e., the constructs or behavior domains; Aiken & West, 1991; Bollen, 1989; Carroll et al., 2006; Cohen, Cohen, Aiken, & West, 2003; Fuller, 1987). When true score variables are correlated, regression coefficient estimates systematically overestimate or underestimate the population value. To mitigate this bias, current best practice recommends the use of SEM which directly incorporate measurement models into the analysis and account for potential latent true score correlations if specified by the researcher.

While measurement error in outcomes can also be problematic, our focus in this paper is on measurement error in predictors, as this is the most commonly considered research scenario. In psychological research, the use of latent variables is consistently recommended to overcome the effect of measurement error in a subset or all observed variables. Implicit in this recommendation is the goal of conducting explanatory research, as opposed to that of prediction. This can be attributed to explanation being the predominant goal of psychological research (i.e., Yarkoni & Westfall, 2017), but is also shaped by the most commonly used statistical framework for specifying latent variables: SEM.

To demonstrate why SEM is an explanatory framework and does not readily lend itself to the incorporation of predictive aims, we build upon our example in using aspects of depression to predict suicidal ideation (SI). SEM can construct perfectly measured latent variables from items on a depression scale and impose a regression path from the latent variable to the outcome (SI) to overcome measurement error in the depression items. This form of modeling will result in an unbiased estimate of $\text{Dep} \rightarrow \text{SI}$, however, there are a number of complications. The first is depicted in Figure 2, where the regression path from a latent variable to an observed outcome cannot be distinguished from a factor loading with the outcome as an indicator of the latent variable (Burt, 1976; Levy, 2017). A practical consequence of this is that the formation of the latent variable (depression) is defined by its relationship with the outcome (SI), which can be referred to as interpretational confounding (Burt, 1976), thus altering the meaning and interpretation of the latent variable relative to its construction with only its indicators (depression items).

This problem of interpretational confounding is attributed to structural misspecification, namely that bias to the parameter coefficients in Figure 2 only occurs if there is no unique

prediction of SI from the depression indicators not through the latent variable (Bollen, 2007).

This manifests as non-zero covariance between the indicator residuals and outcome (see Figure 3), and as we argue below, is very likely to occur in practice. In the presence of residual correlation between indicators and outcomes, both overestimates of structural parameters and biased factor loadings have been observed (Cole et al., 2007; Levy, 2017); however, the pattern of bias is dependent on the nature of the misspecification. Interpretational confounding is typically exacerbated by poor measurement quality as well as strong relationship between the latent predictors and the outcomes (i.e., large structural coefficients; Burt, 1976; Levy, 2017). These misspecifications are likely to occur and are difficult to identify by fit indices¹ or the explained variance in models with and without the specification of a latent variable².

Correctly specified models alleviate concerns about interpretational confounding; however, knowledge of the true model is seldom accessible if it exists at all. To address this methodologically, Levy (2017) proposes a Bayesian approach which severs the outcomes from the measurement models during Gibbs sampling. This ensures uncertainty in the measurement model is carried through the analysis while distinguishing external variables from indicators with respect to the latent variable. Additional approaches which do not preserve uncertainty involve stage wise estimation of the measurement and structural components (Yuan & Chan, 2002; Skrondal & Lake, 2001; Croon, 2002; Skrondal & Kuha, 2012) though neglecting parameter uncertainty is known to be problematic in many applications. These methods consist primarily of various factor score regression techniques and may be a feasible alternative to simultaneous SEM (Devlieger & Rosseel, 2017; Devlieger, Mayer, & Rosseel, 2016; Hayes & Usami, 2020).

An issue that is intertwined with the problem of interpretational confounding is the idea of prediction using latent variables. Explanation and prediction are not competing goals of

scientific research, but each can be used as supplements to bolster the support of the other (Shmueli, 2010). Given that it is difficult to justify the use of SEM in non-explanatory research, since it requires strong justifications that the proxies are valid representations of the latent variables (Rigdon, 2016), the question becomes: what role does the evaluation of prediction have alongside explanation in SEM? This is difficult to answer, as the evaluation of prediction in SEM faces several hurdles. Firstly, while fit metrics can be evaluated using holdout samples (Browne, 2000), either through a resampling method such as k-fold cross-validation or a validation sample, it is not possible to use these same methods to evaluate individual predictions in most SEMs. For instance, it is not possible to “test” the model displayed in Figure 2 or 3 on a holdout sample to generate individual predictions for SI and calculate metrics such as Accuracy or area under the receiver operating characteristic curve (AUC). That is, true factor score values for depression are unknown so their discrepancy with predicted values cannot be evaluated. Similarly, accuracy metrics (e.g., Root Mean Square Error; RMSE) in fully reflective models, those in which all manifest indicators are hypothesized to be produced by an underlying latent variable (e.g., Figure 2), also cannot be evaluated with holdout data (see Shmueli et al., 2016). Secondly, it is important to remember that SEM is a multivariate method that entails modeling a covariance matrix containing all observed variables in the model. While the left pane of Figure 2 seemingly depicts a prediction model that is concerned with maximizing the explained variance of SI, the model in fact gives equal weight to modeling each of the depression indicators and SI. The challenge with prediction in these frameworks is balancing the increasingly prevalent goals of prediction with the explanatory emphasis intrinsic in latent variable measurement models.

Measurement: Items, Scales, and Infinite Domains

Underlying these approaches is a behavior domain theory (BDT; Lord 1955; Lord & Novick, 1968; McDonald, 2003) in which theoretical constructs are conceptualized as behavior domains whose measurement amounts to sampling from an infinite collection of construct-relevant behaviors. In practice, the observable behaviors are often probed by the multiple items on scales to facilitate inference about the domain or construct of interest. As with traditional sampling, the interest resides primarily in population level inference; however, clear specification of behavior domains poses a unique challenge in that domain identification is often an empirical question as well (e.g., Kendler 2020; Kendler, 2017b). Precise domain definitions are scarce in psychology; however, tests of optimal ability (Cronbach, 1984) may afford relatively concrete operationalizations of the intended domains. For example, to an ability of integer addition a test-designer may use addition problems of the form “ $x + y$ ” for integers x and y which has the potential to produce infinitely many samples from the domain of interest.

Unfortunately, formulaic approaches to item generation are the exception rather than the rule. In the absence of a generative schematic, item writing and scale construction relies on an iterative process of conceptualization, measurement, and evaluation (e.g., Clark & Watson, 1995, 2019; Dawis, 1987). Under the behavior domain approach, proper identification of the domain is of primary interest followed by adequate sampling of the domain (Markus & Borsboom, 2013). In other words, the goal is to find indicators which agree with the conceptualization of the domain both theoretically and psychometrically. Item characteristic similarity often guides the development of tests and generation of items (Markus & Borsboom, 2013; McDonald, 2003) since the behavior domain is not directly accessible (i.e., it is latent) for researchers to randomly sample from. As a result, the primary source of evidence in generating items to sample the domain is derived from existing items and not the domain itself (Markus & Borsboom, 2013).

This results in a measurement framework in which the items are secondary to the construct in theory but drive the conceptualization of the construct in practice.

A natural consequence of constructing scales based on items “of the same kind” (McDonald, 2003) is item redundancy. Unsurprisingly, retaining items with similar psychometric characteristics (e.g., high loadings on a common latent factor) bolsters the internal consistency of the scale scores; however, doing so directly limits the potential unique variability in item responses. In other words, the more items reflect the latent variable the less item-specific variance, both systematic and error, they contribute. From a purely explanatory measurement perspective, this is not problematic since non-domain variance reflects error variance under BDT; however, when scores are intended for use in a larger framework or to inform predictions (e.g., Kane, 2013), highly homogenous items may fare worse than less similar items (Loevinger, 1954; Tucker, 1946). To explore the potential of systematic variance beyond the domain, we first consider some fundamental assumptions in BDT.

Psychometric homogeneity (McDonald, 1999) or, equivalently, conditional association and vanishing conditional dependence (Ellis & Junker, 1997) are fundamental assumptions in the domain sampling approach to psychological measurement. Together, these properties ensure that instruments measure a single underlying trait and are further necessary to support the prevalent common-cause interpretation (Bollen & Lennox, 1991) afforded latent variables (Markus, & Borsboom, 2013). Conditional association (CA; Holland & Rosenbaum, 1986) requires that subtest scores remain positively correlated after controlling for a third subtest (Markus & Borsboom). Formally, for a finite subsequence (X) of the latent domain, partitions of the items (Y and Z) remain correlated using arbitrary scoring rules f , g , and h :

$$\text{Cov}(f(\mathbf{Y}), g(\mathbf{Y})|h(\mathbf{Z})) \geq 0 \quad (\text{CA})$$

Vanishing conditional dependence (VCD), on the other hand, requires observed scores to be unrelated when conditioning the tail of the domain (i.e., the remaining infinite sequence of potential domain behaviors):

$$\lim_{m \rightarrow \infty} \text{Cov}(f(\mathbf{Y}), g(\mathbf{Z})|X_{J+1}, \dots, X_{J+m}) = 0 \quad (\text{VCD})$$

This suggests that all covariance between item score is attributable to the domain and vanishes when the domain score is considered. Ellis and Junker (1997) refer to this as an asymptotically empirical property as it cannot be verified in observed data; furthermore, this is directly related to the conditional independence assumption frequently made in item-response models (Markus & Borsboom, 2013). Together, CA and VCD suggest that behavior domain items are exchangeable and heavily redundant (Ellis & Junker, 1997).

