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Using modern methods for missing data analysis with the social relations model: A bridge to social network analysis

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ABSTRACT

Social network analysis identifies social ties, and perceptual measures identify peer norms. The social relations model (SRM) can decompose interval-level perceptual measures among all dyads in a network into multiple person- and dyad-level components. This study demonstrates how to accommodate missing round-robin data using Bayesian data augmentation, including how to incorporate partially observed covariates as auxiliary correlates or as substantive predictors. We discuss how data augmentation opens the possibility to fit SRM to network ties (potentially without boundaries) rather than round-robin data. An illustrative application explores the relationship between sorority members' self-reported body comparisons and perceptions of friends' body talk.

Introduction

The focus of our paper is how the social relations model (SRM; Kenny, 1994; Kenny et al., 2006) can be utilized to model a social relational system in a bounded network. The SRM is traditionally applied to data gathered from a so-called "round-robin design," in which all possible reciprocal perceptions of members in a closed network are recorded. Social network analysis (SNA) typically models the structure of a network comprised of ties between nodes. In this paper, we propose a methodological bridge between SNA and SRM, such that the criterion for recording dyad-level perceptions is whether a directed (or reciprocated) tie between the pair exists. This bridge is built on modern advances in missing-data analysis.

Traditionally, SRM parameters are estimated using random-effects ANOVA to partition a single outcome (Warner et al., 1979) into components associated with the ego,[1] alter, and dyadic relationship. Extensions of the SRM allow ego and alter effects to correlate with other ego or alter characteristics (e.g., Brunson et al., 2016; Kenny, 1994; Kwan et al., 2004). We utilize the multilevel modeling (MLM) framework to fitting the SRM (Snijders and Kenny, 1999), using a flexible Bayesian approach (Hoff, 2005; Lüdtke et al., 2013). One advantage of using Bayesian estimation methods is that missing data can be treated as unknown parameters to be estimated along with the model's fixed and random effects. Although Lüdtke et al. (2013) and Hoff (2005) hinted at this advantage of fitting the SRM in a Bayesian paradigm, the method of fitting the SRM to partially observed data has yet to be developed. We contribute to the SRM literature by (a) elaborating on missing-data mechanisms in the context of the SRM and (b) demonstrating how ignorable missing data can be accommodated using a Bayesian approach. We contribute to SNA literature by demonstrating how (a) perceptions of alters and (b) self-reported characteristics of egos can be modeled simultaneously to answer questions about within-network perceptions. Given the ability to fit the SRM to incomplete round-robin data, we propose that SRM parameters can be interpreted with regard to ties in a social network rather than to round-robin data.

After introducing an extended SRM for partially observed data, we apply it to self-reported body attitudes and body comparison to illuminate the nature of peer-perceptions about body talk. Our investigation explores the association among perceptions of one's peers, peers' perceptions of oneself, and peers' self-report in order

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[1] In previous SRM literature, egos (self) and alters (peer) have been referred to as actors and partners, respectively, in the context of behavioral observations, or as perceivers and targets in the context of interpersonal perception.

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to assess whether perceptions of peers are related more to characteristics of the perceiver (ego) or to characteristics of the peer being perceived (alter). To more fully account for the influence of the ego on perceptions of alters, we also consider the relationship of an ego's perceptions with the ego's weight-related attitudes (i.e., drive for thinness). To date, no publication has explored whether the relationship between an ego's own behavior and her perception of her alters' corresponding characteristics is moderated by her own drive for thinness.

Social network analysis and the social relations model

A traditional goal of SNA is to identify and characterize the structure of a network, typically using graph theory (Wasserman and Faust, 1994). When data are collected from people within a bounded network, individuals (i.e., egos) typically identify connections (i.e., ties) with other network members (i.e., alters), for example, by nominating friends from a list of peers in a classroom. The presence or absence of these ties is meaningful in that these associations describe the type of network (e.g., a friendship network; an advice network), and ties reflect a particular type of relationship between network members. In SNA, ties are often directed, wherein an ego identifying alter(s) as friend(s), which represents the ego's out-degree, and when alter(s) identify ego as a friend it represents the ego's in-degree (Wasserman and Faust, 1994). Each tie can also have a weight (Valente, 2010). This weight could be the strength of the relationship (qualifying the tie as weak or strong) or other information about the link, such as the type of advice or information shared. When ties have weight, it is called valued network data (Valente, 2010). In addition to graphically illustrating the structure of the network (i.e., egos represented as "nodes" and ties represented as links or "edges"), many metrics are used to characterize individuals (e.g., centrality), dyads (e.g., connectivity, reciprocity), groups (e.g., clustering, closure), or the network as a whole (e.g., density, mean vertex degree) (Wasserman and Faust, 1994). Network structure can also be explained or predicted, for instance, using the p_2 model (van Duijn et al., 2004) or exponential random graph models (Robins et al., 2007a; Robins et al., 2007b).

Rather than focusing only on network structure, researchers collecting network data often pose research questions about individual-level outcomes. Valente (2010) makes a strong case for the importance of SNA in understanding health- and disease-related phenomena, particularly regarding the process of behavioral or attitudinal influence. With SNA, each ego's exposure, or "the degree to which a focal individual's alters engage in a particular behavior" (Valente, 2010, p. 65), can be modeled. Exposure is a possible mechanism to explain diffusion of innovations or changes in health behaviors, but standard generalized linear models would be inappropriate to explain individuals' behaviors because observations would not be independent (Kenny et al., 2006). SNA, on the other hand, accounts for interdependency among the observations, and can take into account characteristics of both alters and weights of the ties. Ego-network traditions have sometimes relied upon perceptual data of one's alters as a possible mechanism of measuring exposure, but Valente (2010) cautions that perceptions of one's friends by egos are biased and cannot be taken as an accurate estimate of the actual behavior or attitudes of alters. Importantly, the nature of these perceptional biases (e.g., to be congruent with one's own perceptions) is not something typically modeled in SNA. Valente (2010) even offers examples of prior attempts to reconcile perceptual data with partner self-reports, but does not mention the SRM as a methodological option for doing so. This paper offers SNA researchers a new and statistically appropriate way to model perceptions and biases.

The SRM (Kenny, 1994; Kenny et al., 2006) is fundamentally concerned with how individuals perceive each other (i.e., interpersonal perceptions). In their chapter addressing SNA, Kenny et al. (2006) discuss the similarities between p_1 and SRM, suggesting that the former SNA technique is an extension of SRM for binary data. Kenny et al. (2006) admit that using the SRM for dichotomous SNA is "not entirely appropriate" due to differences in measurement of the ties/perceptions between network members (p. 313). That is, interpersonal perceptions are usually interval level measurements, not dichotomous measures. Interpersonal perceptions can be decomposed into person-level and dyad-level components, allowing investigation of how perceptions relate to each other (i.e., reciprocity) within the network. Data can also be collected on self-perceptions—or relevant attitudes or behaviors—to reveal, for instance, how others' perceptions correlate with self-perceptions (i.e., self–other agreement) or actual behaviors (i.e., accuracy). Many of the conditions of the SRM, including data on perceptions of alters (not just the presence or absence of a tie), interval level measurement, the assumption of primarily reciprocal ties (i.e., bidirectional ties), are atypical in most SNA designs (Valente, 2010). Although researchers are undoubtedly interested in the perceptions of alters in SNA, such as the weight of the ties in relation to alter characteristics or behaviors, it is very rarely done in practice.

The present investigation will extend the SRM to traditional bounded SNA data, which includes perceptions of ties and self-reported behavioral and attitudinal characteristics. This collection of all relevant information about network members—indications of ties as well as characteristics of individuals and of tied dyads—has been referred to as a social relational system (van Duijn et al., 2004; Wasserman and Faust, 1994). The present investigation will be valuable not only for dealing with missing round-robin data, but also for researchers who are interested in exploring the attributes and interpersonal perceptions only among tied network members in existing relationships, in which case ratings from members who are not closely tied would be irrelevant to their research question. For instance, researchers may be interested only in how friends perceive each other, rather than in perceptions among all possible peers. Past research suggests that close relationships are particularly influential and important in understanding health behavior (Valente, 2010). Researchers may not be particularly interested in examining network structure, but instead would use the structure of the network (i.e., presence of directed ties between egos) to define the sampling frame. To apply the SRM to such data, several data management and analysis barriers must be overcome, particularly accommodating the fact that data from a traditional round-robin design would be "missing" when data are gathered from only a subset of all possible dyads.

Missing data mechanisms

Inferences drawn about parameters estimated from partially observed data can be biased to the degree that the missing data are not *ignorable*. Rubin (1976) defined three mechanisms of missingness, some of which can be considered ignorable (Enders, 2010, p. 13; Little et al., 2014), depending on which analytical method is used. If the probability of observation depends on the values of the missing observations themselves, then data are said to be missing not at random (MNAR; Rubin, 1976). Data can also be considered MNAR if missingness depends on variables that are not observed, or are not included in the analysis model. If variables related to missingness are observed and included in the analysis model, then data are said to be missing at random (MAR), given the observed data. That is, whether data are missing is unrelated to the missing data, conditional on the observed data. If missingness is unrelated to missing data even without conditioning on observed data, then data are said to be missing completely at random (MCAR). Only multiple imputation or maximum likelihood methods can return unbiased point and *SE* estimates after adequately incorporating variables related to missingness. Thus, MAR data are ignorable when using multiple imputation or maximum likelihood estimation. MCAR data are also ignorable, without requiring any additional variables to justify a MAR assumption, but the MCAR assumption is much more restrictive and probably only defensible when data are missing by design using random assignment (Little et al., 2014). Even when data are MCAR, multiple imputation and maximum likelihood allow estimation to exploit all available information, which yields greater power than listwise deletion (the usual software default). MNAR however, is not an ignorable mechanism, and even multiple imputation and maximum likelihood will likely return biased estimates and tests (Enders, 2010, ch. 10).

