Dyadic Measurement Invariance and Its Importance for Replicability in Romantic Relationship Research

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Comparisons of group means, variances, correlations, and/or regression slopes involving psychological variables rely on an assumption of measurement invariance—that the latent variables under investigation have equivalent meaning and measurement across group. When measures are noninvariant, replicability suffers, as comparisons are either conceptually meaningless, or hindered by inflated Type I error rates. We propose that the failure to account for interdependence amongst dyad members when testing measurement invariance may be a potential source of unreplicable findings in relationship research. We develop fully dyadic versions of invariance-testing in an Actor-Partner Interdependence Model framework, and propose a Registered Report for gauging the extent of dyadic (non)invariance in romantic relationship research.

Data, analysis code, supplementary material: <https://osf.io/aw4sr/>

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The scientific study of romantic dyads has evolved in short order from a relatively niche and straightforward topic, to a booming subfield of interdisciplinary social science that utilizes increasingly sophisticated methods and data analysis strategies. In the shift from oversimplified analyses of individuals in romantic dyadic contexts (e.g., Hill, Rubin, & Peplau, 1976; Rubin, 1970) to the more complex relationship science of romance that we know today (e.g., Finkel & Eastwick, 2008; Mancosu & Vezzoni, 2018; Muise, Christofides & Desmarais, 2009), the importance of the development and dissemination of dyadic data analysis models (Kenny, Kashy, & Cook, 2006) cannot be overstated. By enabling researchers to appropriately account for the interdependence of observations within a dyad, analytic paradigms like the Actor-Partner Interdependence Model (APIM; Cook & Kenny, 2005) and the Common Fate Model (CFM; Ledermann & Kenny, 2012) have become mainstays of basic relationship science. It also seems as though every year there are

new extensions to these models (e.g., Gistelinck, Loeys, Decuyper, & Dewitte, 2018; Ledermann, Macho, & Kenny, 2011; Loeys & Molenberghs, 2013) or entirely new models of dyadic processes (e.g., Ledermann & Macho, 2014; West & Kenny, 2011) made available, empowering researchers to ask and to answer new questions about romantic relationships. It is an exciting time to be a close relationships scholar.

Yet amidst the excitement of continued methodological development in relationship science, the replication crisis looms large (for reviews see Nelson, Simmons, & Simonsohn, 2018; Spellman, 2015; Vazire, 2018), and its impact is already being felt within our subfield (Campbell, Loving, & Lebel, 2014; Cheung et al., 2016; Joel, Eastwick, & Finkel, 2018). It is surely the case that many well-trodden areas of concern for replicability in general psychology ought to be of concern for relationship science as well (e.g., John, Loewenstein, & Prelec, 2012; Maxwell, 2004; Rosenthal, 1979; Schimmack, 2012). In the current paper, however, we pursue a different methodological issue yet to be substantively investigated in the context of the replication crisis for either general psychology or relationship science (see Fried & Flake, 2018, for a discussion). Namely, we consider the issue of measurement invariance (for a review see Vandenberg & Lance, 2000) and its importance to dyadic data modeling. Dyadic measurement invariance refers to whether it can be reasonably said that a researcher has

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assessed and analyzed the same latent variables for members of the same dyad. We believe dyadic measurement invariance is a core untested—and in some cases, likely faulty—assumption that is essential to the statistical conclusion validity (Shadish, Cook, & Campbell, 2002) of the various within-dyad comparisons, and substantive theory-testing process (Meehl, 1990a, 1990b; Sakaluk, 2019) upon which much of the romantic relationship literature is based.

In the remainder of the article, we advance the argument that although measurement invariance is of some concern in the context of the broader replicability crisis, we think it is likely a uniquely problematic and impactful issue that undermines the replicability of research on romantic dyads; namely, because dyadic invariance is not usually tested with a fully interdependent analytic approach. We first outline the conceptual issue of measurement invariance and its application as it is most frequently discussed: in the context of between-group comparisons. Using the APIM as an exemplar framework, we then describe how measurement invariance can be imported into the sphere of dyadic data analysis with dependent observations. Acknowledging that many readers may be unfamiliar with this approach, we use the process of testing longitudinal measurement invariance within a latent panel model (Little, 2013) as a source of analytic inspiration. We also briefly consider three motivating rationales for increasing dyadic measurement invariance testing, despite the required (and additional) analytic complexity. Throughout this process, we provide path model visualizations and accompanying analytic code in order to concretize the process of testing dyadic measurement invariance within APIMs, using a variety of scale-setting and model identification configurations. And finally, we conclude with a proposal for a Registered Report in which we would investigate the state of dyadic measurement invariance across a range of relational constructs assessed within the collaborative data collection efforts underlying this special issue.

What Is Measurement Invariance?

In Concept

Measurement invariance refers to whether the accuracy of a measuring device is influenced by variation in conditions unrelated to the construct of interest. In psychological research, the constructs of interest are typically abstract and unobservable latent variables, however, Millsap's (2011) analogy to physical measurement is a helpful starting place. Imagine we have a scale designed to measure weight,

but its readings depend on an object's shape as well as its weight. If equally weighted but differently shaped objects are consistently assigned unequal values, we would say that the scale produces biased measures of weight as a function of shape. In other words, the scale violates the assumption of measurement invariance.