To elucidate these assumptions and potential consequences, consider depression as the domain of interest and the PHQ-9 as our example for the finite subsequence sampled from the domain (i.e., $\mathbf{X}$). For simplicity, f , g , and h will all be taken as the simple sum score. $\mathbf{Y}$ and $\mathbf{Z}$ can be any two sets of items from the scale such that combining them returns the full scale. In fact, both CA and VCD apply to all possible splittings of $\mathbf{X}$. CA then requires that scores from the $\mathbf{Y}$ items remain positively correlated with themselves after partialing out the effects of the $\mathbf{Z}$ items. This seems reasonable given the content heterogeneity in the items (i.e., regardless of how we split the items set $\mathbf{Y}$ is not fully explained by set $\mathbf{Z}$). VCD, however, requires that the covariance of item scores $\mathbf{Y}$ and $\mathbf{Z}$ approach zero when controlling for the domain of depression. Suppose $\mathbf{Y}$ is the item targeting suicidal ideation, $\mathbf{Z}$ is the remaining items, and g is a scoring algorithm which assigns unit weighting to item two (“Feeling down, depressed, or hopeless”) and zero to other items. VCD requires scores from SI and hopelessness items be uncorrelated after

controlling for depression. Contemporary theories of suicide (e.g., Klonsky et al., 2015; van Orden et al., 2010), however, challenge this psychometric implication. This disconnect between conceptualizations of constructs and how they are measured serves only to further obfuscate psychological theory (Blinkhorn, 1997).

Item Specific Effects

BDT holds that items are merely samples from domains containing infinitely many relevant behaviors; furthermore, items, should be fundamentally exchangeable and heavily redundant with respect to the domain. Many scales, however, do not preserve this psychometric homogeneity. Items may possess reliable specific variance attributable to the item itself in addition to the random noise induced via the measurement process. When considered in the context of an outcome variable, an item specific effect may be directly relevant in predicting this criterion. In other words, after accounting for the latent factor (e.g., depression), systematic variability in an item (e.g., having little energy) may explain additional outcome (a suicide attempt) variability. This may be attributable to this item's relationship with additional latent processes (e.g., impulsivity); however, given that the specified model may omit this construct, we cannot evaluate it empirically precluding its detection.

A more complete path diagram of the Figure 2 is shown in Figure 3. Here we can see that there are J possible paths between the depression item specific effects and the outcome, SI, whose strength is denoted by β_{s_j} . Note that when $\beta_{s_j} = 0$ for all items, the path diagram reduces to Figure 2. When $\beta_{s_j} \neq 0$, for some items and the proposed model implies the path diagram of Figure 3 interpretational confounding occurs. This can be intuitively seen using the graphical representation in the right pane of Figure 2 as the corresponding coefficients relating the item in

question and the outcome to the latent factor (i.e. λ_j and λ_{dep}) will change to accommodate $\beta_{s_j} \neq 0$.

As models increase in complexity, the impact of model misspecification follows suit and biased parameters become difficult to detect (e.g., Cole, et al., 2007; Levy, 2017; Rhemtulla, van Bork, & Borsboom, 2019).

Similarly, we can show that under a measurement model allowing for item effects, regression coefficients regressing an outcome onto manifest items contains both the relationship between the latent factor and the item specific effect. Specifically, given

$$x_j = \lambda_j T + s_j + \varepsilon_j \quad (3)$$

$$\lambda_j = \frac{\sigma_T^2}{\sigma_{x_j}^2} \quad (4)$$

under the assumptions that s_j and T are independent and ε_j are random errors which are independent of s_j , T , and Y

$$\beta_{x_j} = \frac{\text{cov}(x_j, Y)}{\text{var}(x_j)} = \frac{\text{cov}(\lambda_j T + s_j + \varepsilon_j, Y)}{\text{var}(x_j)} \quad (5)$$

$$\beta_{x_j} = \frac{\lambda_j \text{cov}(T, Y)}{\text{var}(x_j)} + \frac{\text{cov}(s_j, Y)}{\text{var}(x_j)} \quad (6)$$

$$\beta_{x_j} = \frac{\text{var}(T)}{\text{var}(x_j)} \frac{\text{cov}(T, Y)}{\text{var}(x_j)} + \frac{\text{cov}(s_j, Y)}{\text{var}(x_j)} \quad (7)$$

$$\beta_{x_j} = \frac{\text{var}(T)}{\text{var}(T)} \frac{\text{var}(T)}{\text{var}(x_j)} \frac{\text{cov}(T, Y)}{\text{var}(x_j)} + \frac{\text{var}(s_j)}{\text{var}(s_j)} \frac{\text{cov}(s_j, Y)}{\text{var}(x_j)} \quad (8)$$

$$\beta_{x_j} = \frac{\text{var}(T)}{\text{var}(T)} \frac{\text{var}(T)}{\text{var}(x_j)} \frac{\text{cov}(T, Y)}{\text{var}(x_j)} + \frac{\text{var}(s_j)}{\text{var}(s_j)} \frac{\text{cov}(s_j, Y)}{\text{var}(x_j)} \quad (9)$$

$$\beta_{x_j} = \lambda_j^2 \beta_T + \frac{\sigma_{s_j}^2}{\sigma_{x_j}^2} \beta_{s_j} \quad (10)$$

Given that only the manifest items are typically observed, obtaining specific estimates for all parameters is typically unfeasible given model identifiability; however, maximizing predictive validity of an item amounts to jointly optimizing latent factor and item specific reliabilities. Note that if we wish to include the reliability of s_j in the Equation 3, the regression coefficient for the observed score is the sum of regression coefficients for the true score and item specific score each weighted by their squared reliability.

Item specific effects are now acknowledged beyond network approaches to psychopathology as recent work in the personality literature also argues for the importance of item effects in the form of “trait nuances” (McCrae, 2015, Mottus et al., 2016; Mottus & Rozgonjuk, 2019; Vainik et al., 2015) in both explanatory and predictive (e.g., Seeboth & Mottus, 2018; Achaa-Amankwaa, Olaru, & Schroeders, 2021) contexts. This suggests that the traditional domain representation of these constructs fail to fully capture subtleties that may prove consequential for predictive aims. The prevalence of these effects in applied settings, combined with common “on the fly” measurement practices (Flake, Pek, & Hehman, 2017), further undermine the use of sum scores as predictors. Similarly, modeling these item scores with SEM approaches may be intrinsically susceptible to interpretational confounding.

Empirical Example

To demonstrate the contribution of individual scale items within a scale in predicting an outcome, we consider four scales measuring constructs identified as risk factors of suicidal ideation (SI). SI broadly refers to thoughts about committing or planning suicide with passive and active domains of ideation identified (Liu, Bettis, & Burke, 2020). Consequently, passive

and active SI were considered separately as outcome variables and measured using the Passive and Active Suicidal Ideation Scales (PASIS; Liu et al., under review). Each SI scale consisted of item items probing the respective domain of ideation; scale sum scores were used as the outcome variables for all regression models.

A well-known risk factor for SI is depression, characterized by persistent down mood and anhedonia (American Psychiatric Association, 2013; World Health Organization, 2018). Three depression scales, the Patient Health Questionnaire (PHQ-9), the Beck Depression Inventory (BDI-II; Beck, Brown, & Steer, 1996), and the Depression Anxiety and Stress Scale (DASS-21; Lovibond & Lovibond, 1995), are independently considered in predicting both passive and active SI. Both the PHQ-9 and the BDI-II were constructed as symptom lists based on diagnostic criteria while the DASS-21 development emphasized high psychometrics and implemented iterative factor analyses (Lovibond & Lovibond, 1995). DASS-21 items are hypothesized to be drawn from three separate domains (i.e., depression, anxiety, and stress). Scales containing an item targeting SI (i.e., PHQ-9, BDI-II) are considered with and without that item.

The final risk factor measure, the Interpersonal Needs Questionnaire (INQ; van Orden et al., 2012), assesses perceived burdensomeness (PB) and thwarted belongingness (TB), both identified as proximal risk factors of SI. Construction of the INQ involved an iterative sequence of factor analytic models evaluated in multiple samples; poor items identified in initial modeling were removed from subsequent analyses (Van Orden et al., 2012). PB and TB were identified as theoretically relevant constructs prior to instrument construction and empirically supported from factor analytic results meaning that the INQ contains two domains of interest. Under BDT, this suggests that PB and TB items are exchangeable within each domain but not between domains. Reliability metrics (e.g., alpha, omega) for all scales can be found in Table 1.

Data were collected from an online community, Amazon Mechanical Turk (mTurk) at two time points approximately two weeks apart. Research was approved by the University of Notre Dame Institutional Review Board for research ethics. Participants completed blocks of questionnaires targeting suicidality as well as theoretically relevant risk factors. Block order was randomized to minimize potential order effects of questionnaires. At time 1, roughly 30% of predictors were missing as not all measures were administered to all participants during block randomization in the larger study. The outcomes of interest measured at time 2 had minimal missingness (~0.6%). Missing data was imputed using the *mice* package (van Buuren & Groothuis-Oudshoorn, 2011) in R (R Core Team, 2021) resulting in a final sample size of $N=1013$. Participants were primarily white (75.8%), non-Hispanic or Latinx (85.5%), and identified as males (55.8%) with mean age 37.34 years.