Multiple imputation (Rubin, 1987) is a Bayesian technique for substituting each missing value with multiple estimates of what may have been observed, resulting in multiple complete-data copies of the original incomplete data set, and the analysis is performed on each copy. Rubin (1987) outlines procedures for pooling the results across imputations (see also Enders, 2010, ch. 8). Alternatively, no pooling is necessary if the data are imputed as part of the analysis itself (i.e., data augmentation), which has been shown to yield more efficient results (i.e., smaller *SE*s; Merkle, 2011). Data augmentation is the approach we take in this paper, where missing values are treated as additional parameters to be estimated with other model parameters, as described in the following section. However, the data-augmentation model we propose could also be used as an imputation model by researchers interested in separating the imputation and analyses tasks, as described in Rubin (1987) and Enders (2010).

Measuring variables that explain missingness or correlate with the missing values is pivotal in defending the MAR assumption, even if they are not of substantive interest in the hypothesized model for the observed data. For example, if data were missing due to the influence of an observed covariate (or a correlated proxy of it), then the data would be conditionally MAR by including that covariate in the model estimation. When a covariate of missingness is not already part of a hypothesized model, the covariate is referred to as an *auxiliary* variable. In order not to change the interpretations of other model parameters, an auxiliary variable can be included in the model by merely estimating its correlation with other variables or their residuals (e.g., a saturated correlates model; Enders, 2010). In the SRM, there are multiple levels of measurement so auxiliary variables can be included at the person level or at the dyad level. Note that an auxiliary need not be completely observed, but its missing data mechanism must also be assumed ignorable (Thoemmes and Mohan, 2015).

Considering mechanisms and auxiliaries in the SRM context

Round-robin data are dyadic, where observations Y_{ij} and Y_{ji} are nested within people who act as both ego and alter. Thus, the SRM includes dyad-level observed variables and person-level random effects (latent variables). Auxiliary covariates and substantive predictors can be included in the extended SRM (details provided in the next section), which can be measured at the person or dyad level. Thus, missing-data mechanisms should be considered at both levels of analysis.

Dyad-level missing data occurs when one or both observations within a dyad are missing (i.e., if Y_{ij} is missing, Y_{ji} may still be observed). If at least one observation in a dyad is observed and, given their person-level effects, observations within a dyad are correlated, then information about the missing dyad-level value can be informed by the observed value. If both values in a dyad are missing, then their missing values can be informed by their respective person-level effects and by any auxiliary dyad-level relationship information that is correlated with the relationship effects.

Dyad-level nonresponse can be assumed MCAR if dyad-level data are missing by design. For example, Brunson et al. (2016) assigned each participant in their round-robin design to rate only a random subsample of their fellow sorority or fraternity members. Because the random process of missingness was unrelated to any substantive variables, these data were MCAR, yielding unbiased estimates without the need for auxiliary variables. If, however, dyad-level data are missing for reasons related to substantive variables, those variables must be included in the analysis for the MAR assumption to hold. For example, if person i is less likely to provide ratings for female classmates, then the sex of the alter should be included as a covariate or predictor. Omitting sex from the model in this situation would mean data were MNAR.

Unit nonresponse would result in several missing dyad-level data points. For example, if person i does not provide ratings about any classmates (or none provide ratings about person i), then no information is available about that person's ego (or alter) effect. We could refer to these as outgoing and incoming unit nonresponse, respectively. Whenever ego and alter effects are correlated, then information about one can provide information about the other when unit nonresponse might otherwise prevent its estimation. Auxiliary person-level variables can also be included in the model to inform the estimation of missing data. If person i is not a "random draw" from the sample of students (i.e., if people who provide ratings systematically differ from those who do not), then person-level variance components associated with ego or alter effects could be biased. Outgoing unit nonresponse might be MCAR if, for example, a student were simply absent on the day of data collection (assuming they were not absent to avoid participating for reasons that are related to the variable being measured). But if few students provide ratings about shy or asocial students, or if fraternity members who lead an active late-night social life were less likely to participate, then measuring relevant behaviors would be necessary to justify the MAR assumption and yield unbiased estimates of variance components.

Beyond round-robin designs.. Because a missing data point Y_{ij} does not prevent estimating person i's ego effect nor person j's alter effect if they have (been) rated (by) other network members, complete round-robin data are not required to estimate SRM parameters. Furthermore, a round-robin design would not be appropriate if a researcher were interested in interpersonal perception among tied network members rather than among all possible dyads. In our applied example, for instance, sorority members were asked to identify close friends, and then to report perceptions about those friends. Because the research question involves the correspondence between self-reported behavior and perceptions of close friends, round-robin data might yield biased estimates to the extent that women are perceived differently by strangers and casual acquaintances than they are by friends. For example, women are more likely to notice the frequency of a behavior that their close friends exhibit, whereas dyads without a relationship would not, so reports from round-robin data would be attenuated relative to valid reports from friends.

Suppose further that participants provided responses about "out-of-network" individuals who provided no responses themselves. This may be particularly common when sampling open networks, such that the sampling frame (perhaps defined by convenience or arbitrarily by necessity) does not constitute a real boundary to the network. Kossinets (2006) provides some discussion about the boundary specification problem and how it can lead to missing network data. Whether the network is open or closed, there are research questions that could not be properly investigated with round-robin data. The ability to fit the SRM to incomplete round-robin data also allows the SRM to be applied to valid ratings among ties in a network. In this context, the data that would be considered missing in a round-robin design would simply not be part of the population of observable phenomena about which the researcher wants to draw inferences. This context, however, requires some additional considerations of missing-data mechanisms.

Analyzing only individuals in a relationship implies that any non-tied dyads are excluded from the analysis. This assumes that if person i does not identify person j, and vice versa, they truly do not have a relationship. However *relationship* is defined in the context of the study, that criterion should be clear enough for participants to accurately and truthfully identify people with whom they relate (e.g., a friend). If person i is in a relationship with person j but neither identifies the relationship with the other, data would only be MCAR if the reason why they failed to indicate a relationship were independent of the interpersonal ratings they would otherwise have provided. Missing relationships would be MCAR if, for example, two friends were both absent on the day of data collection. Although a MAR process could be possible under very restrictive circumstances (i.e., other friends provided ratings about the absent friends, ego and alter effects are correlated, auxiliary person- and dyad-level information about the absent friends is included in the model), researchers may be unlikely to know whether they should include any such non-tied dyads in the analysis to begin with. Asking participants to identify which of their peers are friends with each other would be possible, but would introduce a greater burden to participants as well as the analyst, who would then need to contend with triadic data (i.e., person i's metaperception of person j's relationship with person k; see Kenny, 1994, ch. 8).

The more obvious case to consider is when only one member of a dyad rates the other, which could occur either because one person did not participate or because person i indicates a relationship with person j but not vice versa (i.e., an asymmetric tie). As discussed above in the context of round-robin data, nonparticipation would yield MCAR data if participants and nonparticipants did not differ with respect to modeled variables. Otherwise, variables predicting nonparticipation would need to be included in the model to justify the MAR assumption.

If, on the other hand, person i's perceived relationship with person j is not reciprocated, MCAR would only be a tenable assumption if person i's perception of person j does not systematically differ from either (a) person i's perceptions of friends who do provide ratings of person i or (b) person j's nominated friends' perceptions of person j. Ego and alter effects would be biased to the degree to which (a) or (b), respectively, do not hold, unless variables explaining those differences were incorporated into the model.

This is not an exhaustive list of considerations, but they are meant to make the reader aware of issues relevant to the estimation of SRM parameters under these special circumstances. Now that we have discussed missing-data mechanisms and the potential importance of auxiliary variables at length, we provide details about how to fit an extended SRM to incomplete data.

Multilevel model of social relations with covariates and missing data

In a general(ized) latent variable modeling framework (e.g., Muthén, 2002; Skrondal and Rabe-Hesketh, 2004), the SRM is simply a cross-classified multilevel regression model, where dyadic observations are nested both within egos and within alters. But unlike typical cross-classified models that can be fit in standard MLM software (e.g., students nested within schools and within neighborhoods, each of which have independent random effects on student-level outcomes), the SRM requires its cross-classified random effects to be correlated because the egos and alters are from the same network. For example, if someone with a high ego effect is more likely to have a low alter effect (or vice versa), then the ego and alter effects would be negatively correlated. Snijders and Kenny (1999) showed that in certain conditions, the SRM can be fitted equivalently as (a) a three-level MLM to a univariate outcome, in which individual ratings are nested within dyads that are nested within egos and alters, or (b) a two-level MLM to a bivariate outcome, in which a dyad's pair of ratings are nested within egos and alters. We adopt the latter approach because it is more general, allowing within-dyad residuals to be negatively correlated, whereas the three-level approach assumes dyad residuals can only be positively correlated (Snijders and Kenny, 1999).

To begin, we review how the SRM parameters are interpreted in the simplest generic case (where ego i rates alter j on an outcome). We then describe how to additionally estimate correlations of person-level covariates with person-level random effects, as well as correlations of dyad-level covariates with dyad-level residuals. We use this simple extension to explain how the extended SRM can accommodate missing dyad-level and person-level data, where the covariates can be used as auxiliary variables to justify the MAR assumption. We then reframe the extended SRM by using the covariates as substantive predictors, discussing how the same information is expressed in statistically equivalent ways.

The standard SRM partitions the variance of dyad-level observations Y_{ij} into variance due to three components, each of which are deviations from a constant:

$$Y_{ij} = \mu + E_i + A_j + R_{ij}. \tag{1}$$

The components of the standard SRM have the following interpretations:

- Grand mean (μ): The average of the outcome, controlling for any person- or dyad-specific effects (described below). Variance in the outcome (σ_Y^2) is partitioned into effects due to the following three sources.
- Ego effect (E_i): How response Y_{ij} provided *by* person i differs in general from μ (i.e., regardless of who (j) the response was about).
- Alter effect (A_j): How the response Y_{ij} provided *about* person j differs in general from μ (i.e., regardless of who (i) provided the response).
- Relationship effect (R_{ij}): How person i's response about person j differs from μ, even after taking into account person i's ego effect and person j's alter effect. Any true relationship effect in the dyad-specific residual R_{ij} is confounded with measurement error, and partitioning it into true-relationship and error components would only be possible when using multiple indicators to measure the same construct.