Extending this concept to the measurement of latent variables, we would say that an assessment violates the assumption of measurement invariance if it produces biased measures based on an unrelated group characteristic. More explicitly, measurement invariance across groups implies the following three things: 1) that the latent variable(s) of interest have the same conceptual meaning in each group; 2) that, regardless of group membership, the latent variable varies in the same systematic way; and 3) that external factors influence the latent variable in the same way across groups (Vanberg & Lance, 2000). For example, if a measure of relationship satisfaction is invariant across members of romantic heterosexual dyads, it should tap into the same abstract concept in men and women, and a score of X should represent to the same level of relationship satisfaction for both men and women. Furthermore, if daily stressors decrease women's relationship satisfaction by increasing their perceptions of tension within their relationship, then a similar influence of daily stressors should be seen on men's relationship satisfaction (Bodenmann, Ledermann, & Bradbury, 2007). If these assumptions are met, then finding that men consistently score higher on relationship satisfaction than women can be interpreted as a meaningful group difference in the latent variable. If these assumptions are violated however, it is unclear whether group differences indicate meaningful variation in relationship satisfaction or that the measure is biased by group membership.

Measurement invariance is closely related to other psychometric properties such as validity and reliability. Violation of measurement invariance necessarily implies that the measure has poor validity (i.e., it is not capturing what it purports to be) for one or more groups. Reliability, however, is neither necessary nor sufficient for measurement invariance (Millsap, 2011). A measure that consistently produces similarly biased scores due to group membership can be said to have high reliability but not measurement invariance. In contrast, a measure that captures the same latent variable across groups but produces highly irregular scores over repeated assessment would be deemed unreliable yet invariant across groups. Consequently, the goal in assessing measurement invariance is to determine whether a measure has equivalent validity across groups. In the case of a physical measure such

as our scale example, the validity across groups could be assessed by comparing repeated measurements from the scale to those obtained from a gold standard (and therefore presumably more accurate, although likely more expensive and time-intensive) measure of weight. With latent variables, however, this strategy is typically not possible. Repeated administration of the same measure of a psychological latent variable is often neither practical (due to cost, time, and placing undue burden on the participant) nor useful (testing effects may change how the participant responds across administrations), and truly unbiased gold-standard measures rarely exist (Millsap, 2011). Consequently, statistical procedures have been developed to determine whether a latent variable satisfies the assumptions of measurement invariance.

In Application

Measurement invariance can be tested in a number of statistical frameworks (i.e., item-response theory, structural equation modeling, and a combination of the two; see Meade & Lautenschlager, 2004 for an overview), however, this paper focuses on the structural equation modeling (SEM) approach as it is likely the most readily accessible to all readers. We will describe the three main levels of measurement invariance (i.e., configural, loading, and intercept; Widaman & Reise, 1997) and how they are tested using confirmatory factor analysis (CFA) and a series of nested model comparisons. Although additional steps (or levels) to invariance testing exist (see Meredith, 1993; Vandenberg & Lance, 2000), we focus on the three that are required for comparing the magnitude of actor and/or partner effects, as well as comparing partner means on the predictor and/or criterion variable(s). For illustrative purposes, we will use a hypothetical measure of attachment insecurity (Wei, Russell, Mallinckrodt, & Vogel, 2007; Sibley, Fischer, & Liu, 2005; Brennan, Clark, & Shaver, 1998) as an example throughout this section. We will assume that this hypothetical measure is a six-item questionnaire capturing two correlated latent variables, *anxiety* and *avoidance*, and that three continuous items load onto each factor (Figure 1). In Figure 1, factor loadings (i.e., slopes of the items regressed onto their respective latent factor(s)) are represented by $\lambda_{i1} - \lambda_{i6}$ (for $i = 1, 2$), item intercepts (i.e., intercepts of item values when the level of the respective latent factor(s) is 0) by $\tau_1 - \tau_6$, and item residual variances (i.e., unique true-score variability of other unmodeled factors and error) by $\theta_{11} - \theta_{66}$. In this hypothetical example, the goal is to test whether the measurement models for attachment *anxiety* and

avoidance are invariant across male and female partners in heterosexual romantic dyads, so that we can conduct valid comparisons of structural parameters (e.g., comparing men and women's mean *anxiety* scores, or comparing the correlation between *anxiety* and *avoidance* for men and women).

Configural invariance. Configural invariance is the least restrictive level of equivalence testing and can be thought of as a test of *model form* across groups (Putnick & Bornstein, 2016). In other words, configural invariance tests whether *the pattern of item loadings* is consistent in both groups of interest. In our example, this is equivalent to seeing whether the same items load onto the same latent variables for both men and women (i.e., items 1-3 onto *Anxiety* and items 4-6 onto *Avoidance*), without any idiosyncratic cross-loadings or omitted loadings. If this model displays acceptable overall fit (see Putnick & Bornstein, 2016, and Chen, 2007 for discussions of model fit indices), we say that the measure meets the assumption of configural invariance and this becomes our baseline model moving forward. Insufficient model fit, however, would imply that the organization of latent factors and items differs between men and women and the measure is noninvariant across groups.