In total, across the four risk factor scales and two outcome scales twenty-eight separate multiple regression models were obtained (see Tables 2 and 3). Predictor scale sum scores and item scores were considered separately for all predictors and outcomes; moreover, subscale scores were included for the DASS-21 and the INQ as well. Risk factors assessed at time 1 were used as predictors for SI at time 2.

For this example, we examined the performance of traditional regression models in predicting passive and active SI from risk factor measures with differing item conceptualizations. We are primarily interested in comparing the performance of models predicting SI from scale scores with the item scores of the same measure (e.g., PHQ total vs. PHQ items) and do not seek to determine which measure best predicts SI. Models are evaluated using AIC, BIC, model R^2 and root mean square error (RMSE). Model R^2 reflects the overall outcome variability accounted for by predictors, while RMSE provides a metric of case-wise prediction error.

Since R^2 and RMSE incur no penalty for increasing model complexity, we expect them to improve with more granular predictors. Both AIC and BIC, however, directly model complexity as the number of estimated parameters. BIC is asymptotically consistent (Shao, 1997) and possesses a lower false positive rate relative to AIC (Vrieze, 2012); consequently, BIC is used as the primary selection metric given its emphasis on parsimony. Differences in BIC (ΔBIC) larger than 10 serve as very strong evidence for selecting the model with smaller BIC while differences less than 2 provide weak evidence (Raftery, 1995).

We hypothesize that predictor scales constructed in accordance with BDT (DASS-21 and INQ) will demonstrate diminishing returns in R^2 , RMSE, and AIC when directly modeling scale items; further, for these two scales we expect BIC to select the subscale score models since items within subscales are conceptually exchangeable with respect to the domain. For the PHQ-9 and BDI-II, we expect improvement across all metrics as the predictor variables become more granular. Since the models being compared are not nested models, we rely on information criteria to select the optimal model (i.e., of those considered) but report R^2 and RMSE as well to aid interpretation. Throughout ΔBIC and ΔAIC are computed as the complex model (i.e., relatively more parameters) minus the simpler model (i.e., fewer parameters) such that negative ΔBIC provide evidence in favor of the complex model.

Results

PHQ-9

Direct inclusion of the PHQ-9 items ($R^2 = 0.58$, $RMSE = 6.34$) directly reduced both information criteria relative to the scale ($R^2 = 0.43$, $RMSE = 7.39$), score model ($\Delta AIC = -292.8$, $\Delta BIC = -253.45$) in predicting active SI. A similar pattern emerged even when the PHQ-9 item

targeting SI was removed ($\Delta AIC = -176.54$, $\Delta BIC = -142.09$). Unsurprisingly, the item model ($R^2 = 0.49$, $RMSE = 6.98$) still predicted SI better than the scale score model ($R^2 = 0.39$, $RMSE = 7.67$).

A similar pattern occurred with passive SI as the outcome. The item score model () was preferred both including the SI item ($R^2 = 0.62$, $RMSE = 6.24$, $\Delta AIC = -55.63$, $\Delta BIC = -16.26$) and without it ($R^2 = 0.52$, $RMSE = 7.19$, $\Delta AIC = -112.74$, $\Delta BIC = -78.3$) relative to simply modeling the scale score ($R^2 = 0.50$, $RMSE = 7.22$; $R^2 = 0.46$, $RMSE = 7.77$ respectively). Interestingly, the item model for the PHQ-9 without the SI item outperformed the scale score model including the SI item ($\Delta AIC = -101.6$, $\Delta BIC = -67.16$) when predicting active SI but not passive SI ($\Delta AIC = 184.76$, $\Delta BIC = 219.2$).

BDI-II

The item level BDI model for active SI ($R^2 = 0.38$, $RMSE = 7.72$) improved information criteria ($\Delta AIC = -237.41$, $\Delta BIC = -138.99$) relative to the scale score model ($R^2 = 0.18$, $RMSE = 8.85$). Similarly, when the SI item from the BDI-II was removed from consideration, this improvement remained ($\Delta AIC = -181.42$, $\Delta BIC = -87.92$).

For the BDI-II, the item level model ($R^2 = 0.43$, $RMSE = 7.69$) outperformed the scale level model ($R^2 = 0.24$, $RMSE = 8.66$) when predicting passive SI ($\Delta AIC = -245.05$, $\Delta BIC = -146.63$). When the item targeting SI is removed, a similar pattern is observed ($\Delta AIC = -176.84$, $\Delta BIC = -83.34$). Again, for active SI the item level model without the BDI ideation item outperformed the BDI total score model including the ideation item ($\Delta AIC = -170.99$, $\Delta BIC = -77.49$); moreover, the same was true for passive SI ($\Delta AIC = -163.77$, $\Delta BIC = -70.27$).

DASS-21

For active SI, item level modeling of the DASS-21 ($R^2 = .53$, RMSE = 6.72) improved upon the total score model ($R^2 = .44$, RMSE = 7.35; $\Delta AIC = -139.25$, $\Delta BIC = -40.84$); however, it did not substantially improve relative to the subscale model ($R^2 = 0.48$, 7.07; $\Delta AIC = -65.16$, $\Delta BIC = 23.41$). The subscale model also outperformed the total score model ($\Delta AIC = -74.09$, $\Delta BIC = -64.25$). Consequently, modeling of the DASS-21 subscales appears sufficient.

The subscale level model for passive SI ($R^2 = 0.51$, RMSE = 7.13) substantially reduced information criteria metrics relative to the scale score model ($R^2 = .49$, RMSE = 7.26; $\Delta AIC = -31.12$, $\Delta BIC = -21.29$); however, the item level model ($R^2 = .56$, RMSE = 6.77) did not improve upon this ($\Delta AIC = -68.19$, $\Delta BIC = 20.39$). Additionally, the item level model was not found superior to the total score model when accounting for model complexity ($\Delta BIC = -0.9$).

INQ

When predicting active SI, the INQ item level model ($R^2 = 0.55$; RMSE = 6.58) outperformed the total score model ($R^2 = 0.28$; RMSE = 8.32; $\Delta AIC = -449.34$, $\Delta BIC = -380.45$); however, did not improve upon the subscale model ($R^2 = 0.51$; RMSE = 6.86; $\Delta AIC = -58.34$, $\Delta BIC = 5.63$). A similar pattern was found with passive SI as the outcome (see Figures 3 and 4).

Summary

This example examined a series of SI risk factor scales and evaluated which level of granularity (i.e., scale total score, subscale score, or item scores) provides the most efficient, in terms of information criteria, account of both active and passive SI using linear regression. Two scales constructed in alignment with BDT recommendations (i.e., the DASS-21 and INQ) demonstrated that inclusion of predictor subscale scores were sufficient. While R^2 , RMSE, and AIC continued to show improvement when transitioning from subscales scores to item scores,

the gain was insufficient when considering model complexity using BIC. This suggests that properly identifying the dimensionality of a scale constructed under the behavior domain perspective is crucial when evaluating predictive validity.

An additional two scales constructed from non-BDT approaches (i.e., PHQ-9, BDI-II) were also examined. For these instruments, the item score models outperformed the total score models even when accounting for model complexity. Additionally, since both of these scales contained an item targeting the outcome, analyses were repeated without this SI item where the same pattern was observed. Furthermore, the item score model without the SI predictor item outperformed the total score model containing it. This suggests that aggregating item scores may produce worse prediction models than omitting highly relevant variables entirely. As a result, measuring depression at the symptom level may prove more fruitful than at the construct level (e.g., Fried 2015).

Overall, these results demonstrate that the conceptualization behind scale development have implications for prediction. Measures designed using under BDT prioritize identification of the underlying domain; further, once the domain is adequately defined, the inclusion of additional items will not substantially improve predictive validity of the scale. Following from ideas of Ellis & Junker (1997), items sampled from a tail-measurable domain are inherently exchangeable with respect to the construct. Adding more provides negligible contributions to the outcome because these items are restricted by their covariance with previous items. In fact, modeling BDT derived items directly may impair prediction efforts by increasing prediction variance without compensatory predictive signal (e.g., Hocking, 1976).

Measures constructed with less emphasis on item covariance structure, however, may be more appropriately modeled at the item level particularly if predicting a criterion is relevant.

Primary concerns about measurement error for statistical inference (e.g., parameter bias, altered power) may be methodologically addressed without relying on common factor approaches (e.g., Fuller, 1987; Carroll et al, 2006). It is not sufficient to justify scale scores over items scores simply for arguments of reliability or precision; the nature of the measurement model imposed inherently influences subsequent interpretations and generalizations drawn from the scores. Over-reliance on measurement methods subsumed within the behavior domain approach has merged statistical models with theoretical ones (e.g., Haefel et al., 2021) resulting in the theoretical stagnation forecasted by Meehl (1978).