The ego and alter effects are assumed to be bivariate normally distributed with means of zero and variances σ_E^2 and σ_A^2:

$$\begin{bmatrix} E_i \\ A_i \end{bmatrix} \sim \mathcal{N}\left(\begin{bmatrix} 0 \\ 0 \end{bmatrix}, \begin{bmatrix} \sigma_E^2 & \\ \rho_{EA}\sigma_E\sigma_A & \sigma_A^2 \end{bmatrix} \right), \tag{2}$$

where$i = 1 \ldots N$. The correlation between person i's ego and alter effects (ρ_{EA}) is referred to as *generalized reciprocity*.

The dyad-specific residuals R_{ij} and R_{ji} are also assumed to be bivariate normally distributed with means of zero and a common variance σ_R^2:

$$\begin{bmatrix} R_{ij} \\ R_{ji} \end{bmatrix} \sim \mathcal{N}\left(\begin{bmatrix} 0 \\ 0 \end{bmatrix}, \begin{bmatrix} \sigma_R^2 & \\ \rho_R\sigma_R & \sigma_R^2 \end{bmatrix} \right). \tag{3}$$

The within-dyad correlation between R_{ij} and R_{ji} (ρ_R) is referred to as *dyadic reciprocity*. The equality constraint on the residual variances could be relaxed if i and j denoted specific roles or characteristics,[2] such as men only rating women (and vice versa) in a block design (e.g., a speed-date study with heterosexual participants; Kurzban and Weeden, 2005). In such a case, there may be more unexplained variance (or relationship variance) for men rating women than for women rating men. However, when the order of dyad members i and j is arbitrary, there is no substantive reason to expect residuals R_{ij} and R_{ji} to have different variances. Furthermore, estimated differences could change if the order of participants in the data set were rearranged, without any particular order being meaningful.

The sum of the three variances represents the total variance of individual ratings: $\sigma^2_{\text{Total}} = \sigma^2_E + \sigma^2_A + \sigma^2_R$. An intraclass correlation coefficient (ICC) for the ego and alter effects can be calculated by dividing those variances by the total variance.

Estimating SRM parameters with missing data

To estimate the SRM parameters, we adopt the Bayesian framework described by Lüdtke et al. (2013) (see also Hoff, 2005). The posterior distribution $P(\boldsymbol{\theta} \mid \mathbf{Y})$ of model parameters $\boldsymbol{\theta}$ given the observed data $\mathbf{Y}$ is proportionally equivalent to the product of the prior probability distribution $P(\boldsymbol{\theta})$ and the likelihood of the data conditional on the parameters, $P(\mathbf{Y} \mid \boldsymbol{\theta})$. Markov chain Monte Carlo (MCMC) estimation $P(\boldsymbol{\theta} \mid \mathbf{Y})$ requires specifying $P(\boldsymbol{\theta})$ for (hyper)parameters. Using noninformative priors results in estimates of the posterior distributions that are influenced solely by the observed data. In such cases, the modal *a posteriori* (MAP) estimate of a parameter in $\boldsymbol{\theta}$ corresponds to its maximum likelihood point estimate, and its mean or "expected" *a posteriori* (EAP) estimate corresponds to a least-squares point estimate. When the posterior distribution of a parameter is approximately normal (or at least symmetric with a hump in the middle), MAP and EAP estimates differ only negligibly. The *SD* of the marginal posterior distribution of a parameter in $\boldsymbol{\theta}$ corresponds to the *SE* of the estimate, and 2.5th and 97.5th percentiles correspond to 95% confidence limits, called a Bayesian credible interval (BCI).

Estimation with MCMC allows all unknown quantities in the model to be estimated jointly. In the SRM, the unknown quantities include μ in (1), the N vectors of person-level random effects in (2), the variances and correlation in (2), and the variance and correlation in (3). The variances and correlation in (2) can be considered hyperparameters that describe the distribution of the $2N$ parameters $\{E, A\}$, and the variance and correlation in (3) are the parameters describing the conditional distribution of Y_{ij} given μ and $\{E, A\}$. These $2N + 6$ parameters are stored in the parameter vector $\boldsymbol{\theta}$, which is estimated by drawing a large number of random samples from the joint posterior distribution, $P(\boldsymbol{\theta} \mid \mathbf{Y})$.

The bivariate normal likelihood of observed data $P(\mathbf{Y} \mid \boldsymbol{\theta})$ is specified using the vector of expected values for $\mathbf{Y}_{\{ij\}}$ as the mean vector:

$$\hat{\mathbf{Y}}_{\{ij\}} = \begin{bmatrix} \hat{Y}_{ij} \\ \hat{Y}_{ji} \end{bmatrix} = \begin{bmatrix} \mu + E_i + A_j \\ \mu + E_j + A_i \end{bmatrix}, \tag{4}$$

where the curly brackets in (4) indicate that the order of i and j within a dyad does not matter, so $\{ij\}$ simply indicates both observations within a dyad. The covariance matrix in the likelihood in (5) is the covariance matrix of residuals in (3):

$$\begin{bmatrix} Y_{ij} \\ Y_{ji} \end{bmatrix} \sim \mathcal{N}\left(\begin{bmatrix} \hat{Y}_{ij} \\ \hat{Y}_{ji} \end{bmatrix}, \begin{bmatrix} \sigma^2_R & \\ \rho_R \sigma^2_R & \sigma^2_R \end{bmatrix} \right). \tag{5}$$

[2] Note that in block designs, researchers may also estimate separate ego and alter variances, as well as reciprocity parameters, for each role in a block design (Snijders and Kenny, 1999).

When $\mathbf{Y}$ is only partially observed, the $m < N$ missing data points can also be treated as unknown parameters to be estimated, in which case $\boldsymbol{\theta}$ would be of length $k + m$ (see Song and Lee, 2008, for an application in the context of multilevel structural equation modeling). Conceptually, there is no distinction between the m missing data points and the $2N$ random effects, which are essentially missing data (unobserved latent variables). This is sometimes referred to as "augmenting" the observed data with latent or missing data (Lüdtke et al., 2013; Song and Lee, 2008; see Koskinen et al., 2010, for an example in the context of network data). The prior distribution for these missing data would be the same as the likelihood specified for the observed data in (5). If the data are MCAR or MAR given the observed data, estimates of the missing data points and other model parameters will be unbiased, and the marginal posterior *SD* of other model parameters will reflect the additional uncertainty due to missing data, providing valid inferences (Enders, 2010; Little et al., 2014).

A person-level auxiliary variable X can be included in the SRM estimation by sampling the vector described in (2) from a multivariate normal instead of bivariate normal distribution.

$$\begin{bmatrix} E_i \\ A_i \\ X_i \end{bmatrix} \sim \mathcal{N}\left(\begin{bmatrix} 0 \\ 0 \\ \mu_X \end{bmatrix}, \begin{bmatrix} \sigma^2_E & & \\ \rho_{EA}\sigma_E\sigma_A & \sigma^2_A & \\ \rho_{XE}\sigma_X\sigma_E & \rho_{XA}\sigma_X\sigma_A & \sigma^2_X \end{bmatrix} \right) \tag{6}$$

The mean and variance of X are freely estimated, as are the correlations of X with E and A. Missing values in X can also be estimated as parameters, using (6) as the prior distribution. Multiple auxiliaries can be included in this manner without loss of generality. When the correlations in (6) are nonzero, person i's ego and alter effects can inform what their missing X values may have been (and vice versa in the case of incoming or outgoing unit nonresponse). If person-level data are MCAR (e.g., missing by design), then sampling random effects from the distribution in (2) would be sufficient to provide unbiased estimates, but including correlated auxiliaries might reduce the sampling variance reflected in the posterior *SD*.

Similar to how (6) extends (2) to include person-level auxiliaries, we can include dyad-level auxiliaries by extending (3). Dyad-level auxiliaries may be constant within a dyad (e.g., the duration of a relationship person i and person j will be the same value for both of them); we denote a dyad-constant variable as $V_{\{ij\}}$. Dyad-level auxiliaries may also vary within a dyad (e.g., how often person i calls person j might differ from how often person j calls person i); we denote a dyad's vector of such auxiliaries as $\mathbf{W}_{\{ij\}}$. Both types of dyad-level auxiliary variable can be included in the SRM estimation by using a multivariate normal distribution:

$$\begin{bmatrix} Y_{ij} \\ Y_{ji} \\ V_{\{ij\}} \\ W_{ij} \\ W_{ji} \end{bmatrix} \sim \mathcal{N}\left(\begin{bmatrix} \hat{Y}_{ij} \\ \hat{Y}_{ji} \\ \mu_V \\ \mu_W \\ \mu_W \end{bmatrix}, \begin{bmatrix} \sigma^2_R & & & & \\ \rho_R\sigma^2_R & \sigma^2_R & & & \\ \rho_{VR}\sigma_V\sigma_R & \rho_{VR}\sigma_V\sigma_R & \sigma^2_V & & \\ \rho_{WR}\sigma_W\sigma_R & \rho_{WR}\sigma_W\sigma_R & \rho_{WV}\sigma_W\sigma_V & \sigma^2_W & \\ \rho_{WR}\sigma_W\sigma_R & \rho_{WR}\sigma_W\sigma_R & \rho_{WV}\sigma_W\sigma_V & \rho_W\sigma_W & \sigma^2_W \end{bmatrix} \right). \tag{7}$$

The means of $V_{\{ij\}}$ and $\mathbf{W}_{\{ij\}}$ and the variance of $V_{\{ij\}}$ are freely estimated, as are most correlations, but the estimated mean and variance of $\mathbf{W}_{\{ij\}}$ and residual variance of $\mathbf{Y}_{\{ij\}}$ are constrained to equality, as are any correlations with $\mathbf{W}_{\{ij\}}$ or with $\mathbf{Y}_{\{ij\}}$. Similar to the equality constraint on residual variances in (3), these equality constraints can be relaxed if i and j denote specific roles or characteristics. Continuing with the example of men and women only rating each other in a speed-dating study, there may be a greater mean or variance of W for one sex than for another, or W may correlate with V or with Y to a greater degree for one sex than for another. However, if the order of members within a dyad is arbitrary, then there would be no substantive reason for estimating parameter without the equality constraints in (7). Missing values in $V_{\{ij\}}$ and $\mathbf{W}_{\{ij\}}$ can also be estimated as parameters, using the likelihood in (7) as the prior distribution.