Loading invariance. The next level of measurement invariance, *loading* (also referred to as *weak* or *metric*) *invariance*, assesses whether the item loadings are equivalent across groups. In our example, achieving loading invariance, not only implies that the pattern of item loadings is consistent across men and women, but also that the *items are comparably associated with their respective latent factors by the same degree in both groups* (Putnick & Bornstein, 2016). If this is the case, we can conduct meaningful tests comparing the magnitude of latent variances, covariances (correlations), and regression slopes between groups (Little, 2013).

To test for metric invariance, item loadings are constrained to be equal across groups (e.g., fixing $\lambda_{11} = \lambda_{21}, \dots, \lambda_{16} = \lambda_{26}$), and overall model fit is compared to that of the unconstrained baseline model (i.e., the model for configural invariance). Evaluating the reasonableness of the loading invariance model (and the levels of invariance thereafter) involves comparing its fit to the baseline model that preceded it. Here, the process of comparing model fit could be approached in a number of ways. Traditional likelihood ratio tests (LRTs; or χ^2 difference tests) can be used to determine if the added constraints of loading invariance significantly degrade the fit of model beyond the configural invariance model (Little, 2013), but this method of invariance testing is limited by the concerns

of sample size sensitivity that undermines the utility of the basic χ^2 test of model fit (Brannick, 1995). Other methodologists (e.g., Chen, 2007; Cheung & Rensvold, 2002) have therefore recommend that researchers examine whether indexes of absolute (or relative) model fit (see Hu & Bentler, 1999) such as the RMSEA (or CFI) increase (or decrease) beyond some determined threshold, such as > 0.015 (or > 0.01), which would be taken as evidence of an unacceptable level of model fit degradation.

By whichever standard(s) of model fit a researcher selected, if the fit of the nested weak invariance model is not appreciably worse than the configural invariance model, we would conclude that the measure has met weak invariance across groups, and this model would become our new baseline for comparison. If the overall fit of the weak invariance model was significantly worse, however, this would imply that at least one of the item loadings was not equivalent across groups. Successively unconstraining one loading at a time and assessing the overall fit of each partially constrained model could determine which items were noninvariant. If acceptable model fit was achieved for a partially constrained model, partial loading invariance would be met and this would become the new baseline for comparison.

Intercept invariance. If loading (or partial loading) invariance has been met, the assumption of *intercept* (also referred to as *scalar* or *strong*) invariance can then be tested. Intercept invariance means that the items are expected to have the same predicted value when their corresponding latent variable(s) take on a value of zero. If this is the case, we can conclude that latent factor means are capturing the same amount of variation in each group, and the between-group mean differences are interpretable (Putnick & Bornstein, 2016). Intercept invariance is tested by keeping the factor loadings constrained as they were in the loading (or partial loading) invariance model, and further constraining the item intercepts to be equal across groups (e.g., fixing $\tau_1 = \tau_1, \dots, \tau_6 = \tau_6$). If only partial invariance was achieved at the previous level, the intercepts of items with unconstrained loadings are also allowed to be freely estimated. This more restrictive model is then compared to the previous baseline model and assessed for overall model fit. In our example, finding that the fit of the intercept invariance model was not significantly worse than that of loading invariance model would imply that scores for *anxiety* and *avoidance* have the same meaning for men and women, and that differences in variances, covariances (and correlations), regression slopes and means between groups reflect true differences in the latent variables (as opposed to differing because of nuisance

differences in measurement and test construction). As in the case of loading invariance, if the more constrained model displays significantly worse fit than the previous loading (or partial loading) invariance model, partial intercept invariance can be achieved by progressively unconstraining item intercepts (Putnick & Bornstein, 2016) until sufficient overall model fit is reached. Whether full or partial intercept invariance is achieved, this model would become the new baseline as we move on to comparisons of latent means.

Implications for Replicability

The implications of measurement practices for the replicability of substantive tests of theory are only beginning to be made explicit (Fried & Flake, 2018). Nevertheless, there is already a compelling literature to suggest that the failure to establish invariance should lead to unreplicable findings. Indeed, concurrent with contemporary replicability discourse, generalist (Guenole & Brown, 2014), specialist (Putnick & Bornstein, 2016; Schmitt, Golubovich, & Leong, 2011; Chen 2008), and methodology-focused journals (Masyn, 2017; Steinmetz, 2013) have published simulations evidencing the impact of noninvariance for tests of structural parameters.

As the failure to achieve configural invariance renders comparisons of structural parameters (e.g., means, variances, correlations, regression slopes) largely meaningless, configural noninvariance likely poses the most obvious threat to replicability. In the face of configural noninvariance, researchers could still force a comparable structural model across groups (or, equivalently, simply average or sum across items from a factor), and more than likely find some comparisons with significant *p*-values; however, comparing structural parameters in this case would amount conceptually to comparing “apples to spark plugs.” (Vandenberg & Lance, 2000, p. 9) For example, with our hypothetical attachment measure, should a unidimensional model of *anxiety* fit reasonably well for women, but the pattern of loadings differ appreciably for men (e.g., a two-factor model of *anxiety* with multiple cross-loadings), there is little to be learned--or replicated--from treating men and women as if they have the same conceptualization of attachment when testing for gender differences in the effects of attachment anxiety.