Simulation Study

To examine the potential impact of interpretational confounding when a structural path is added from a measurement model indicator to an external criterion variable, we conducted a simulation in which nonzero paths between item specific variance and the outcome variable were not all zero. We consider a simple single factor model with four indicator variables (X1-X4) and a single manifest outcome variable (Y; e.g., Figure 2 with 4 indicators). The latent factor and all four item specific variables were generated as standard normal variables. Item specific variance was constrained to represent half of the remaining variance not attributable to the latent factor; the remaining variance constitutes random error and was generated to ensure each the variance of each observed variable equaled one. The outcome was generated as a weighted composite of the latent factor and item specific effects as well as an error term with zero mean and variance of 0.25 (i.e. $Y = \beta_F F + \beta_S S + \text{error}$).

Factor loadings ($\lambda_1, \dots, \lambda_4$), the structural parameter relating the outcome to the latent factor (β_F), and parameters relating the outcome to item specific effects ($\beta_{s1} - \beta_{s4}$) were varied (see table 4 for a complete breakdown). Two patterns of misspecification were considered across

two magnitudes. First, all item specific components were allowed to relate to Y but remain independent of each other (all $\beta_s = 0.3$, all $\beta_s = 0.1$). Second, only two items (i.e., X1 and X3) demonstrated item specific relationships with the outcome ($\beta_{s1} = \beta_{s3} = 0.3$, $\beta_{s1} = \beta_{s3} = 0.1$). We refer to the former as uniform misspecification and the latter as nonuniform misspecification. Similarly, we refer to misspecification with $\beta_s = 0.3$ as large and $\beta_s = 0.1$ as small simply for convenience. Sample size was also varied ($N = 100, 500, 5000$) resulting in 144 conditions each with 1000 replications.

For each replication, an SEM model omitting the item specific outcome paths was fit using *lavaan* (Rosseel, 2012). Model R^2 , RMSE, β_F , and λ_i estimates were extracted and compared to the corresponding true model values to examine consequences of this misspecification. Structural and measurement parameters from the SEM model were assessed by average relative bias (RB) and average RMSE over the replications. RB quantifies the degree estimates are over or under estimated relative to the population value. Parameter estimate RMSE serves as a metric of overall accuracy as it incorporates aspects of bias and efficiency in its computation. The comparative fit index (CFI; Bentler, 1990) and the root mean square error of approximation (RMSEA; Steiger, 1990) were also examined for the SEM models to assess the misspecification impact on common fit indices. The *lavInspect* function was used to extract R^2 , β_F , and fit indices. The *lavPredict* function was used to extract case-level outcome predictions to compute RMSE. A linear regression regressing the outcome onto the manifest indicators was also conducted for comparison. Patterns across sample size conditions were similar; consequently, we only discuss the results for $N = 500$ below.

Results

Model R^2

True R^2 was calculated as the proportion of total outcome variance attributable to the generating model (i.e., from latent factor and latent specific effects) and naturally increased as the magnitude of misspecification and the structural parameter increased. For conditions with low factor loadings ($\lambda_j = 0.5$) and small misspecification, R^2 estimates may be mildly positively biased (see Figure 6). Model misspecification in other conditions, however, reduced R^2 estimates and obscured the ability to explain outcome variance. Additionally, latent variable modeling demonstrated larger R^2 estimates compared to direct item regression across conditions.

RMSE

Model RMSE varied across conditions while true RMSE remained constant. In general, as the latent factor and item specific effects increase in magnitude, less discrepancy between model-implied and observed outcomes occurred. For conditions with factor loadings less than $\lambda_i = 0.9$, model RMSE decreased beyond true RMSE for both small uniform and nonuniform misspecification. When $\lambda_i = 0.5$, this overfitting extended to strong misspecification when the latent factor effect was large (see Table 4). This suggests individual predictions in the latent variable model overfit observations in these cases.

Case-level performance of the regression models, however, worsened as the latent factor effect increased. Regression performance was closest to true RMSE when model misspecification was nonuniform small, factor loadings were large, and the latent factor was weakly related to the outcome ($\beta_F = 0.3$). Further, regression on the indicators produced less accurate predictions than the latent variable model in all conditions.

Parameter Estimates

In line with previous studies (Cole et al., 2007; Levy, 2017), structural parameters were upwardly biased in all conditions though the bias was drastically reduced when factor loadings were large (i.e., $\lambda_i = 0.9$) and approached zero in these conditions as the magnitude of misspecification decreased. Structural coefficient estimates were most impacted in conditions with large misspecification, weak factor loadings, and small population level structural coefficients relating the latent factor to the outcome (see Table 5).

Parameter bias patterns in factor loadings also mirrored those previously found. For conditions with uniform misspecification and equal factor loadings, factor loading bias is negligible as it was likely absorbed in the structural estimate. When factor loadings were unequal, however, both positive and negative bias occurred in nearly all conditions (see Table 6). Smaller bias was observed in factor loading estimates when population loadings were small and non-uniform misspecification occurred.

CFI and RMSEA

Both CFI and RMSEA suggested good fit for all SEM conditions considered (CFI > 0.97; RMSEA < 0.05). Interpretational confounding of the kind considered here constitute relatively small misspecifications (i.e., 2 or 4 omitted weak structural paths) making it incredibly unlikely to detect using traditional fit indices guidelines. While all conditions possessed acceptable fit, conditions with strong non-uniform misspecification demonstrated the worst observed values particularly when combined with low factor loadings. Examination of modification indices suggested the addition of covariances between items and outcome may improve the model; however, relying on modification indices alone especially when an acceptable model by other metrics has already been obtained (e.g., MacCallum, 1986).

Summary

Combined, these simulation results suggest that model misspecification from item specific effects adversely impact SEM in insidious ways. Models with low to moderate factor loadings produce negatively biased RMSE when weak misspecification occurs (i.e., both uniformly and nonuniformly). Unfortunately, these measurement conditions are more likely observed in practice. Similarly, the possibility of small item effects is also more in line with applied research as well as the empirical example shown above. These findings bring into question the veracity of RMSE as a prediction metric in SEM when indicators are not simply sampled from a behavior domain and may have specific contributions to external variables.

For low factor loadings, R^2 estimates were also inflated for certain conditions though this was more isolated and dependent on the strength of relationship between the latent factor and outcome. However, we expect R^2 estimates to be increasingly inflated as factor loadings are reduced and the strength of the relationship between the latent factor and outcome increase. These findings are in line with the existing research on interpretational confounding (Burt, 1976; Levy, 2017) in that scenarios with poor measurement (i.e., small factor loadings) in tandem with strong structural relationships between latent predictors and outcomes are most impacted by interpretational confounding.

While not of primary interest in this paper, bias in parameter estimates is in accordance with existing simulation studies examining interpretational confounding (e.g., Levy, 2017) and correlated residuals (e.g., Cole et al., 2007). The structural parameters were upwardly biased across all conditions. This follows from the model expected covariance between indicators and the outcome to be solely through the latent factor (i.e., $\text{Cov}_{\text{model}}(x_j, Y) = \lambda_i \sigma_T^2 \beta_T$) but the actual covariance between the confounding indicators is larger due to the residual covariance with the

outcome (i.e., $\text{Cov}_{\text{true}}(x_j, Y) = \lambda_i \sigma_T^2 \beta_T + \sigma_{s_i}^2 \beta_{s_i}$). The bias decreased when indicators highly reflected the latent factor as well as when the magnitude of interpretational confounding decreased. Bias in factor loadings only occurred with population loadings were not all equal or when these loadings were small and the pattern of misspecification was non-uniform. When population loadings were equal or the interpretational confounding occurred uniformly across indicators, the bias manifested in the structural coefficient.

Together, these limit the prediction utility of SEM models in psychology primarily for case-level goals. RMSE inflation indicates that the model estimates may overly conform to idiosyncrasies of the data; however, the R^2 results suggest that the overall outcome variability is less impacted by misspecification. Additionally, examination of fit indices does not appear to allude to potential misspecification. This further emphasizes the need for direct theoretical consideration of item-specific effects as traditional statistical approaches may fail. It is also worth noting that interpretational confounding also threatens the explanatory value of SEM. Parameter bias fosters systematic misinterpretations concerning the strength of relationships between latent variables and outcomes. This may overstate the importance of constructs as explanatory agents for a given outcome.

Discussion

In this paper, we reviewed the relationship between reliability and prediction, the importance of item specific effects, and some limitations of SEM as a predictive method. For multi-item scales, items with less than perfect reliability are preferable to their counterparts in terms of predictive validity; the decrease in reliability allows individual items to contribute unique information by ultimately increasing the explained variance of the outcome. The importance of these unique effects may also prove important in prediction of external variables

beyond the latent construct. Further we highlight the assumptions underlying traditional measurement, namely that items are sampled from infinite behavior domains and should be exchangeable with respect to the latent construct. Many scales in practice, however, do not undergo the rigorous construction process to produce psychometrically homogeneous instruments.

In an empirical example, we highlight the importance of scale conceptualization and scoring in predictive contexts. Scales constructed to preserve behavior domain assumptions contained items which did not provide information beyond the composite score as long as the dimensionality of the construct was considered (i.e., via subscale scores). On one hand, this suggests that the recommended iterative factor analytic methods of scale construction are functioning as intended. Items from these scales simply provide information about the underlying domain(s). But on the other, these items are redundant and provide no additional predictive signal. Conversely, items from scales which represent constitutive approaches to measurement (i.e., the items are components of the construct) did provide incremental information about the outcome. When considering items from these scales as predictors for outcome variables, it is crucial to consider how items may uniquely contribute to prediction (or explanation of outcome variance, i.e., R^2) prior to resorting to the use of composite scores.