Incorporating covariates as substantive predictors

Covariates can also be incorporated into the SRM as predictors if their effects are of theoretical interest. The person-level random effects in (1) can be defined as a linear combination of fixed and random effects, yielding a cross-classified Level-2 model:

$$E_i = \beta_1 X_i + \varepsilon_i \quad A_j = \alpha_1 X_j + \delta_j \tag{8}$$

The ego effect in (1) is defined in (8) as the sum of an effect of X_i (weighted by the slope β_1) and an ego-specific residual (ε_i). The difference between σ^2_E and σ^2_ε represents the portion of the ego effect that can be explained by ego characteristics (X_i), and dividing this difference by σ^2_E provides the level-specific effect size measure pseudo-R^2 used in MLM. The magnitude of pseudo-R^2 would be the square of ρ_{XE} in (6), and the sign of β_1 would correspond to the sign of ρ_{XE}. Thus, the models are statistically equivalent whether the relationship between the covariate and the ego effect is modeled as a free correlation in (6) or as a directed regression path in (8).

Likewise, the alter effect is defined as the sum of an effect of X_j (weighted by the slope α_1) and an alter-specific residual (δ_j). The same variable is therefore in the model twice: X_i explains ego effects, whereas X_j explains alter effects. With X as a predictor of ego and alter effects, μ would be interpreted as an intercept (i.e., the average of Y_{ij} when $X = 0$ for both the ego and alter), but X can be centered at the mean (or other meaningful value) to make the intercept interpretable if 0 is outside the range of X. Consistent with linear regression in any other context, the residuals of random effects are assumed independent of the covariates that predict the random effects. So rather than estimating the parameters in (6), the distribution of ego and alter residuals (conditional on X) is now independent of the distribution of X.

$$\begin{bmatrix} \varepsilon_i \\ \delta_i \\ X_i \end{bmatrix} \sim \mathcal{N}\left(\begin{bmatrix} 0 \\ 0 \\ \mu_X \end{bmatrix}, \begin{bmatrix} \sigma^2_\varepsilon & & \\ \sigma_{\varepsilon\delta} & \sigma^2_\delta & \\ 0 & 0 & \sigma^2_X \end{bmatrix} \right) \tag{9}$$

Partially observed dyad-level predictors $V_{\{ij\}}$ and $\mathbf{W}_{\{ij\}}$ can be added to the Level-1 model in a similar fashion.

$$\hat{\mathbf{Y}}_{\{ij\}} = \begin{bmatrix} \hat{Y}_{ij} \\ \hat{Y}_{ji} \end{bmatrix} = \begin{bmatrix} \mu + \gamma_1 V_{\{ij\}} + \gamma_2 W_{ij} + E_i + A_j \\ \mu + \gamma_1 V_{\{ij\}} + \gamma_2 W_{ji} + E_j + A_i \end{bmatrix} \tag{10}$$

Analogous to the logic given for constraints on (co)variances in (7), if the order of partners within the dyad is arbitrary, the effect γ_1 of $V_{\{ij\}}$ is constrained to equality, as is the effect γ_2 of $\mathbf{W}_{\{ij\}}$, just as a single dyad-level residual variance is estimated. But these equality constraints could be relaxed if data are gathered from subjects with specific roles. Similar to (9) for the Level-2 model, the model for saturated auxiliaries in (7) would be replaced with covariates that are independent of the residuals that are conditional on those covariates:

$$\begin{bmatrix} Y_{ij} \\ Y_{ji} \\ V_{\{ij\}} \\ W_{ij} \\ W_{ji} \end{bmatrix} \sim \mathcal{N}\left(\begin{bmatrix} \hat{Y}_{ij} \\ \hat{Y}_{ji} \\ \mu_V \\ \mu_W \\ \mu_W \end{bmatrix}, \begin{bmatrix} \sigma^2_R & & & & \\ \rho_R\sigma^2_R & \sigma^2_R & & & \\ 0 & 0 & \sigma^2_V & & \\ 0 & 0 & \rho_{WV}\sigma_W\sigma_V & \sigma^2_W & \\ 0 & 0 & \rho_{WV}\sigma_W\sigma_V & \rho_W\sigma_W & \sigma^2_W \end{bmatrix} \right). \tag{11}$$

Including dyad-level covariates to explain any deterministic part of the relationship effect can thus tease apart at least some of the dyad-level variance that is truly due to a relationship effect rather than merely error. However, without the use of multiple indicators, it would still be unknown how much remaining residual variance could be explained by omitted dyad-level covariates.

Illustrative application

Having described how to accommodate missing data in a SRM analysis of incomplete round-robin or SNA data, we demonstrate how to apply the SRM to data from friends in a sorority. That is, we are interested in explaining interpersonal perceptions not among all members of a large network, but rather interpersonal perceptions among close friends (ties) within that network.

Theoretical foundation for investigating perceptions of body comparison

Body image concerns have been called a “normative discontent” among women (Rodin et al., 1984), as approximately half of women experience body dissatisfaction (Fiske et al., 2014). Body image concerns reside on a spectrum, from women who experience minimal dissatisfaction with their body shape and weight to the extreme preoccupation observed in eating disorders. Body image concerns are risk factors for a host of negative outcomes including overweight status (Goldschmidt et al., 2016), eating disorders (Goldschmidt et al., 2016; Jacobi et al., 2004; Stice, 2002; Stice et al., 2011), and depressive symptoms (Goldschmidt et al., 2016; Paxton et al., 2006). Even in the absence of a diagnosable mental disorder, body image concerns are associated with distress and lower quality of life (Mitchison et al., 2013; Vannucci et al., 2012).

Sociocultural theories of body image concerns posit that pressures to be thin from family, peers, and media cause women to develop body image concerns through two pathways (Thompson et al., 1999). The first pathway is through social comparison, specifically comparing one’s body to the bodies of peers, celebrities, and others. The second pathway is through internalization of the thin ideal, or the development of the belief that being thin is important and a prerequisite for a happy and successful life. Media, peer, and family influences are thought to increase both social comparisons and thin-ideal internalization. Numerous cross-sectional (Rodgers et al., 2011; van den Berg et al., 2002; Yamamiya et al., 2008) and prospective studies (Fitzsimmons-Craft et al., 2014; Rodgers et al., 2015) support this framework.

Peer influences are particularly relevant in the development or maintenance of body image concerns as young women develop independence from their family of origin. There is substantial evidence of peer similarity in body image concerns and preliminary evidence that peers’ attitudes prospectively predict body image concerns (Badaly, 2013). Both social norms regarding disordered eating and appearance-related conversations (e.g., “fat talk”) are associated with extreme weight/shape concerns and eating pathology (Forney et al., 2012; Forney and Ward, 2013; Gerbasi et al., 2014; Giles et al., 2007; Tzoneva et al., 2015). Consistent with these processes, studies of college women provide evidence for socialization of body image concerns (Meyer and Waller, 2001). Engaging in appearance-related conversations increases body image concerns in college students (Salk and Engeln-Maddox, 2012), and the small literature to date suggests that “fat talk” may be a causal risk factor for body image concerns (Sharpe et al., 2013).

Both peer and media influences on body image concerns are posited to be partially mediated through appearance comparisons. Indeed, comparing one’s own body to that of others, either directly through appearance-related conversation or covertly (i.e., in thought only), is associated with body image concerns (Fitzsimmons-Craft et al., 2014; Myers and Crowther, 2009; Thompson et al., 1999). Similarly, comparisons of one’s own appearance to models and peers partially mediate the relationship between thinness norms and body image concern (Blowers et al., 2003; Carey et al., 2014). Specifically, upward body comparisons (i.e., to those perceived as more attractive than the self) are associated with increased body image concerns and thoughts about dieting (Leahey et al., 2007). Thus, comparisons to celebrities and models who typify the thin ideal likely increase body image concerns and risk for disordered eating. Indeed, media exposure and consumption are associated with increased body image concerns (Grabe et al., 2008; Groesz et al., 2002; Krcmar et al., 2008).

The extant literature supports that peer influences, including appearance-related conversations and body comparisons, contribute to body image concerns. Many of these studies rely on individuals' perceptions of their peer environments (e.g., Gerbasi et al., 2014; Giles et al., 2007; Tzoneva et al., 2015). Perceptions of the peer environment may reflect an individual's own body image concern, which is consistent with longitudinal evidence that body dissatisfaction increases perceptions of peer influence (Rayner et al., 2013a), yet there is also evidence for similarity in body image concerns among friends and peers relative to non-friends and non-peers using SNA and other designs (Meyer and Waller, 2001; Rayner et al., 2013b). The extent to which self-report measures of peer behaviors reflects the peer environment remains poorly understood. Although SNA studies of peer influences (e.g., Rayner et al., 2013b) allow for the objective assessment of similarity between girls' body-related attitudes, analysis of network structure alone would not enable direct incorporation of peer perceptions at the individual level. In one study of adolescent girls, perceptions of the peer environment, including perceptions of appearance conversations, were modestly correlated with girls' friendship cliques' body image concern (Paxton et al., 1999). A girl's perception of her peer environment, but not her cliques' self-reported body image concern, predicted her own body image concern. However, these studies assessed perceptions of friends in general, rather than perceptions of specific friends as identified by girls. That is, the critical question of the degree to which these perceptions were accurate reflections of their peers' behavior remains unanswered. Furthermore, these studies did not directly test the relative contributions of girl's pathological attitudes and the peer environment in influencing her perceptions of the peer environment.