Moving further up the ladder of invariance testing, should configural invariance between groups be established, but loading and/or intercept invariance fail, then there is still a negative impact on future replicability. Specifically, when loading and/or intercept invariance are not tenable, error rates for comparisons of structural parameters “can greatly

deviate from the nominal $\alpha = .05$,” (Masyn, 2017, p. 187) resulting in “bogus interaction effects” and “pseudo group differences” (Chen, 2008, p. 1005). The extent of this biasing depends on a number of factors, including the measurement parameter type (loading vs. intercept), the structural comparison type (e.g., mean differences vs. regression slope differences), measurement model complexity, the extent the magnitude of noninvariance, and the extent of partial invariance, amongst others (Chen 2008; Guenole & Brown, 2014; Masyn, 2017; Putnick & Borenstein, 2016; Schmitt et al, 2011; Steinmetz, 2013). In the field of close relationships, the impact for ignoring variability in measurement on the replicability of findings is therefore not a question of “if?”, but rather of “how much?”

Extending Invariance Testing to the Dyadic Context

Measurement invariance is a tacit assumption that pervades many of the statistical comparisons dyadic analysts would wish to make. Take, for example, a generic (non-latent) APIM with distinguishable members (Cook & Kenny, 2005) predicting Y from X (see Figure 2). A researcher modeling this pattern of dyadic interdependence might not only be interested in estimating separate actor ($a1$, $a2$) and partner effects ($p1$, $p2$), but also in: (1) comparing actor and/or partner effects (e.g., are actor effects comparable between two dyad members?); and (2) comparing partners’ standings on either X and/or Y . The validity of these comparisons relies doubly on the soundness of measurement invariance, as invariance must be present for both X and for Y variables.

In comparing analytic strategies for dyadic modeling, Ledermann and Kenny (2017) offer the ability to evaluate measurement invariance as one of the strengths of a structural equation modeling (SEM) approach. Further espousing the virtues of SEM for fitting dyadic models like the APIM, Ledermann and Kenny (2017) point to the example of South, Krueger, and Iacono (2009), in which the authors, “...estimated a CFA model to test invariance across partners for Spanier’s (1976) Dyadic Adjustment Scale.” (p. 448) Upon closer inspection, however, the invariance tests conducted by South and colleagues (2009) appear to ignore the interdependence in observations among dyad members. From the description of their analytic approach:

To account for possible non-independence of the data (i.e., both spouses are reporting on the same marriage), we modeled the data at the couple level

(both husbands and wives were on the same line of data). The factor model for husbands and wives was connected through a correlation between the higher order latent factors. (South et al., 2009, p. 624).

There are two misunderstandings in this description worth clarifying. The first is that using a couple-level data structure somehow accounts for the dependency among observations between members of a dyad. Although a dyad-level dataset is the necessary data structure for specifying an APIM in an SEM framework (Kenny et al., 2006)—it does nothing to directly model the interdependence. Similarly, it is a misconception that correlating the latent factors across partners would address the dependency among observations between partners. This strategy is no more effective at accounting for dependency than correlating X terms in an APIM would. Instead, interdependence requires the correlating of *error terms* (both at the measurement model and structural model levels) between partners, in order to appropriately specify.

This process of specifying dependency is generally identical to how dependency is specified in latent longitudinal models (see Little, 2013), in which error terms for repeated assessments of the same items for a given factor are correlated (e.g., the first item assessing *anxiety* at Time 1, Time 2, and Time 3, in a hypothetical 3-wave longitudinal study of attachment). And likewise, just as invariance can (and should) be tested between groups, so to can (and should) it be tested across time—or other dependency-introducing design features, like dyadic data—in order to ensure the latent variable(s) under consideration retain their meaning, and that structural comparisons are thereby meaningful.

Dyadic Invariance Modeling

After accounting for the interdependency of observations by correlating the appropriate measurement-model error terms (e.g., θ_{11} and θ_{55} , θ_{22} and θ_{66} ...) and structural-model error terms (i.e., Ψ_{33} and Ψ_{44}), researchers would then incrementally increase constraints on measurement parameters in the normal invariance testing sequence (Putnick & Bornstein, 2016). Figure 3 - Figure 5 provide exemplar path diagrams for the appropriate specification of dyadic configural, loading, and intercept invariance.

In specifying a properly interdependent latent APIM—and so long as the pattern of observed variables loading onto latent variables remains the same for both partners—a researcher will have already imposed dyadic configural invariance (Figure 3). Researchers would then evaluate the fit of this model

based on the usual criteria (Hu & Bentler, 1999; Bollen & Long, 1993), and provided acceptable fit, it would become the initial baseline model for subsequent dyadic invariance testing models.