The simulation study demonstrates that item specific effects which covary with the outcome of interest result in interpretational confounding. Structural estimates (i.e., regression of the outcome onto the latent factor) were positively biased across all conditions considered. Measurement parameters showed bias whenever item specific effects were non-uniform; structural bias absorbed this bias when uniform confounding occurred. More importantly for prediction, SEM overfits case-level prediction in the presence of interpretational confounding

making it ill-equipped for prediction. This susceptibility of SEM to interpretational confounding is particularly problematic for questions of incremental validity which are often motivated by predictive utility (Westfall & Yarkoni, 2016). Combined with the inability to evaluate fully reflective models on holdout data, SEM should not generally be used to assess predictive goals. For questions of incremental validity, it should be used with caution in conjunction with justification (both theoretical and statistical) that manifest predictors do not uniquely explain outcome variance.

In total, these findings emphasize the importance of items in bridging the gap between explanatory and predictive goals of psychology. Items form the basis of analysis particularly in psychological research so heavily reliant on scales and questionnaires. As demonstrated above, scales targeting the same construct with comparable psychometrics (i.e., internal consistency) perform very differently depending on how the items are conceptualized. Items written in line with domain sampling principles may be better equipped for explanatory or inferential aims. Instruments constructed without this emphasis, however, may better lend themselves to predictive aims as individual items tend to provide unique information which may be informative to external constructs. When explanatory or inferential goals are sought from measures constructed “on the fly” or lacking psychometric homogeneity, alternative methods to account for measurement error should be considered (e.g., simulation extrapolation; Carrol et al., 2006; Cook & Stefanski, 1994). Succinctly, the conceptualization of items should match the intended level of abstraction for subsequent analyses.

An area of research in which item conceptualization remains relatively unstudied is that of network analysis. In psychopathology network research, items are treated as proxies for symptoms. Intra-symptom relationships are then examined in terms of their partial correlation

structure which are influenced directly by unique item information. Consequently, networks evaluated from scales constructed in accordance with domain sampling ideas may produce sparser networks than those extracted from instruments containing conceptually distinct items. Alternatively, scales which do not preserve psychometric homogeneity may produce more connected networks. Similarly, applications of machine learning may also be stunted by the use of psychometrically homogeneous scales. These scales are constructed to facilitate clear inferences concerning constructs of interest; however, item responses are unlikely to provide additional outcome information making them inefficient from a prediction perspective.

While the domain sampling approach provides a useful measurement framework for psychometrically homogenous constructs, it may be more appropriate when applied to behavioral constructs rather than abstract mental constructs (Markus & Borsboom, 2013). We argue that it is imperative for researchers to consider and articulate the intended function of scale items for a given study. In the spirit of transparent reporting, we recommend that researchers justify the inclusion or omission of item specific effects using theoretical rationale and study-oriented goals. Kane's (2013) validity framework provides a useful template for articulating arguments for scores as appropriate evidence for their intended interpretations and uses (e.g., prediction) sought from the data (see Cook et al., 2015). Note that we are not arguing unilaterally against (or for) the behavior domain conceptualizations of measurement but instead wish to add to the cry for critical consideration of measurement throughout the research process (Blinkhorn, 1997; Flake & Fried, 2020; Flake et al., 2017; Lilienfeld & Strother, 2020). Similarly, the use of latent variable measurement models and SEM to address measurement error is often invoked reflexively in psychology; however, the effectiveness of this approach also depends on the conceptualization of the scales and the goals of the analysis.

Footnotes

1. Depending on the type of misspecification, there is no model misfit at all, just biased parameter estimates. Further, if the residual covariances are relatively equal in magnitude between the observed indicators and outcome, then one would not observe an appreciable change in factor loadings by fitting models with and without the outcome (following Bollen's [2017] suggestion).
2. This depends on the type of unique relationship between the observed indicators and outcome, but in the simple case of equal relationships among observed indicators there is a corresponding bias to the explained variance of the outcome.

References:

- Achaa-Amankwaa, P., Olaru, G., & Schroeders, U. (2021). Coffee or tea? Examining cross-cultural differences in personality nuances across former colonies of the British Empire. *European Journal of Personality*, 35(3), 383-397.
- Aiken, L. S., & West, S. G. (1991). *Multiple regression: Testing and interpreting interactions*. Sage Publications, Inc.
- Beck AT, Brown G, & Steer RA. (1996). *Beck Depression Inventory II manual*. San Antonio, TX: The Psychological Corporation
- Bentler, P. M. (1990). Comparative fit indexes in structural models. *Psychological Bulletin*, 107(2), 238–246. <https://doi.org/10.1037/0033-2909.107.2.238>
- Bock, R. D., Gibbons, R., & Muraki, E. (1988). Full-Information Item Factor Analysis. *Applied Psychological Measurement*, 12(3), 261–280.
<https://doi.org/10.1177/014662168801200305>
- Bollen, K. A. (1989). *Structural equations with latent variables*. John Wiley & Sons.
<https://doi.org/10.1002/9781118619179>
- Bollen, K. A. (2007). Interpretational confounding is due to misspecification, not to type of indicator: Comment on Howell, Breivik, and Wilcox (2007).
- Bollen, K., & Lennox, R. (1991). Conventional wisdom on measurement: A structural equation perspective. *Psychological Bulletin*, 110, 305-314
- Borsboom, D., & Cramer, A. (2013). Network Analysis: An Integrative Approach to the Structure of Psychopathology. *Annual Review of Clinical Psychology*, 9, 91–121.
<https://doi.org/10.1146/annurev-clinpsy-050212-185608>
- Borsboom, D., Mellenbergh, G. J., & Heerden, J. V. (2003). The theoretical status of latent variables. *Psychological Review*, 203–219.
- Breiman, L. (2001). Random Forests. *Machine Learning*, 45(1), 5–32.
<https://doi.org/10.1023/A:1010933404324>
- Brown, W. (1910). Some Experimental Results in the Correlation of Mental Abilities1. *British Journal of Psychology*, 1904-1920, 3(3), 296–322. <https://doi.org/10.1111/j.2044-8295.1910.tb00207.x>

- Browne, M. W. (2000). Cross-validation methods. *Journal of mathematical psychology*, 44(1), 108-132.
- Burt, R. S. (1976). Interpretational Confounding of Unobserved Variables in Structural Equation Models. *Sociological Methods & Research*, 5(1), 3–52.
<https://doi.org/10.1177/004912417600500101>
- Carroll, R. J., Ruppert, D., Stefanski, L. A., & Crainiceanu, C. M. (2006). *Measurement Error in Nonlinear Models: A Modern Perspective*, Second Edition (2nd ed.). Chapman and Hall/CRC. <https://doi.org/10.1201/9781420010138>
- Clark, L., & Watson, D. (1995). Constructing Validity: Basic Issues in Objective Scale Development. *Psychological Assessment*, 7(3) 309-319.
- Clark, L. A., & Watson, D. (2019). Constructing Validity: New Developments in Creating Objective Measuring Instruments. *Psychological Assessment*, 31(12), 1412–1427.
<https://doi.org/10.1037/pas0000626>
- Cohen, J., Cohen, P., West, S. G., & Aiken, L. S. (2013). *Applied multiple regression/correlation analysis for the behavioral sciences*. Routledge.
- Cole, D. A., Ciesla, J. A., & Steiger, J. H. (2007). The insidious effects of failing to include design-driven correlated residuals in latent-variable covariance structure analysis. *Psychological Methods*, 12(4), 381–398. <https://doi.org/10.1037/1082-989X.12.4.381>
- Cook, D. A., Brydges, R., Ginsburg, S., & Hatala, R. (2015). A contemporary approach to validity arguments: a practical guide to Kane's framework. *Medical education*, 49(6), 560-575.
- Cook, J. R., & Stefanski, L. A. (1994). Simulation-Extrapolation Estimation in Parametric Measurement Error Models. *Journal of the American Statistical Association*, 89(428), 1314–1328. <https://doi.org/10.1080/01621459.1994.10476871>
- Crocker, L., & Algina, J. (2008). *Introduction to classical and modern test theory*. Cengage Learning.
- Cronbach, L. J. (1951). Coefficient alpha and the internal structure of tests. *Psychometrika*, 16(3), 297-334.
- Cronbach, L. J., & Meehl, P. E. (1955). Construct validity in psychological tests. *Psychological Bulletin*, 52(4), 281–302. <https://doi.org/10.1037/h0040957>