The current study aims to clarify the accuracy of perceptions of peers by exploring how perceptions of friends' body talk are related to peer's own self-reported body comparisons and egos' self-reported body comparison. In doing so, we assume that engagement in body talk is an observable indicator of body comparisons. We do so with the SRM, whose parameters express these relationships simultaneously, allowing us to estimate the degree to which perceptions of peers can be attributed to egos' own self-reported behaviors.

If perceptions of one's peer environment are accurate, then we would expect a strong alter effect (i.e., *consensus* among peers about a person; Kenny, 1994, ch. 4), and we would expect a person's alter effect to correspond positively with that person's self-reported comparisons. Because we use self-reports of internal "behavior" rather than an observable objective criterion, *self–other agreement* may be a more appropriate term than *accuracy* (Kenny, 1994, ch. 7 and ch. 9), although the latter would be appropriate to the degree that self-reports are an adequate criterion for accuracy in this context.

Alternatively, if perceptions of friends in the network are more associated with one's own behaviors, then there would be a strong ego effect. Kenny (1994, ch. 3) refers to this perceiver effect as *assimilation*—a person's general perception of others' talk. We would also expect a person's ego effect to correspond positively with that person's self-reported body comparisons, which could be characterized as *projection* of the ego's characteristics onto alters being perceived.

Table 1
Peer network questions and corresponding self-report questions.

Variable Name	Peer Network Question	Self-Report Question
Celebrity body talk (comparison)	How often do you hear [this friend] mention celebrities' **bodies**?	How often do you compare your **body** to actors' and celebrities' **bodies** that you see on TV and the movies?
Self body talk (comparison)	How often does [this friend] talk about her **body**?	How often do you compare your **body** to your friends' **bodies**?

This approach provides a framework to compare the explanatory power of each source of variance in perceptions within a single statistical model. We also sought to examine how a woman's drive for thinness might be associated with her perceptions of other's self body talk and with others' perceptions of her self body talk.

Method

Participants

All participants were members of a 168-member all-female social sorority in a small private college in the southeastern United States during the spring of 2005. This study was part of a larger research project exploring body image and eating disorders on the college campus. Researchers approached several sororities' leadership about their members participating in the study. One sorority agreed to participate. Rather than an individual incentive, the sorority was offered a $75 gift card incentive for the entire organization that was not based on rates of participation or completion. The leadership of the sorority distributed the online instrument to their members and encouraged their members to participate in the study.

Participants were an average age of 20.3 years old (*Mdn* = 20), 97% White and 3% Asian American, and were distributed evenly between freshmen to seniors. Ninety members of the sorority completed SNA measures (peer nominations), peer perception measures, and self-report measures. Two additional members completed SNA and peer perceptions but not self-report measures. Of the 168 sorority members, six members neither completed study measures nor were nominated by participating members and were excluded from all further analyses.

The sample to which we fit the SRM therefore included 90 (56%) women who functioned both as egos and as alters, and an additional 72 (44%) women who functioned only as alters. Due to such a higher proportion of outgoing unit nonresponse, only 120 (17%) out of the 724 observed dyads provided complete data (i.e., Y_{ij} and Y_{ji} were both observed), but one of the two possible responses from the remaining dyads were observed, so we were able to include all available information in the analysis.

Instrumentation and measures

Participants completed an online survey. Participants first identified their "very close friends" in the sorority from a roster of the membership. No restrictions were placed on the number of friend nominations. Participants typically identified about nine other women in the sorority as very close friends (M = 9.44, SD = 6.17, *range* 1–35). Then, participants were asked a series of questions about each nominated friend, two of which were used in the present study and are shown in Table 1. Responses to peer-network questions were measured on scales from 1 (*never or almost never*) to 5 (*very often*).

After completing the social network component of the study, participants responded to the two items shown in the right column on Table 1, as well as other measures not reported here. Responses to self-report questions were measured on 5-point scales from −2 (*never*) to 2 (*very often*). These two items were each used as a covariate in an extended SRM fit to the corresponding peer-perceptions.

Finally, participants completed the Eating Disorder Inventory Drive for Thinness subscale as an indicator of body preoccupation (Garner et al., 1983). The Drive for Thinness subscale comprises seven items such as "I am terrified of gaining weight" and "I exaggerate or magnify the importance of weight" on a 6-point scale, so subscale scores ranged from 7 to 42. Higher scores reflect greater preoccupation with weight and a stronger pursuit of thinness, and so the expected relationship with peer perceptions is negative, given that low peer perception scores reflect more frequent comparison. The scale discriminates between women with and without an eating disorder (Garner et al., 1983; Hurley et al., 1990). Although the Drive for Thinness subscale is correlated with body mass index (Keski-Rahkonen et al., 2005), it taps a more pathological aspect of weight concerns than normative desires to be at a medically healthy weight when overweight or obese. The nonclinical scoring was used to increase sensitivity (Keel et al., 2007). Cronbach's alpha was 0.92.

Analysis plan

To explore the sources of variability in celebrity body talk, we fit our extended SRM in (6) to the questions in the top row of Table 1, treating self-reports as auxiliary correlates to improve estimation of missing data. This illustrates a situation when a covariate is not of substantive interest, but in our case we are interested in interpreting these parameters. We subsequently fit the extended SRM in (8) and (9) by using the self-reported frequency of body comparison as a predictor of perceptions of friends, and we point out how the ICCs and slopes provide statistically equivalent information from (6). Finally, we added participants' scores on the Drive for Thinness subscale as an additional person-level predictor, first its main effect and then its interaction to explore the moderating influence of participants' drive for thinness on perceptions of friends. We then fit SRMs to remaining rows in Table 1, again building models with self-reports as predictors and adding drive for thinness to test moderation.

Software for estimation.. Among the family of MCMC algorithms used in Bayesian modeling software, perhaps the most commonly applied is Gibbs sampling, as implemented in the freely available software OpenBUGS (Lunn et al., 2009) and JAGS (Plummer, 2015). Due to the complexity of estimating a correlated, cross-classified multilevel model with missing data, we instead relied on the No-U-Turn Sampler (NUTS), a special case of Hamiltonian Monte Carlo (HMC) implemented in the software Stan (Carpenter et al., 2017). Stan is available in the R (R Core Team, 2017) package `rstan` (Stan Development Team, 2016a). Whereas Lüdtke et al. (2013) described sequentially sampling each individual element of $\boldsymbol{\theta}$ from $P(\boldsymbol{\theta} \mid \mathbf{Y})$, conditional on previously sampled parameters (i.e., Gibbs sampling), NUTS (and HMC in general) simultaneously samples the entire vector $\boldsymbol{\theta}$ by simulating it as a point in k-dimensional space, where $k = 2N + 6$ (the length of $\boldsymbol{\theta}$). Estimation with NUTS provides speed and efficiency advantages compared to Gibbs sampling, particularly in models with highly correlated parameters (Monnahan et al., 2017).

Model diagnostics.. We used three independent chains to estimate each model, running 500 warmup (burn-in) iterations before saving 500 samples from the posterior in each chain, yielding a sample of 1500 to estimate the *M*, *SD*, and 95% BCI for each parameter. To monitor convergence, we inspected trace plots to verify the three independent chains mixed well (i.e., all chains converged on the same posterior distribution). We also inspected Gelman and Rubin's (1992) potential scale-reduction factor (R-hat), where R-hat < 1.10 (preferably close to 1) indicates adequate mixing across independent chains.

We also inspected the effective sample size (N_{Eff}) for individual parameters and a multivariate N_{Eff} (Vats et al., 2016), as provided in the R package `mcmcse` (Flegal et al., 2016), for the set of parameters about which we drew inferences. Because MCMC produces autocorrelated parameter estimates, the estimated posterior distribution using 1500 samples contains less statistical information than it would if the parameters were not autocorrelated at all. N_{Eff} is the estimated number of unautocorrelated samples required from the posterior that would yield the same amount of information as the 1500 autocorrelated parameters that were drawn.

Some heuristic guidelines have been proposed for the required minimum N_{Eff} (e.g., a few hundred) to have confidence in the accuracy of posterior estimates, although Gong and Flegal (2016) assert that the alter N_{Eff} should depend on the dimensionality of the posterior (i.e., how many parameters are estimated) and the desired level of precision (i.e., size of Monte Carlo *SE*). Monte Carlo *SE* reflects the degree to which the posterior *M* is expected to vary between model fittings due to sampling variability. When the posterior *M* and *SD* (or BCI) are used to draw an inference about each parameter, the Monte Carlo *SE* should be small relative to the posterior *SD*. Rearranging the familiar formula for the *SE* of an estimated mean ($SE_{\text{MC}} = SD/\sqrt{N_{\text{Eff}}}$), N_{Eff} can be interpreted as the ratio of the posterior variance to the Monte Carlo sampling variance of the posterior *M* (i.e., $\sqrt{N_{\text{Eff}}} = SD/SE_{\text{MC}}$, so $N_{\text{Eff}} = SD^2/SE^2$). Thus, if $N_{\text{Eff}} = 100$ (or 400), the Monte Carlo *SE* is only $1/\sqrt{N_{\text{Eff}}} = 10\%$ (or 5%) as large as the posterior *SD*. Given the same posterior *SD*, the posterior *M* and BCI limits (and any inferences drawn) should therefore remain similar.