By building off of the dyadic configural model to also constrain loadings for the same item across partners to equivalency, Hong and Kim (2018) appear to provide one of the first examples of properly testing dyadic loading invariance (see their Model 3). If the constraints of the dyadic loading invariant model were reasonable when compared to the dyadic configural invariance model, researchers would use this model as the new baseline model. Specifically, this model would serve as a basis for valid comparisons of latent actor and/or partner effects between dyad members, and for the subsequent test of dyadic intercept invariance. Dyadic intercept invariance, finally, would require researchers to equate intercept terms for the same items across partners, and this model would serve as a baseline for valid comparisons of latent means on the X and Y variables between partners.

Why Bother?

It is fair to say that the implementation of properly specified dyadic invariance testing will increase analytic demands on scholars who rely on dyadic analyses. Even so, we can think of three primary reasons why there is value in rising to the analytic challenge of implementing properly specified dyadic invariance models in our field.

Interdependence matters. Some readers might view our call to increase dyadic invariance modeling as overburdensome and unnecessary in light of the extent to which close relationship researchers already evaluate invariance using multi-group confirmatory factor analysis approaches (e.g., Karantzas, Feeney & Wilkinson, 2010; Maxwell et al., 2017; Schrodt et al., 2009; Whisman & Li, 2015). Although we agree it is a good thing that relationship researchers have made efforts to establish the invariance of the measurement structures upon which they rely, we have already described how this process fails to take the interdependent nature of dyadic data into account during the invariance testing process. If analytic errors are made in the process of testing dyadic invariance, then the lesser-worst-case scenario is the Type I error rates of subsequent structural tests will be inflated (McCoach & Adelson, 2010). On the other hand, the worst-worst case is that fundamentally different latent variables are involved, and these structural tests will be entirely meaningless (Vandenberg & Lance, 2000). We

should not be surprised when previous effects beset by either possibility fail to replicate.

Beyond the negative impact on replicability, ignoring dependency in the process of invariance testing is also at-odds with the zeitgeist of data analysis in close relationships research. Indeed, dyadic data analysis methods rose to prominence not simply because dependency presents a statistical nuisance that must be addressed (as it certainly does), but rather, because enough researchers in the field found matters of interdependence theoretically interesting in their own right. If one were to list some of the central assumptions researchers in our field are comfortable making, that interdependent contexts change patterns of thought, feeling, and behavior would have to top the list. Continuing to evaluate invariance in measures with methods that ignore dependency is therefore not only statistically ill-advised (and ultimately compromising of replicability), it is also incongruent with the fundamental ways in which our field understands relational psychology and behavior. If the dyadic context matters for how people experience the world, and modeling interdependence matters for the valid estimation of dyadic effects, then there is no escaping how dyadic context and modeling interdependence should also matter for evaluating measurement invariance among dyad members.

The default of distinguishability. The typical ways in which close relationship researchers model their dyadic data—their analytic norms—present another reason for the adoption of dyadic invariance testing. Dyadic models like the APIM present researchers with a choice: to model the members of dyads as either *indistinguishable* or *distinguishable* from one another in some important way. In the context of research on heterosexual romantic couples, gender/sex (see van Anders, 2015 for the meaning of this term) is easily the most common distinguishing variable used, such that separate levels of X and Y , and separate actor and partner effects (along with other parameters) are estimated for men and women. Although numerous strategies are available for statistically testing distinguishability of actor-partner effects (Olsen & Kenny, 2006; Gistelinck, Loeys, Decuyper, & Dewitte, 2018), it is safe to say—at least in the context of dyadic studies on heterosexual romantic relationships—that the majority of researchers anticipate that men and women will somehow be different in terms of the variables of interest and the patterns of association between them, and without always testing this assumption directly, default to modeling dyad partners as distinguishable (cf. Kenny et al., 2006). But if dyadic researchers are so bullish on the prospect of differences between distinguishable members of a dyad, why

would they be bearish on measurement structures, and presume dyad partners (e.g., men and women) to have entirely similar ways of conceptualizing the processes researchers are studying? If anything, it would be more congruent for dyadic researchers to likewise anticipate that distinguishable dyad members have some non-negligible differences in their meanings of psychological and relational concepts.

Incorporating regular dyadic invariance testing would therefore help to bring dyadic researchers' analytic norms into better alignment with the ways in which they think about measurement, resulting in a more coherent process of theorizing and analyzing dyadic interdependence. Should dyadic invariance testing reveal frequent and/or important differences in measurement and conceptualizations between dyad members, it would complicate how dyadic researchers would need to proceed to test their structural parameters of interest. However, it would also supply dyadic researchers with a new kind of outcome of interest, as differences in measurement needn't be thought of exclusively as a mere analytic annoyance, but could—and should—be viewed as a substantively meaningful analytic outcome in its own right (see Sakaluk, 2019).

Capacity for change. Our final reason in motivating for increased dyadic invariance testing reflects our optimism in the methodological capacity of the field of dyadic researchers. Although barely 10 years old, contemporary cross-sectional models of dyadic interdependence (Kenny et al., 2006) have already undergone considerable revision and expansion. All the while, the field of dyadic researchers have kept pace. Researchers incorporating dyadic tests of moderation (Campbell & Kashy, 2002) and/or mediation (Ledermann, Macho, Kenny, 2011), and/or analyzing patterns of dyadic effects in complex longitudinal designs (Bolger & Laurenceau, 2013) are not the exception; they are the rule, in spite of the added complexity these analyses demand. And so, while we continue to acknowledge the added burden and complication that properly implementing dyadic invariance models will require, we simultaneously see there being considerable benefits to the credibility of our research in doing so, and believe that the field of dyadic researchers are up to the challenge.