- Croon, M. (2002). Using Predicted Latent Scores in General Latent Structure Models. In *Latent Variable and Latent Structure Models*. Psychology Press.
- Davis, R. V. (1987). Scale Construction. *Journal of Counseling Psychology*, 34(4), 481-489.
- Devlieger, I., & Rosseel, Y. (2017). Factor Score Path Analysis: An Alternative for SEM? *Methodology*, 13(Supplement 1), 31–38. <https://doi.org/10.1027/1614-2241/a000130>
- Devlieger, I., Mayer, A., & Rosseel, Y. (2016). Hypothesis Testing Using Factor Score Regression: A Comparison of Four Methods. *Educational and Psychological Measurement*, 76(5), 741–770. <https://doi.org/10.1177/0013164415607618>
- Edwards, J. R., & Bagozzi, R. P. (2000). On the nature and direction of relationships between constructs and measures. *Psychological Methods*, 5, 155-174
- Ellis, J. L., & Junker, B. W. (1997). Tail-measurability in monotone latent variable models. *Psychometrika*, 62(4), 495–523. <https://doi.org/10.1007/BF02294640>
- Embretson, S. E., & Reise, S. P. (2013). *Item response theory*. Psychology Press.
- Flake, J. K., & Fried, E. I. (2020). Measurement Schmeasurement: Questionable Measurement Practices and How to Avoid Them. *Advances in Methods and Practices in Psychological Science*, 3(4), 456–465. <https://doi.org/10.1177/2515245920952393>
- Flake, J. K., Pek, J., & Hehman, E. (2017). Construct Validation in Social and Personality Research: Current Practice and Recommendations. *Social Psychological and Personality Science*, 8(4), 370–378. <https://doi.org/10.1177/1948550617693063>
- Fried, E. I. (2015). Problematic assumptions have slowed down depression research: Why symptoms, not syndromes are the way forward. *Frontiers in Psychology*, 6, 309. <https://doi.org/10.3389/fpsyg.2015.00309>
- Friedman, J. H. (2001). Greedy Function Approximation: A Gradient Boosting Machine. *The Annals of Statistics*, 29(5), 1189–1232.
- Fuller, W. A. (1987). Measurement error models. In *Wiley Series in Probability and Mathematical Statistics*.
- Haefel, G. J., Jeronimus, B. F., Kaiser, B. N., Weaver, L. J., Soyster, P. D., Fisher, A. J., Vargas, I., Goodson, J. T., & Lu, W. (2021). Folk Classification and Factor Rotations: Whales, Sharks, and the Problems With the Hierarchical Taxonomy of Psychopathology (HiTOP). *Clinical Psychological Science*, 21677026211002500. <https://doi.org/10.1177/21677026211002500>

- Hastie, T., Tibshirani, R., & Friedman, F. (2009). *The Elements of Statistical Learning: Data Mining, Inference, and Prediction* (2nd ed.). Springer-Verlag.
- Hayes, T. (2021). R-squared change in structural equation models with latent variables and missing data. *Behavior Research Methods*. <https://doi.org/10.3758/s13428-020-01532-y>
- Hayes, T., & Usami, S. (2020). Factor Score Regression in the Presence of Correlated Unique Factors. *Educational and Psychological Measurement*, 80(1), 5–40.
<https://doi.org/10.1177/0013164419854492>
- Hocking, R. R. (1976). A Biometrics Invited Paper. The Analysis and Selection of Variables in Linear Regression. *Biometrics*, 32(1), 1–49. <https://doi.org/10.2307/2529336>
- Holland, P. W., & Rosenbaum, P. R. (1986). Conditional Association and Unidimensionality in Monotone Latent Variable Models. *The Annals of Statistics*, 14(4), 1523–1543.
- Jacobucci, R., & Grimm, K. J. (2020). Machine Learning and Psychological Research: The Unexplored Effect of Measurement. *Perspectives on Psychological Science*, 15(3), 809–816. <https://doi.org/10.1177/1745691620902467>
- Joreskog, K. G. & Sorbom, D. (1979). *Advances in factor analysis and structural equation models*. Cambridge, MA: Abt Books
- Kane, M. T. (2013). Validating the Interpretations and Uses of Test Scores. *Journal of Educational Measurement*, 50(1), 1–73. <https://doi.org/10.1111/jedm.12000>
- Kendler, K. S. (2020). The Origin of Our Modern Concept of Depression—The History of Melancholia From 1780-1880: A Review. *JAMA Psychiatry*, 77(8), 863.
<https://doi.org/10.1001/jamapsychiatry.2019.4709>
- Kendler, K. S. (2017a). DSM disorders and their criteria: How should they inter-relate? *Psychological Medicine*, 47(12), 2054–2060.
<https://doi.org/10.1017/S0033291717000678>
- Kendler, K. S. (2017b). The clinical features of mania and their representation in modern diagnostic criteria. *Psychological Medicine*, 47(6), 1013–1029.
<https://doi.org/10.1017/S0033291716003238>
- Klonsky, E. D., & May, A. M. (2015). The Three-Step Theory (3ST): A New Theory of Suicide Rooted in the “Ideation-to-Action” Framework. *International Journal of Cognitive Therapy*, 8(2), 114–129. <https://doi.org/10.1521/ijct.2015.8.2.114>

- Kroenke, K., Spitzer, R. L., & Williams, J. B. W. (2001). The PHQ-9: Validity of a brief depression severity measure. *Journal of General Internal Medicine*, 16(9), 606–613. <https://doi.org/10.1046/j.1525-1497.2001.016009606.x>
- Levy, R. (2017). Distinguishing outcomes from indicators via Bayesian modeling. *Psychological Methods*, 22(4), 632–648. <https://doi.org/10.1037/met0000114>
- Lilienfeld, S. O., & Strother, A. N. (2020). Psychological measurement and the replication crisis: Four sacred cows. *Canadian Psychology/Psychologie Canadienne*, 61(4), 281–288. <https://doi.org/10.1037/cap0000236>
- Liu, R. T., Bettis, A. H., & Burke, T. A. (2020). Characterizing the phenomenology of passive suicidal ideation: A meta-analysis of its prevalence, psychiatric comorbidity, correlates, and comparisons with active suicidal ideation. *Psychological Medicine*, 50(3), 367–383. <https://doi.org/10.1017/S003329171900391X>
- Liu, R. T., Lawrence, H. R., Burke, T. A., Sanzari, C. M., Levin, R. Y., Maitlin, C., Paszek, C., & Zhu, X. *Passive and active suicidal ideation among left-behind children in rural China: An evaluation of intrapersonal and interpersonal vulnerability and resilience* (manuscript under review).
- Loevinger, J. (1954). The attenuation paradox in test theory. *Psychological Bulletin*, 51(5), 493–504. <https://doi.org/10.1037/h0058543>
- Lord, F. M. (1955). Estimating Test Reliability. *Educational and Psychological Measurement*, 15(4), 325–336. <https://doi.org/10.1177/001316445501500401>
- Lord, F. M., Novick, M. R., & Birnbaum, A. (1968). *Statistical theories of mental test scores*. Addison-Wesley.
- MacCallum, R. (1986). Specification searches in covariance structure modeling. *Psychological Bulletin*, 100(1), 107. <https://doi.org/10.1037/0033-2909.100.1.107>
- Markus, K. A., & Borsboom, D. (2013). *Frontiers of Test Validity Theory: Measurement, Causation, and Meaning*. Routledge. <https://doi.org/10.4324/9780203501207>
- McCrae, R. R. (2015). A More Nuanced View of Reliability: Specificity in the Trait Hierarchy. *Personality and Social Psychology Review*, 19(2), 97–112. <https://doi.org/10.1177/1088868314541857>
- McDonald, R. P. (1996). Path Analysis with Composite Variables. *Multivariate Behavioral Research*, 31(2), 239–270. https://doi.org/10.1207/s15327906mbr3102_5

- McDonald, R. P. (1999). Test theory: A unified approach.
- McDonald, R. P. (2003). Behavior Domains in Theory and in Practice. *Alberta Journal of Educational Research*, 49(3), 212–230.
- McNally, R. J. (2016). Can network analysis transform psychopathology? *Behaviour Research and Therapy*, 86, 95–104. <https://doi.org/10.1016/j.brat.2016.06.006>
- McNeish, D. (2018). Thanks coefficient alpha, we'll take it from here. *Psychological Methods*, 23(3), 412–433. <https://doi.org/10.1037/met0000144>
- McNeish, D., & Wolf, M. G. (2020). Thinking twice about sum scores. *Behavior Research Methods*, 52(6), 2287–2305. <https://doi.org/10.3758/s13428-020-01398-0>
- Meehl, P. E. (1978). Theoretical risks and tabular asterisks: Sir Karl, Sir Ronald, and the slow progress of soft psychology. *Journal of Consulting and Clinical Psychology*, 46(4), 806–834. <https://doi.org/10.1037/0022-006X.46.4.806>
- Messick, S. (1989). Meaning and Values in Test Validation: The Science and Ethics of Assessment. *Educational Researcher*, 18(2), 5–11.
<https://doi.org/10.3102/0013189X018002005>
- Möttus, R., Kandler, C., Bleidorn, W., Riemann, R., & McCrae, R. R. (2016). Personality traits below facets: The consensual validity, longitudinal stability, heritability, and utility of personality nuances. *Journal of Personality and Social Psychology*, 112(3), 474.
<https://doi.org/10.1037/pspp0000100>
- Möttus, R., & Rozgonjuk, D. (2019). Development is in the details: Age differences in the Big Five domains, facets, and nuances. *Journal of Personality and Social Psychology*, 120.
<https://doi.org/10.1037/pspp0000276>
- R Core Team (2021). R: A language and environment ofr statistical computing. R Foundation for Statistical Computing, Vienna, Austria. <https://www.R-project.org/>.
- Raftery, A. E. (1995). Bayesian Model Selection in Social Research. *Sociological Methodology*, 25, 111–163. <https://doi.org/10.2307/271063>
- Revelle, W. (2020) psych: Procedures for Personality and Psychological Research, Northwestern University, Evanston, Illinois, USA, <https://CRAN.R-project.org/package=psych> Version = 2.0.12,.