Parsimony-adjusted fit measures are also provided via the Watanabe–Akaike (or "widely applicable") information criterion (WAIC; Watanabe, 2010), which is a Bayesian analog of AIC. Model complexity is "punished" in the AIC by subtracting the number of estimated parameters (k) from the log-likelihood (ℓ): AIC = $\ell - k$. In the familiar "deviance" metric,[3] this quantity is multiplied by negative 2, in which case the -2ℓ indicates poorness of fit, and model complexity is punished by adding twice the number of parameters (i.e., AIC = $-2\ell + 2k$). In a Bayesian framework, ℓ for varies across $m = 1, 2, \ldots M$ samples of the parameter vector (θ_m) from the posterior distribution. For each of $d = 1, 2, \ldots D$ observed dyads, the average likelihood across M posterior samples is calculated, and the natural log is taken (Vehtari et al., in press, Eq. (3)):

$$\ell_{\text{post},d} = \log\left(\frac{1}{M}\sum_{m=1}^{M} p\left(Y_d \mid \boldsymbol{\theta}_m\right)\right). \tag{12}$$

The D values from (12) are then summed across dyads, resulting in an overall mean log posterior density for the model: $\ell_{\text{post}} = \Sigma\ell_{\text{post},d}$.

Likewise, the number of parameters (k) in a Bayesian framework is not as simple as counting model parameters of interest; instead, the effective number of parameters (k_{Eff}) is estimated, which is not generally an integer quantity. For example, each imputed missing value and random effect is also an estimated parameter, and stronger prior information on any parameter can decrease k_{Eff}. To calculate k_{Eff}, each dyad's ℓ across M posterior samples is calcu-

[3] The deviance metric is popular because differences in the deviances of nested models are distributed as χ^2 random variables with df = the difference in number of estimated parameters, so we report WAIC in a deviance metric.

lated, then the variances are summed across dyads (Vehtari et al., in press, Eq. (11)):

$$k_{\text{Eff}} = \sum_{d=1}^{D} \text{Var}_{m=1}^{M}(\log[p(Y_d|\boldsymbol{\theta}_m)]). \tag{13}$$

Analogous to the formula for AIC, WAIC is calculated as $\ell_{\text{post}} - k_{\text{Eff}}$ (Vehtari et al., in press, Eq. (13)), where $\ell_{\text{post}} = \Sigma\ell_{\text{post},d}$. We report WAIC in the deviance metric, due to its popularity: $\text{WAIC} = -2\ell_{\text{post}} + 2k_{\text{Eff}}$.

Like AIC, models with lower WAIC values are preferred, but the *SE* for the difference (Δ) between models' WAICs gives an indication of whether the difference exceeds what could be expected due to sampling error alone (Vehtari et al., in press, Eq. (24)). The *SE* for ΔWAIC (in deviance metric) between fitted Models A and B is calculated as twice the square-root of the number of dyads times the variance (across dyads) of differences between quantities in (12) and (13):

$$SE_{\Delta\text{WAIC}} = 2 \times \sqrt{D \times \text{Var}_{d=1}^{D}\left[\left(\ell_{\text{post},d}^{\text{A}} - k_{\text{Eff}}^{\text{A}}\right) - \left(\ell_{\text{post},d}^{\text{B}} - k_{\text{Eff}}^{\text{B}}\right)\right]}. \tag{14}$$

Prior distributions.. An advantage of HMC over Gibbs sampling is that conjugate priors are not required, so we were free to specify intuitive prior distributions even for standard deviations and correlations. We specified noninformative or weakly informative prior distributions for all parameters, although the specific distribution varied across parameters.

Modeling the covariate as an auxiliary, the distribution in (6) served both as the likelihood with respect to observed values of self-reports and as the prior for estimates of missing self-reports and for the ego and alter effects; its covariance matrix and the mean of the auxiliary were thus hyperparameters. We specified priors separately for the *SD*s (σ) and the correlation matrix of person-level effects.

For the correlation matrix we specified a noninformative prior distribution on the entire correlation matrix simultaneously, using an "LKJ" prior (named for Lewandowski et al., 2009). An LKJ distribution has a single shape parameter (η) that determines how informative the prior is. We specified an LKJ prior with $\eta = 1$, indicating the expected value was an identity matrix, with the probability density spread uniformly over all positive-definite matrices. This effectively represents a uniform prior between −1 and +1 for each individual correlation in the matrix (Stan Development Team, 2016b). An LKJ prior becomes more informative as η deviates from 1. When $\eta > 1$, the identity matrix has the highest probability density, and nonzero correlations become less probable as η increases. When $0 < \eta < 1$, the identity matrix has the lowest probability density of all positive-definite matrices, and nonzero correlations become more probable as η decreases.

For the *SD*s we specified uniform distributions between zero and half of the range of the variables. A variable's largest possible *SD* would be observed when half of the observations are at one extreme and the other half are at the other extreme, in which case the *SD* would be half of the distance between the minimum and maximum values. For example, peer perceptions and self-reports cannot have a larger *SD* than 2 because they range from 1 to 5. Because we had no dyad-level covariates, there was only one correlation to estimate in (5), for which we specified a uniform prior between −1 and +1, and a uniform prior between 0 and 2 for the residual *SD*.

Because we mean-centered the self-reports, μ_X in (6) was given a normally distributed prior with $\mu = 0$ and $\sigma = 5$ (half the range of the self-reports). The prior for the grand mean μ was specified as uniform over the range of the data, because the mean could only take values in that range.

Modeling the covariate(s) as predictor(s) required the distribution specified in (9) in place of (5) in the previous description. Because the covariate(s) distribution is independent of how the random-effect residuals are distributed, priors were specified separately for the submatrices of random-effect residuals and of the covariate(s), but we used the same procedure to specify uniform priors for all *SD*s and correlations. Because a regression slope is the change in the outcome per unit-change in the predictor, the prior distributions for regression slopes in (8) were specified as normal with $\mu = 0$ and σ equal to the ratio of the range of the outcome to the range of the predictor. For example, the range of the peer perceptions is $5 - 1 = 4$ and the range of corresponding self-reports is $2 - (-2) = 4$, so the prior σ for those regression slopes was $4/4 = 1$. We consider these "weakly informative" priors that are only informative enough to prevent the sampler from exploring irrelevant parts of the parameter space, but not so informative as to restrict the sampler from exploring relevant values. We also fit the model using "diffuse" priors (i.e., normal with $\mu = 0$ and $\sigma = 10$, or $\sigma^2 = 100$) and observed similar results.

Results

For all models, the SRM parameter estimates are reported in Tables 2–4 but the *M*

and *SD* of self-reports are excluded because they are not of substantive interest. BCIs are included as indicators of statistical significance in order to test null hypotheses that population effects are zero. In Table 2 (covariates as auxiliary correlates), R-hat and N_{Eff} are reported for each parameter, along with multivariate N_{Eff} for the full model. Due to spatial constraints, only multivariate N_{Eff} is reported for each model in subsequent tables (comparing models with additional covariates as substantive predictors).

Covariates as auxiliary correlates

The grand mean (μ) is the average perceived frequency of celebrity body talk by friends. Table 2 shows the EAP (i.e., the posterior mean) estimate of μ was 1.94 (close to the arithmetic mean of the observed outcome), 95% BCI [1.81, 2.07]. This indicates that on average, respondents perceived their friends as mentioning celebrities' bodies "once in a while" (response category 2).

The ego effect (E_i) is how different person *i* perceived their friends' body talk frequency to be from μ, on average (i.e., regardless of which *j* the responses were about). The EAP estimate of the *SD* (σ_E) represents how much individuals (*i*) typically differ in how they tend to perceive others' (*j*) body talk frequency. As shown in Table 2, this ego effect explains ICC = 36% of the overall variability in perceptions of friends' celebrity body talk frequency. The penultimate row of Table 2 shows that ego effects are positively correlated to a moderate-to-high degree with their own self-reported body comparison frequencies (EAP $\rho_{EX} = 0.43$, 95% BCI [.22, 0.62]). This is evidence that the ego effect represents the degree to which women project their own behavior onto their peers. That is, the more women compare their own bodies to celebrities' bodies, the more they tend to report that their friends talk about celebrities' bodies.

The alter effect (A_j) is how much the perception of friend *j*'s celebrity body talk differs from μ, on average (regardless of which *i* is indicating their perception of *j*). The alter effect can be described as a *consensus effect* because it is the consensus among the peer network (i.e., every *i* who provided ratings about *j*) about person *j*'s celebrity body talk. The EAP estimate of the *SD* (σ_A) indicates how much individuals (*j*) typically differ in how their celebrity body talk tends to be perceived by others (*i*). Table 2 shows that consensus only explains 15% of the overall variability in perceptions of friends' celebrity body talk. If the perceptions of the peer environment are accurate, then these mean perceptions about person *j*'s perceived body talk frequency should correlate with person *j*'s self-reported body comparison frequency (assuming self-reports

Table 2
Parameter estimates for SRM of friends' celebrity body talk and correlations with self-report.

	Parameter	EAP	*SD*	95% BCI Lower	95% BCI Upper	R-hat (N_{Eff})
	Grand Mean μ	1.923	0.066	1.788	2.055	1.005 (411)
Variance Components:	Dyad (residual) σ_R	0.700 (54%)	0.021	0.659	0.742	1.004 (629)
	Ego σ_E	0.570 (36%)	0.053	0.473	0.682	1.003 (585)
	Alter (consensus) σ_A	0.372 (15%)	0.040	0.289	0.453	1.006 (578)
Correlations	Dyadic Reciprocity (ρ_R)	**0.263**	0.092	0.070	0.430	1.008 (383)
	Generalized Reciprocity (ρ_{EA})	−0.113	0.165	−0.424	0.221	1.003 (369)
	Projection (ρ_{EX})	**0.428**	0.102	0.212	0.612	1.001 (806)
	Accuracy (ρ_{AX})	0.114	0.149	−0.173	0.414	1.002 (521)

Note. EAP = expected *a posteriori* estimate (the *M* of the posterior distribution). *SD* = the *SD* of the posterior distribution. BCI = the Bayesian credible interval. The multivariate N_{Eff} = 748.