Locating Dyadic Variability in Measurement

Past research hints that dyadic researchers are not only up to the challenge but acutely interested in considerations of dyadic invariance and noninvariance between men and women in romantic relationships.

Considerable empirical attention has been given to the examination of gender differences in both the subjective meaning and experience of various relational constructs such as marital quality, communication, and romantic jealousy, among others (e.g., Beam et al., 2018; Wheeler, Reis, & Nezlek, 1983; Tan, Jarnecke, & South, 2017; Zandbergen & Brown, 2015). Moreover, assumptions of noninvariance between men and women are inherent in the scientific discourse surrounding 'his and her' marriages (e.g., Kiecolt-Glaser & Newton, 2001; Stokes, 2017). Thus, it would appear that the time is ripe for dyadic researchers to put well-established and commonly-used measures of romantic relationship processes to the test in a dyadic framework. Specifically, we propose that dyadic invariance testing has the potential to enrich and extend scientific understanding and measurement in three key domains: *relationship well-being* (e.g., relationship quality, commitment, satisfaction), *relationship personality* (e.g., attachment, communal strength), and *sexuality* (e.g., sexual motivation, sexual satisfaction). In the following sections, we provide brief examples of why and how dyadic invariance may be particularly important and/or (un)likely in each of these domains of romantic relationship measurement.

Relationship Well-Being

Often-used measures of relationship quality such as the Perceived Relationship Quality Components (PRQC) Inventory (Fletcher, Simpson, & Thomas, 2000) typically rest on the assumption that romantic relationship quality is composed of the same components (e.g., love, trust, intimacy, satisfaction, commitment, and passion) for both men and women alike, and that each of these components contribute similarly to men and women's relational well-being. Yet, when evaluating the quality of their relationships, men have been found to place greater value on the role of passion than do women (Fehr & Broughton, 2001). Conversely, women tend to place greater emphasis on commitment and intimacy in a relationship (Sprecher, 2002; Sternberg, 2006). These findings suggest that men and women may differ in their subjective assessments of relationship quality.

Relationship Personality

Dyadic measurement invariance is also an important consideration for measures of relationship-related elements of personality. For example, communal strength is conceptualized as the degree to which an individual is motivated to be responsive to a partner's

needs (Mills, Clark, Ford, & Johnson, 2004). Critically, however, traditional gender roles prescribe that women should be more communal and relationally-focused than men and that men should be more agentic and individually-focused than women (e.g., Eagly & Wood, 1999). Men and women's motivations to be responsive to their partners' needs are likely shaped by these broader gender-role expectations. Indeed, women do tend to be more relationally-focused (e.g., Cross & Madson, 1997), as well as more attuned and responsive to the emotional needs of their partners than men (Croyle & Waltz, 2002; Neff & Karney, 2005). Thus, what it *means* to be a highly responsive partner may shift depending on whether one is a man or a woman. Consequently, a man who is highly responsive to the emotional needs of his partner may demonstrate higher communal strength relative to other men, yet he may still fall short when compared to the communal strength of the average woman.

Sexuality

Perhaps the most obvious case for the necessity of testing for dyadic measurement invariance comes from the literature highlighting various gender differences in sexual motivation and sexual satisfaction. For example, in long-term relationships, women's primary motivations for engaging in sex tend to be emotional whereas men's primary motivations tend to be physical (e.g., Armstrong & Reissing, 2015; Vohs, Catanese & Baumeister, 2004). Accordingly, women tend to report a wider range of motivations for engaging in sex compared to men (Stephenson, Ahrold, & Meston, 2011). Women's motivations for engaging in sex are also more likely to change over time whereas men's motivations tend to remain stable across the life course (Gebauer, Baumeister, Sedikides & Neberich, 2014). These findings call into question whether invariance in both the structure and nature of men and women's sexual motivations can be reasonably assumed.

Men and women also appear to differ in how they conceptualize and evaluate their own sexual satisfaction. For instance, men tend to report experiencing greater pleasure from sexual activities such as penile-vaginal intercourse and giving/receiving oral sex compared to women (e.g., Haavio-Mannila & Kontula, 1997; Galinsky & Sonenstein, 2011; Holmberg & Blair, 2009; Pinkerton, Cecil, Bogart & Abramson, 2003). Yet, other research has found that women consistently report higher sexual satisfaction than men (e.g., Sprecher, 2002). Upon first pass, these findings might seem contradictory. However, one possible explanation for this seeming contradiction is that men and women may employ a different set of

criteria when evaluating their sexual satisfaction. In support of this account, men report that achieving orgasm is of primary importance to their sexual satisfaction whereas women consider achieving orgasm to be more of a 'bonus' rather than the primary determinant of their sexual satisfaction (Salisbury & Fisher, 2014, p. 620).