- Rhemtulla, M., van Bork, R., & Borsboom, D. (2020). Worse than measurement error: Consequences of inappropriate latent variable measurement models. *Psychological Methods*, 25(1), 30–45. <https://doi.org/10.1037/met0000220>
- Rigdon, E. E. (2016). Choosing PLS path modeling as analytical method in European management research: A realist perspective. *European Management Journal*, 34(6), 598–605.
- Rosseel, Y. (2012). lavaan: An R package for Structural Equation Modeling. *Journal of Statistical Software*, 48(2), 1–36.
- Seeboth, A., & Möttus, R. (2018). Successful explanations start with accurate descriptions: Questionnaire items as personality markers for more accurate predictions. *European Journal of Personality*, 32(3), 186–201.
- Shao, J. (1997). An Asymptotic Theory for Linear Model Selection. *Statistica Sinica*, 7(2), 221–242.
- Shmueli, G. (2010). To Explain or to Predict? *Statistical Science*, 25(3), 289–310. <https://doi.org/10.1214/10-STS330>
- Shmueli, G., Ray, S., Velasquez Estrada, J. M., & Chatla, S. B. (2016). The elephant in the room: Predictive performance of PLS models. *Journal of Business Research*, 69(10), 4552–4564. <https://doi.org/10.1016/j.jbusres.2016.03.049>
- Skrondal, A., & Kuha, J. (2012). Improved Regression Calibration. *Psychometrika*, 77(4), 649–669. <https://doi.org/10.1007/s11336-012-9285-1>
- Skrondal, A., & Laake, P. (2001). Regression among factor scores. *Psychometrika*, 66(4), 563–575. <https://doi.org/10.1007/BF02296196>
- Spearman, C. (1910). Correlation Calculated from Faulty Data. *British Journal of Psychology*, 1904-1920, 3(3), 271–295. <https://doi.org/10.1111/j.2044-8295.1910.tb00206.x>
- Steiger, J. H. (1990). Structural Model Evaluation and Modification: An Interval Estimation Approach. *Multivariate Behavioral Research*, 25(2), 173–180. https://doi.org/10.1207/s15327906mbr2502_4
- Tucker, L. R. (1946). Maximum validity of a test with equivalent items. *Psychometrika*, 11(1), 1–13. <https://doi.org/10.1007/BF02288894>

- Vainik, U., Möttus, R., Allik, J., Esko, T., & Realo, A. (2015). Are Trait–Outcome Associations Caused by Scales Or Particular Items? Example Analysis of Personality Facets and Bmi. *European Journal of Personality*, 29(6), 622–634. <https://doi.org/10.1002/per.2009>
- Van Buuren, S., Groothuis-Oudshoorn, K. (2011). mice: multivariate imputation chain equations in R. *Journal of Statistical Software*, 45(3), 1–67.
- Van Orden, K. A., Cukrowicz, K. C., Witte, T. K., & Joiner, T. E. (2012). Thwarted Belongingness and Perceived Burdensomeness: Construct Validity and Psychometric Properties of the Interpersonal Needs Questionnaire. *Psychological Assessment*, 24(1), 197–215. <https://doi.org/10.1037/a0025358>
- Van Orden, K. A., Witte, T. K., Cukrowicz, K. C., Braithwaite, S., Selby, E. A., & Joiner, T. E. (2010). The Interpersonal Theory of Suicide. *Psychological Review*, 117(2), 575–600. <https://doi.org/10.1037/a0018697>
- Vrieze, S. I. (2012). Model selection and psychological theory: A discussion of the differences between the Akaike Information Criterion (AIC) and the Bayesian Information Criterion (BIC). *Psychological Methods*, 17(2), 228–243. <https://doi.org/10.1037/a0027127>
- World Health Organization. (2018). *International classification of diseases for mortality and morbidity statistics* (11th Revision).
- Yarkoni, T., & Westfall, J. (2017). Choosing Prediction Over Explanation in Psychology: Lessons From Machine Learning. *Perspectives on Psychological Science*, 12(6), 1100–1122. <https://doi.org/10.1177/1745691617693393>
- Yuan, K.-H., & Chan, W. (2002). Fitting structural equation models using estimating equations: A model segregation approach. *British Journal of Mathematical and Statistical Psychology*, 55(1), 41–62. <https://doi.org/10.1348/000711002159699>

Figure 1. Correlation of scale scores with latent ability (θ) with item discrimination (σ), interitem correlation, and factor loading (λ_j) for equivalent items with median difficulty. Curves reproduced from Tucker's (1946) derivations with additional scale lengths. Vertical lines denote the maximum predictive validity for a given scale length.

Figure 2. Simplified path diagram relating a latent representation of depression (Dep) measured by p indicators ($d_1, d_2, \dots, d_p$) to a manifest outcome variable for suicidal ideation (SI). The left panel depicts SI as an outcome while the right panel depicts SI as an indicator of depression. Both depictions are equivalent models under simultaneous estimation. Note that unique variances and residuals are omitted.

Figure 3. Fully specified path diagram relating a latent depression to a manifest SI outcome.

Figure 4. Active SI Linear Regression Model Results. Error bars represented ± 2 SE computed using 100 bootstrap samples.

Figure 5. Passive SI Linear Regression Model Results. Error bars represented ± 2 SE computed using 100 bootstrap samples.

Figure 6. Simulation outcome R^2 and RMSE when $\lambda_i = 0.5$. Note: Uniform=all $\beta_s > 0$; Non-uniform = only $\beta_{s1}, \beta_{s3} > 0$; True = Data Generating Model; LV = latent variable approach (i.e., SEM); Reg = item regression.

Table 1. Internal Consistency Reliability Metrics for Empirical Example Scales

Scale	α	ω_{Total}	ω_{h}
PHQ-9	0.90	0.92	0.87
PHQ-9 (no SI)	0.88	0.91	0.86
BDI-II	0.93	0.95	0.83
BDI-II (no SI)	0.93	0.95	0.80
DASS-21	0.95	0.96	0.80
DASS-dep	0.91	0.94	0.84
DASS-anx	0.90	0.92*	0.81*
DASS-stress	0.86	0.89	0.76
INQ Total	0.91	0.95	0.62
INQ PB	0.96	0.97	0.92
INQ TB	0.87	0.91	0.59
PASIS	0.99	0.99	0.95
PASISact	0.98	0.98*	0.96*
PASISpas	0.98	0.99	0.97

Note: α = Cronbach's alpha; ω_{Total} = McDonald's Omega Total; ω_{h} = McDonald's Hierarchical Omega (McDonald, 1999); no SI refers to scales with corresponding SI item removed; DASS-dep = DASS depression items; DASS-anx = DASS anxiety items; DASS-stress = DASS stress items; PB = Perceived Burdensomeness; TB = Thwarted Belongingness; PASISact = PASIS active ideation subscale; PASISpas = passive ideation subscale; * factor score estimation did not converge for these subscales

Table 2. Active SI Linear Regression Models

Predictor	AIC	BIC	R ² (se ¹)	RMSE (se ¹)
PHQ Total	6932.30	6947.07	0.431 (0.0025)	7.39 (0.020)
PHQ Item	6639.50	6693.62	0.581 (0.0024)	6.34 (0.023)
PHQ Total (no SI)	7007.24	7022.00	0.388 (0.0026)	7.67 (0.022)
PHQ Item (no SI)	6830.70	6879.91	0.493 (0.0028)	6.98 (0.022)
INQ Total	7176.02	7190.78	0.277 (0.0022)	8.33 (0.024)
INQ Subscales	6785.02	6804.70	0.509 (0.0030)	6.86 (0.024)
INQ Item	6726.68	6810.33	0.548 (0.0027)	6.58 (0.024)
DASS Total	6921.19	6935.95	0.438 (0.0024)	7.35 (0.020)
DASS Scale	6847.10	6871.70	0.479 (0.0025)	7.07 (0.025)
DASS Item	6781.94	6895.11	0.529 (0.0024)	6.72 (0.020)
BDI Total	7298.52	7313.28	0.184 (0.0025)	8.85 (0.023)
BDI Item	7061.11	7174.29	0.379 (0.0027)	7.72 (0.026)
BDI Total (no SI)	7308.95	7323.71	0.175(0.0021)	8.90 (0.030)
BDI Item (no SI)	7127.53	7235.79	0.336 (0.0030)	7.98 (0.029)

¹ standard errors were calculated using 100 bootstrap samples. PHQ = Patient Health Questionnaire, INQ = Interpersonal Needs Questionnaire, DASS = Depression Anxiety and Stress Scale, BDI = Beck Depression Inventory II