Table 3
Building an extended SRM of celebrity-body talk with self-reports as predictors.

	Parameter	Model 1	Model 2	Model 3
Fixed	μ	1.914 [1.80, 1.99]	1.906 [1.78, 1.99]	1.919
(Ego)	Self-Report (β_1)	**0.221** [0.11, 0.33]	**0.232** [0.09, 0.37]	**0.237**
	Drive-Thin (β_2)		−0.031 [−0.14, 0.08]	−0.002
	Interaction (β_3)			−0.002
(Alter)	Self-Report (α_1)	0.036 [−0.07, 0.14]	−0.002 [−0.02, 0.01]	−0.025
	Drive-Thin (α_2)		**0.017** [0.002, 0.03]	**0.017**
	Interaction (α_3)			−0.002
Random	Dyadic ρ_R	**0.277** [0.11, 0.44]	**0.285** [0.09, 0.45]	**0.273**
	Generalized ρ_{EA}	−0.216 [−0.52, 0.12]	−0.184 [−0.49, 0.17]	−0.188
	Residual σ_R	0.699 [0.66, 0.74]	0.701 [0.66, 0.74]	0.700
	Ego σ_ε	0.514 [0.42, 0.62]	0.510 [0.42, 0.62]	0.515
	Ego ΔR^2	19.0% [4.0, 37.0]	20.5% [5.3, 38.5]	
	Alter σ_δ	0.370 [0.29, 0.46]	0.347 [0.26, 0.44]	0.344
	Alter ΔR^2	1.8%[a] [0.0, 16.0]	16.4% [1.6, 39.0]	
N_{Eff}[b]		626	682	583
Fit	WAIC (pD)	3586.0 (442.5)	3592.6 (444.3)	3591.5 (442.9)
	ΔWAIC (*SE*)		6.6 (6.2)	1.1 (5.6)

Note. EAPs are reported along with BCIs in brackets. BCIs excluded from Model 3 to save space, but adding the nonsignificant interaction terms did not substantially affect other estimates.
[a] Posterior median reported instead of mean (EAP) due to extreme skew.
[b] Multivariate effective sample size.

Table 4
Building an extended SRM of friends' self-body talk with self–friend comparisons as predictors.

	Parameter	Model 1	Model 2	Model 3
Fixed	μ	1.982 [1.94, 2.00]	1.982 [1.93, 2.00]	1.974
(Ego)	Self-Report (β_1)	**0.219** [0.06, 0.37]	**0.176** [0.003, 0.36]	0.177
	Drive-Thin (β_2)		0.009 [−0.01, 0.03]	0.006
	Interaction (β_3)			0.015
(Alter)	Self-Report (α_1)	0.06 [−0.07, 0.19]	−0.010 [−0.16, 0.14]	0.014
	Drive-Thin (α_2)		0.014 [−0.002, 0.03]	0.008
	Interaction (α_3)			0.016
Random	Dyadic ρ_R	0.083 [−0.13, 0.29]	0.076 [−0.13, 0.28]	0.050
	Generalized ρ_{EA}	0.327 [−0.04, 0.65]	0.293 [−0.14, 0.65]	0.321
	Residual σ_R	0.764 [0.72, 0.81]	0.767 [0.73, 0.81]	0.770
	Ego σ_ε	0.585 [0.47, 0.71]	0.592 [0.48, 0.73]	0.562
	Ego ΔR^2	12.1% [0.8, 28.1]	14.1% [2.4, 30.4]	
	Alter σ_δ	0.315 [0.22, 0.41]	0.279 [0.17, 0.39]	0.237
	Alter ΔR^2	3.5%[a] [0.0, 28.8]	22.6% [1.2, 59.8]	
N_{Eff}[b]		723	624	508
Fit	WAIC (pD)	3575.2 (404.8)	3585.3 (402.9)	3586.3 (397.4)
	ΔWAIC (*SE*)		10.1 (6.2)	1.0 (7.8)

Note. EAPs are reported along with BCIs in brackets. BCIs excluded from Model 3 to save space, but adding the nonsignificant interaction terms did not substantially affect other estimates.
[a] Posterior median reported instead of mean due to extreme skew.
[b] Multivariate effective sample size.

are a valid criterion for accuracy). The last row in Table 2 shows that the data provide no support for this hypothesis, EAP ρ_{AX} = 0.11, 95% BCI [−0.18, 0.39]. Combined with the fact that the ego effect explains more than twice as much variance as the consensus effect, the results suggest peer perceptions are not accurate, or at least that there is low self–other agreement. Instead, peer perceptions seem to be more a reflection of the ego than of the perceived alter.

A positive correlation between person *i*'s ego and alter effects would indicate that if a young woman tends to perceive more (or less) frequent celebrity body talk among her friends, then her friends also tend to perceive more (or less) frequent celebrity body talk in her. A negative correlation would indicate the opposite: if a young woman tends to perceive more frequent celebrity body talk among her friends, then her friends tend to perceive less frequent celebrity body talk from her. Table 2 indicates that the correlation was negligible (EAP ρ_{EA} = −0.10) and indistinguishable from zero, 95% BCI [−0.42, 0.22]. There appears to be no relationship between a young woman's perception of others' celebrity body talk and others' perception of her celebrity body talk.

Controlling for their ego and alter effects, a positive dyadic reciprocity would indicate that if person *i* perceives person *j* to talk especially often about celebrities' bodies, then person *j* would also perceive person *i* to talk especially often about celebrities' bodies. Table 2 indicates that this is the case (EAP ρ_R = 0.27) to a near-moderate degree, 95% BCI [.09, 0.44].

Covariates as substantive predictors

We fit the extended SRM in (8) to celebrity body talk again, but using self-reported celebrity body comparison as the predictor of perceptions of friends' celebrity body talk. Rather than estimating correlations with ego and alter effects, the self-reports explained ego and alter effects. Table 3 shows a significantly positive effect of egos' self-reported celebrity body comparison frequency on their perception of friends' celebrity body talk frequency, EAP β = 0.22, BCI [0.11, 0.33]. Self-reports explained about 19% (BCI [4.0, 37.0]) of the variance in ego effects, consistent with the significant positive correlation we estimated in the previous model, whose squared estimate ($.426^2$ = 18.1%) is the same magnitude as the estimated pseudo-R^2 in this model, and the direction of the correlation is available via the sign of the slope (i.e., both are positive). Thus, the same information in the auxiliary-correlates model is available indirectly in the regression model. Consistent with the results in Table 2, Table 3 shows no significant effect of alters' self-reports on consensus, EAP α = 0.04, BCI [−0.07, 0.14]. Likewise, the regression model revealed significant dyadic reciprocity (EAP ρ_R = 0.277, similar to 0.272 from Table 2) but no significant generalized reciprocity.

Drive for thinness was added to the model, but it was not a significant predictor of the ego effect, barely raising the pseudo-R^2 from 19% to 20.5%. Greater drive for thinness was, however, significantly associated with greater perceived body talk frequency by others, EAP β = 0.02, BCI [0.002, 0.03], which explained about 16% of variance in the consensus effect. The WAIC increased, indicating the simpler model should be favored, but the change in WAIC was not much more than its estimated *SE*. There was no evidence of an interaction between self-reported body comparison and drive for thinness on either the ego or alter effects, so the relationship between self-reported body comparison and perception of friends' body talk does not appear to be moderated by drive for thinness.

Perceptions of friends' self body talk.. We fit the same predictive models to the variables in the second row of Table 1. Table 4 shows that women who self-reported more frequently comparing their own body to their friends' bodies also tended to perceive their friends as talking more about their own bodies, EAP β = 0.22, BCI [0.06, 0.37], which explained about 12% of the variance in ego effects, BCI [0.8, 28.1]. Women's self-reported comparisons were not significantly related to how frequently they were perceived by their friends to talk about their bodies. Thus, there is evidence of projection, just as there was for the questions about celebrity body talk. We had no evidence, however, that drive for thinness was related to either individual ego or alter effects.

Discussion

The current study sought to examine whether perceptions of peers' behaviors are related more to the characteristics of the ego (perceiver) or to the characteristics of the alters (targets). Across two types of appearance-related conversation, we consistently observed much stronger ego effects than alter effects (i.e., more assimilation than consensus; Kenny, 1994). Women's own self-reported body comparisons were associated with their perception of friends' body talk. We did not observe a relationship between self-reports and alter effects; that is, women's self-reported body comparisons were not associated with how often they were perceived by friends to in engage celebrity- and self-body talk. These results suggest that perceptions of peers are more related to the characteristic of the perceiver rather than the characteristics of the peer being perceived. These findings replicate and extend those of Paxton et al. (1999) who found that perceptions of friends' body image concerns, but not friends' self-reported body image concerns, were associated with girls' body image concerns. Furthermore, we did not find evidence that a woman's drive for thinness (her attitude) interacted with her own self-reported body comparisons (her behavior) in explaining variance in her perceptions of others' behavior. Thus, ego effects do not appear to be dependent upon one's own attitudes, indicating that any bias in perception affected all women similarly, regardless of their level of drive for thinness.

Our results indicate that perceptions of the peer environment are colored by an individual's own behaviors, consistent with findings in the substance abuse literature (Bauman and Fisher, 1986; Iannotti and Bush, 1992). This may reflect the salience of appearance-related conversations to women who engage in body comparison, consistent with prospective data suggesting that higher body image concern predicts increased perception of peers' influence on body image concern (Rayner et al., 2013a). The ego effect could reflect conversation topics among women, such that women who engage in body comparison are more likely to discuss the bodies of celebrities and models with friends. This second interpretation is consistent with the dyadic reciprocity observed when examining celebrity body talk. However, dyadic reciprocity was not observed for women's perceptions of each other's self-body talk. Thus, this ego effect may instead reflect a cognitive bias such that women assume others engage in similar behaviors as they do (Rayner et al., 2013a). This has important implications for body image intervention efforts. Challenging the belief that particular behaviors or attitudes are common and normative may aid women in challenging the thin ideal, which may result in decreased body image concerns.