Summary

Ultimately, the above examples serve mainly to highlight what close relationship researchers already seem to intuit: that dyadic invariance in measures of relationship well-being, relationship personality, and sexuality cannot and should not reasonably be assumed. As for those who remain skeptical, we hope these examples will raise even the slightest of doubts about the tenability of dyadic invariance in relational domains and, in doing so, demonstrate that at the very least dyadic invariance warrants further consideration. Above all, we hope these examples have demonstrated that dyadic researchers are perfectly poised to consider and tackle issues of dyadic invariance within their own research. Doing so will improve the credibility of our science by empowering researchers to draw more meaningful, and more accurate, inferences about dyadic processes.

The Proposed Registered Report

When measures are noninvariant, inferential comparisons of dyadic structural coefficients like actor and partner effects are either biased or conceptually meaningless. As we have argued, properly specified tests of dyadic invariance have largely been absent from the close relationship literature (cf. Hong & Kim, 2018), and therefore, a crucial auxiliary theory of research on romantic relationships has gone untested (Fried & Flake, 2018; Meehl, 1990a, 1990b; Sakaluk, 2019). Dyadic variability in measurement may therefore serve as a potent and wide-spread undermining factor of the replicability of close relationship research.

The dyadic data collection facilitated by the Editors of the *Special Issue* presents a rare opportunity to gauge the extent to which measurement models in heterosexual romantic relationships are invariant between distinguishable partners, across a range of latent variables of broad interest to the field. We therefore propose to test dyadic *configural*, *loading*, and *intercept* invariance in APIMs across measures used in the shared dyadic data sets of the *Special Issue*.

Proposed Methods

Participants

Participants will be members of romantic couples recruited via the *Editors* collaborative data collection efforts of ten dyadic samples for the *Special Issue*, collected via Qualtrics Panels. Since dyadic sample sizes will vary from invariance test to invariance test—as in cases where the identical measure (e.g., a common measure of relationship quality) is present in multiple samples—we will conduct our invariance tests combining across these samples. Minimum sample sizes for dyadic invariance tests will be at least 200 spousal dyads. Although statistical power requirements for testing invariance in the dyadic context are not well understood, invariance tests involving at least 400 distinguishable members would be well above the norm for typical recommendations of sample sizes needed for invariance testing in other analytic contexts involving dependent observations (see Little, 2013).

Measures

The available measures will largely be dictated by the interests of those granted one of the dyadic samples collected by the *Editors*. However, given the collection of data from romantic dyads, we have identified types of measures—and specific examples—that we would find interesting and important for the purpose of evaluating dyadic invariance (see Table 1). We will also test invariance for any measure identified as central to a research team's dyadic analysis, even if that measure(s) falls outside the scope of the three broad types of measures that we have identified.

Data Analysis Strategy

Measurement models for invariance will be determined either based on available measurement-related papers for the measures in question, or based on researcher scoring decisions (e.g., we would model all items as loading onto a factor that a researcher described as averaging together into one composite); we think that in the majority of cases, it is likely that both strategies will be aligned in their implications for the measurement model we specify. All invariance analyses will be conducted in *R* (R Core Team, 2018) using the *lavaan* (Rosseel, 2012) and *semTools* (Jorgensen, Pornprasertmanit, Schoemann & Rosseel, 2018) packages. When indicator variables have more than four ordered response options—a case we anticipate for the vast majority of latent variables we analyze—we will estimate our models using a maximum likelihood estimator that is robust to non-normality (*mlr*; see Bischl, Lang, Kothof, Schiffner,

Richter, Studerus, Casalicchio & Jones, 2016;). When indicator variables have fewer than four ordered response options, or involve nominal responses, we will use a categorical estimator that is robust to non-normality (*ULSMVS*; see Rhemtulla, Brosseau-Liard, & Savalei, 2012). Finally, whenever possible, we will use full-information maximum likelihood to handle missingness in participant responses, and thereby minimize estimation bias and lost statistical power (Enders, 2010).

All invariance tests will be conducted within a latent APIM framework (as in Figures 3-5). Other dyadic models can be fit (e.g., Ledermann & Kenny, 2012) to the same kind of dyadic data, using latent variable analyses, and may be preferred among researchers testing other substantive questions. The matter of invariance, however, remains an important assumption and auxiliary theory baked into *any* dyadic model, and the APIM presents a straightforward means of evaluating it. Given the APIM's relative popularity, we will pursue this formulation of dyadic invariance testing to achieve a consistent invariance testing strategy across measures.

Three final interrelated analytic details require a more nuanced unpacking: (1) the distinction between measurement model adequacy and configural invariance; (2) the matter of evaluating model fit; and (3) the matter of managing model complexity. Regarding the first matter, as Jorgensen and his colleagues (Jorgensen, Kite, Chen, & Short, 2017) have aptly identified, the traditional way of evaluating the reasonableness of configural invariance conflates the of process configural invariance testing with that of assessing the fit of the underlying model, irrespective of invariance. As methods for disentangling the two issues are only just emerging, it is important to acknowledge that our proposed investigation will be limited in this way too. In other words, the measures we evaluate for dyadic invariance may fail the configural invariance check not because the factor structure is different for one member of the dyad or the other, but simply because the measurement model may not be well-fitting for *either* dyad.