Table 3. Passive SI Linear Regression Models

Predictor	AIC	BIC	R ² (se ¹)	RMSE (se ¹)
PHQ Total	6661.36	6676.12	0.495 (0.0018)	7.22 (0.017)
PHQ Item	6605.73	6659.86	0.623 (0.0019)	6.24 (0.021)
PHQ Total (no SI)	6958.86	6973.62	0.457 (0.0026)	7.77 (0.020)
PHQ Item (no SI)	6846.12	6895.32	0.521 (0.0022)	7.19 (0.020)
INQ Total	7079.81	7094.58	0.388 (0.0019)	7.95 (0.021)
INQ Subscales	6759.43	6779.11	0.555 (0.0027)	6.78 (0.027)
INQ Item	6713.73	6797.38	0.585 (0.0021)	6.54 (0.0234)
DASS Total	6896.06	6910.83	0.489 (0.0022)	7.26 (0.024)
DASS Scale	6864.94	6889.54	0.507 (0.0020)	7.13 (0.020)
DASS Item	6796.75	6909.93	0.555 (0.0019)	6.77 (0.025)
BDI Total	7299.89	7314.65	0.239 (0.0021)	8.86 (0.027)
BDI Item	7054.84	7168.02	0.426 (0.0021)	7.69 (0.025)
BDI Total (no SI)	7312.96	7327.72	0.229 (0.0024)	8.91 (0.026)
BDI Item (no SI)	7136.12	7244.38	0.377 (0.0026)	8.02 (0.023)

¹ standard errors were calculated using 100 bootstrap samples. Bolded values indicate selected model for a given predictor and criteria. PHQ = Patient Health Questionnaire, INQ = Interpersonal Needs Questionnaire, DASS = Depression Anxiety and Stress Scale, BDI = Beck Depression Inventory II

Table 4. Latent Variable Model RMSE Relative Bias

	Strong Uniform RB (RMSE)	Weak Uniform RB (RMSE)	Strong Non- uniform RB (RMSE)	Weak Non- uniform RB (RMSE)
$\beta_F = 0.3$				
All $\lambda_i = 0.5$	29.90 (0.16)	-7.73 (0.06)	12.94 (0.08)	-7.44 (0.05)
All $\lambda_i = 0.7$	47.08 (0.24)	1.30 (0.03)	24.04 (0.12)	-1.22 (0.03)
All $\lambda_i = 0.9$	54.14 (0.27)	5.97 (0.04)	29.48 (0.15)	2.56 (0.03)
$\lambda_1, \lambda_2 = .5,$ $\lambda_3, \lambda_4 = 0.7$	42.77 (0.22)	-1.29 (0.04)	21.28 (0.11)	-3.02 (0.04)
$\beta_F = 0.5$				
All $\lambda_i = 0.5$	10.68 (0.08)	-22.25 (0.12)	-3.06 (0.06)	-19.18 (0.11)
All $\lambda_i = 0.7$	38.70 (0.20)	-5.85 (0.05)	17.166 (0.09)	-7.40 (0.05)
All $\lambda_i = 0.9$	52.24 (0.26)	4.07 (0.04)	27.62 (0.14)	0.84 (0.03)
$\lambda_1, \lambda_2 = 0.5,$ $\lambda_3, \lambda_4 = 0.7$	30.94 (0.16)	-10.65 (0.07)	11.67 (0.07)	-11.28 (0.07)
$\beta_F = 0.7$				
All $\lambda_i = 0.5$	-9.96 (0.09)	-37.24 (0.20)	-18.22 (0.12)	-31.68 (0.17)
All $\lambda_i = 0.7$	29.00 (0.15)	-14.52 (0.08)	8.63 (0.06)	-15.22 (0.09)
All $\lambda_i = 0.9$	49.83 (0.25)	1.45 (0.03)	25.48 (0.13)	-1.71 (0.03)
$\lambda_1, \lambda_2 = 0.5,$ $\lambda_3, \lambda_4 = 0.7$	17.75 (0.10)	-21.58 (0.12)	0.07 (0.05)	-20.91 (0.11)

Note: RB = Relative Bias, RMSE = Root Means Squared Error (of parameter estimates), bolded values indicate RB larger than 10%. Results reflect average RB and RMSE values over 1000 replications with a simulated sample size of N=500. Strong Uniform indicates $\beta_s = 0.3$ for all indicators; Weak Uniform indicates $\beta_s = 0.1$ for all indicators; Strong Non-uniform indicates $\beta_s = 0.3$ for indicators 1 and 3 and zero elsewhere; Weak Non-uniform indicates $\beta_s = 0.1$ for indicators 1 and 3 and zero elsewhere.

Table 5. Structural Coefficient Estimate ($\hat{\beta}_F$) Relative Bias and RMSE

	Strong Uniform RB (RMSE)	Weak Uniform RB (RMSE)	Strong Non- uniform RB (RMSE)	Weak Non- uniform RB (RMSE)
$\beta_F = 0.3$				
All $\lambda_i = 0.5$	50.37 (0.16)	16.22 (0.07)	27.28 (0.10)	8.43 (0.05)
All $\lambda_i = 0.7$	20.89 (0.08)	7.56 (0.05)	10.89 (0.06)	3.31 (0.04)
All $\lambda_i = 0.9$	5.40 (0.05)	1.94 (0.04)	2.78 (0.04)	0.82 (0.04)
$\lambda_1, \lambda_2 = .5,$ $\lambda_3, \lambda_4 = 0.7$	29.23 (0.10)	9.67 (0.05)	14.84 (0.07)	5.00 (0.05)
$\beta_F = 0.5$				
All $\lambda_i = 0.5$	30.05 (0.16)	10.19 (0.07)	16.65 (0.10)	5.13 (0.05)
All $\lambda_i = 0.7$	12.38 (0.08)	3.82 (0.05)	6.68 (0.06)	1.78 (0.05)
All $\lambda_i = 0.9$	3.20 (0.05)	0.94 (0.04)	1.38 (0.05)	0.58 (0.04)
$\lambda_1, \lambda_2 = 0.5,$ $\lambda_3, \lambda_4 = 0.7$	17.45 (0.10)	5.85 (0.05)	9.51 (0.07)	2.71 (0.05)
$\beta_F = 0.7$				
All $\lambda_i = 0.5$	21.68 (0.16)	7.44 (0.07)	11.59 (0.10)	3.66 (0.06)
All $\lambda_i = 0.7$	9.18 (0.08)	3.04 (0.05)	4.54 (0.06)	1.62 (0.05)
All $\lambda_i = 0.9$	2.43 (0.06)	0.62 (0.05)	1.09 (0.05)	0.18 (0.05)
$\lambda_1, \lambda_2 = 0.5,$ $\lambda_3, \lambda_4 = 0.7$	12.81 (0.11)	3.92 (0.05)	6.16 (0.07)	1.77 (0.05)

Note: RB = Relative Bias, RMSE = Root Means Squared Error (of parameter estimates), bolded values indicate RB larger than 10%. Results reflect average RB and RMSE values over 1000 replications with a simulated sample size of N=500. Strong Uniform indicates $\beta_s = 0.3$ for all indicators; Weak Uniform indicates $\beta_s = 0.1$ for all indicators; Strong Non-uniform indicates $\beta_s = 0.3$ for indicators 1 and 3 and zero elsewhere; Weak Non-uniform indicates $\beta_s = 0.1$ for indicators 1 and 3 and zero elsewhere

Table 6. Factor Loading Estimate ($\hat{\lambda}$) Relative Bias and RMSE with Unequal Loadings

	Strong Uniform RB (RMSE)	Weak Uniform RB (RMSE)	Strong Non- uniform RB (RMSE)	Weak Non- uniform RB (RMSE)
$\beta_F = 0.3$				
λ_1	-12.74 (0.15)	-13.35 (0.15)	-10.70 (0.14)	-12.32 (0.15)
λ_2	-13.05 (0.15)	-13.67 (0.15)	-16.13 (0.16)	-15.27 (0.15)
λ_3	18.18 (0.15)	18.80 (0.15)	22.44 (0.16)	20.47 (0.15)
λ_4	18.31 (0.15)	19.12 (0.15)	15.57 (0.14)	18.09 (0.15)
$\beta_F = 0.5$				
λ_1	-11.84 (0.14)	-12.78 (0.15)	-9.09 (0.13)	-12.29 (0.14)
λ_2	-11.99 (0.15)	-13.01 (0.15)	-16.39 (0.16)	-15.47 (0.16)
λ_3	18.00 (0.15)	19.10 (0.15)	22.58 (0.16)	21.06 (0.16)
λ_4	17.52 (0.14)	18.82 (0.15)	15.03 (0.14)	17.90 (0.14)
$\beta_F = 0.7$				
λ_1	-11.48 (0.14)	-13.26 (0.15)	-8.81 (0.13)	-12.29 (0.15)
λ_2	-11.49 (0.14)	-13.28 (0.15)	-16.91 (0.16)	-15.76 (0.16)
λ_3	17.82 (0.14)	18.52 (0.15)	21.78 (0.16)	20.45 (0.15)
λ_4	17.42 (0.14)	18.88 (0.15)	14.83 (0.13)	17.77 (0.14)

Note: RB = Relative Bias, RMSE = Root Means Squared Error (of parameter estimates), bolded values indicate RB larger than 10%. Results reflect average RB and RMSE values over 1000 replications with a simulated sample size of $N=500$. Strong Uniform indicates $\beta_s = 0.3$ for all indicators; Weak Uniform indicates $\beta_s = 0.1$ for all indicators; Strong Non-uniform indicates $\beta_s = 0.3$ for indicators 1 and 3 and zero elsewhere; Weak Non-uniform indicates $\beta_s = 0.1$ for indicators 1 and 3 and zero elsewhere; Population Factor Loadings were $\lambda_1 = \lambda_2 = 0.5$; $\lambda_3 = \lambda_4 = 0.7$