We found some evidence that ratings of the alter's behavior (i.e., consensus effects for body talk) were associated with the alter's own attitudes. Specifically, the greater a woman's drive for thinness, the more frequently she was rated as talking about celebrities' bodies, suggesting that women are able to detect individual differences in peers' attitudes. This provides some evidence of the validity of the drive for thinness construct and scale; a woman's drive for thinness manifests in her talk in a way that is observable by a woman's close friends. Although research suggests that women who talk about their own bodies endorse greater levels of drive for thinness (Salk and Engeln-Maddox, 2011), the drive for thinness association was not replicated when examining talk about one's own body. One interpretation of this finding is that the norms of self-presentation may be different in this particular social sorority relative to other studied populations. That is, it is possible that it is more normative to talk about celebrities' bodies than one's own body. Alternatively, we may have been underpowered to detect the association between drive for thinness and ratings of alters' behaviors. The general lack of association with alter effects has important methodological implications, and future research examining mechanisms of peer influence should assess both the women of interest and their nominated friends' attitudes and behaviors.

The study benefited from the social relations model and the availability of data on both the ego and alter(s). We examined a relatively common and frequent behavior—social comparison, a behavior which over 90% of college women endorse (Ridolfi et al., 2011) on average three to four times per day (Fardouly et al., 2017; Myers et al., 2012). The use of a social sorority allowed for the assessment of a bounded network, although women in the sorority likely interacted with, and had friendships with, women outside of the sorority. Thus, we were unable to examine how influences outside the sorority (e.g., non-sorority friends, family, campus groups) may have moderated observed effects. If so, this would constitute an MNAR mechanism, and estimates of variance components and correlations may not generalize to female college students' close female friends in general.

This applied example was not without limitations and should be considered when interpreting results. Although our results mirror those found in younger girls (Paxton et al., 1999), it is unknown to what extent these results generalize to young girls who are most at risk for the onset of body image concerns. We focused on a nonclinical population and results may not generalize to those with clinical eating disorders. Indeed, previous research suggests that women with eating disorders are able to assess others' preferred body types with the same accuracy as women without eating disorders (Benninghoven et al., 2007). Our sample was predominantly Caucasian, college aged, and completely female; future work should replicate these findings in more diverse samples. Data on weight and body mass index were unavailable; thus, we were unable to adjust for the influence of higher body weight in drive for thinness. We focused on just one aspect of peer and media influences on body image concerns (social comparisons). The larger literature suggests many other peer characteristics (e.g., weight) play a role in the development of body image concerns and eating pathology (Blanchflower et al., 2009; Costa-Font and Jofre-Bonet, 2013), as do genetic and other biological influences (e.g., weight; Keski-Rahkonen et al., 2005; Wade et al., 2003). Finally, our measure of self-reported behavior (self-reported comparison) did not match perfectly with our observable behavior (appearance-conversations), so the relationship between perceptions and self-reports may not have been as strong as if we measured self- and peer-responses to identical questions. Previous research supports the assumption that those who engage in covert comparisons are more likely to engage in appearance-related conversations (Corning and Gondoli, 2012). However, the lack of perfect correspondence between self- and peer-response questions may have attenuated the relationship we tried to detect, effectively lowering our power to detect any real effects.

Missing data may also be responsible for attenuated power. The 90 sampled women who provided self- and peer-reports contributed information to the estimation of both ego and alter effects, their correlation, and their associations with self-reported behaviors and attitudes. The remaining 72 women provided information only about consensus, but no other information about how consensus is related to other phenomena. If covariate information on the nonparticipants had been available, the uncertainty due to missing data (the *fraction missing information*; Enders, 2010) would have been reduced, resulting in more precise 95% BCIs and greater power, and could have provided greater support for assuming a MAR mechanism. However, without access to more person- and dyad-level information, it is impossible to assess whether or to what degree the nonparticipants in the sorority differed substantially from the participants, or whether asymmetric ties among participants differed substantially from observations in symmetric ties.

Taken together, these results highlight the importance of methodology when studying peer influences on body image concerns. Methodology that incorporates both perception ratings and peer self-report is needed to better understand how the peer environment influences body image concerns. A more objective criterion for accuracy than self-reports would also have been preferable; however, as Kenny (1994, p. 134) pointed out, objective observations would not be possible for internal behavior such as body comparison, so self-reported frequency of body comparison may be the best accuracy criterion we could hope for. But if self-reports had concerned the exact same question asked about peers (i.e., frequency of body talk), then objective measures may have been possible, although difficult. Furthermore, Kenny (1994, ch. 7) also discusses many different types of accuracy, which are estimable when the criterion is observed not just once per person (as in self-reports) but once for every person's interaction with each other person. In the context of our study, this would have required measuring a woman's frequency of body talk with each friend, in order to decompose those scores the same way perceptions are decomposed in (1). This would have allowed us to estimate how strongly each respective variance component (i.e., ego, alter, and relationship) was correlated between perception and behavior (i.e., perceiver accuracy, generalized accuracy, and dyadic accuracy; Kenny, 1994).

Results also have important implications for etiological models of body image concerns, suggesting that perceptions of the peer environment, rather than the peer environment itself, play a larger role in the development of body image concerns. Interventions intended to correct norms, such as social-norms interventions used to decrease substance use (Lewis and Neighbors, 2006) or interventions intended to decrease body comparisons, may assist in disrupting the processes that increase women's body image concerns.

Methodological conclusions

Our method for handling missing data was to treat missing values as unknown parameters to estimate (data augmentation). Although this method has been validated in standard multilevel modeling contexts (Enders et al., 2016), it has yet to be validated in the particularly complex context of dependent cross-classification, which is how the SRM is conceptualized as a multilevel model. Future simulation research is therefore necessary to verify the generality of our assumptions. Data augmentation is a commonly applied method for multiple imputation of missing data (Enders, 2010; Little et al., 2014), which allows statistical analyses that require complete data to be utilized even when some data are missing. Validation of the method described here would therefore allow its use as an imputation model for social network data. Future development could extend this method to impute discrete variables, so that exponential random graph models could be used to explain network structure even with incomplete data.

In addition to multiple imputation of missing data, maximum likelihood provides unbiased estimates under a MAR mechanism. Some recent software advances allow the SRM to be estimated using full-information or restricted maximum likelihood. For example, Brunson et al. (2016) implement a planned missing data design (thus, MCAR data) and specify a SRM for two variables (neuroticism and emotional support) simultaneously, as well as using ego and alter effects to predict self-reports. They estimate the model using full-information maximum likelihood in the experimental software package xxM (Mehta, 2013) for *n*-level structural equation modeling. In our online supplementary materials,[4] we provide syntax to fit our model in both rstan and xxm. Nestler (2016) also recently developed restricted maximum likelihood estimation for mixed-model specifications of SRM, which he intends to implement in the R package TripleR (Schönbrodt et al., 2012). Future research should compare these estimation methods for handling missing data with the data augmentation outlined here.

When the MAR assumption is not tenable, a pattern-mixture model would probably not be applicable to MNAR data with a complex network structure. However, a selection model to predict whether values are missing might be incorporated into the SRM, which could provide a viable method for analyzing MNAR round-robin or network-tie data. The development of such a hybrid model was beyond the scope of the current article, but we encourage its development in the future.

The methods described for accommodating missing round-robin data also provide the opportunity to fit the SRM to network ties only, potentially without having to define network boundaries. We advise researchers considering these options to consider the delicate issue of recording valid data for their research questions, which may require gathering person- and dyad-level covariate data, especially about nonparticipants or out-of-network nominees, if applicable. Asking participants potentially relevant dyad-level information about each alter they rate may also provide useful auxiliary information necessary to justify a MAR assumption. We hope our consideration of this novel approach stimulates more discussion of these missing-data issues. Instructional articles would be particularly helpful in guiding future research, such as extending Thoemmes and Mohanös (2015) graphical representation of missing-data mechanisms to missing network data.

Future applications

The methodological contribution and illustrative application herein will hopefully inspire new research questions, study designs, and analysis opportunities. We conclude by sharing some examples we have encountered in our own experience collaborating with colleagues who either have ideas for data collection or have already collected network data without knowing how to fully exploit it to answer additional research questions about interpersonal perceptions among network ties. In speed-dating contexts, researchers could test whether daters' partner preferences (e.g., personality, demographic characters) correspond with their partners' actual characteristics, specifically among daters who report a "match" or reciprocate interest in dating. Social media researchers could test whether ego's network position (e.g., centrality, connectedness) corresponds to network alters' use of ego's hashtags, thus linking two measures of influence. Health researchers could explore the degree to which ego's health behavior is associated with the emotional closeness of alters engaging in the same behavior. In intensive health settings, where patients are seen by a treatment team (e.g., therapists, nurses, doctors), the therapeutic alliance could be modeled in new ways. Individual patients' perceptions of individual members of their treatment team could be partitioned into ego and alter effects, and by treatment team role. SNA using weighted ties and partner perceptions have long needed an analysis strategy that could fully account for the statistical challenges inherent to these designs (Valente, 2010). The present study proposes the SRM can be used as just such a tool.

[4] The online Appendix for this article includes only the software syntax. The syntax files and data files are available to download from the first author's Open Science Framework account, listed under a project associated with a conference presentation: https://osf.io/fmhg6/files/.

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Appendix A. Supplementary data

Supplementary data associated with this article can be found, in the online version, at https://doi.org/10.1016/j.socnet.2017.11.002.

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