Regarding the second matter, and in response to the conflation of basic model fit and configural invariance tenability, we propose descriptively interpreting the comparability of factor solutions between dyad members, *even when the fit of the overall model is not acceptable*. Comparisons of stricter levels of invariance (e.g., dyadic loading invariance) to baseline models (e.g., dyadic configural invariance) will rely on interpretations of changes in RMSEA and CFI (Chen, 2007; Cheung & Rensvold, 2002), although we will also note when LRTs indicate a significant decrease in

fit as well. Although a measure with a generally poor-fitting model should also not be expected to produce replicable findings, this process will help to distinguish between two potential sources of unreplicable findings: poor fitting measurement models in general, and differing measurement models between dyad members.

Finally, regarding the second matter, in some cases the measures used by researchers published in the *Special Issue* may be quite elaborate, with many items loading onto many factors. In some cases, exceptionally complex models can cause estimation problems, or yield poor fit simply because the underlying measurement model is not sufficiently parsimonious (Brown, 2015). In cases such as these, our analytic contingency plan is to create a maximum of five parcel-indicators per factor (Little, Cunningham, Shahar, & Widaman, 2002), using the bias-minimizing random item-to-parcel allocation strategy (Sterba, 2011; Sterba & MacCallum, 2010). The resulting measurement model would be over-identified, providing us some (less generalizable) ability to evaluate general model fit, while also enabling us to test the subsequent levels of measurement invariance.

We will report our results in an accessible summary table styled in the manner of Table 2. However, we will also provide a more detailed supplementary table of results for all dyadic invariance tests (along with our reproducible analytic syntax), including all individual model fit indexes, LRTs, and changes in model fit indexes between levels of invariance.

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Table 1

Summary of Relationship Measure Domains and Example Desirable Measures for Dyadic Invariance Testing

Domain	Scale/Measure	Citation
Relationship Well-Being	Investment Model Scale	Rusbult, Martz, & Agnew, 1998
	Perceived Relationship Quality Components (PRQC) Inventory	Fletcher, Simpson, & Thomas, 2000
	Dyadic Adjustment Scale (DAS)	Spanier, 1976
	Couple Satisfaction Index (CSI)	Funk & Rogge, 2007
	Marital Adjustment Test (MAT)	Locke & Wallace, 1959
Sexuality	Sociosexual Orientation Inventory (SOI)	Simpson & Gangestad, 1991
	Sociosexual Orientation Inventory-Revised (SOI-R)	Penke & Asendorpf, 2008
	Infidelity Dilemma	Buss, Larsen, Westen & Semmelroth, 1992
	Infidelity Dilemma	Buss et al., 1999
	Changes in Sexual Functioning Questionnaire (CSFQ-14)	Keller, McGarvey & Clayton, 2006
	Pinney Sexual Satisfaction Inventory (PSSI)	Pinney, Gerard & Denney, 1987
	Interpersonal Exchange Model of Sexual Satisfaction (IEMSS)	Lawrance & Byers, 1995
	Erotophilia-Erotophobia	Fisher, Byrne, White & Kelley, 1988

	Erotophilia-Erotophobia	Rye, Serafini & Bramberger
	Sexual Systems Functioning Scale (SFSS)	Birnbaum, Mikulincer, Szepeswol, Shaver & Mizrahi, 2014
	Sexual Communal Strength (SCS)	Muise, Impett, Kogan & Desmarais, 2013
Relationship Personality	Experiences in Close Relationships (ECR)	Brennan, Clark & Shaver, 1998
	Caregiving Questionnaire (CQ)	Kunce & Shaver, 1994
	Relationship Closeness Inventory	Mills, Clark, Ford & Johnson, 2004

Table 2

Table of Summary Results from Dyadic Invariance Tests

Measure	Measure Do- main	<i>n</i> dyads/num- ber of sam- ples included	Dyadic Con- figural Invari- ance?	Dyadic Load- ing Invari- ance?	Dyadic Inter- cept Invari- ance?
Example Measure “X”	Relationship Well-Being	400/2	✓	X	X
Example Measure “Y”	Relationship Well-Being	200/1	✓	✓	✓
Example Measure “Z”	Relationship Well-Being	1000/5	✓	<i>P</i>	✓
...	...	...	...	...	...

Note. ✓ = invariance level tenable; X = invariance level not supported; *P* = *partial invariance level tenable*.

Figure 1. A hypothetical two-factor measure of attachment insecurity for heterosexual romantic partners (F_1 = Anxiety, F_2 = Avoidance).

Figure 2. A generic observed-level (non-latent) distinguishable actor-partner interdependence model.

Figure 3. A latent distinguishable APIM with four indicators for X and Y that is configurally invariant between dyad partners.

Figure 4. A latent distinguishable APIM with four indicators for X and Y that is constrained to loading equivalency between dyad partners.

Figure 5. A latent distinguishable APIM with four indicators for X and Y that is constrained to loading and intercept equivalency between dyad partners.