The Confidence Interval that Wasn't: Bootstrapped "Confidence Intervals" in ℓ_1-Regularized Partial Correlation Networks

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Abstract

I shed much needed light upon the default measure of parameter uncertainty in network psychometrics; that is, "confidence intervals" (CI) computed from bootstrapping ℓ_1-regularized partial correlations. Due to the nature of the ℓ_1-penalty, however, bootstrapping does not provide an accurate sampling distribution. Although this has long been known in the statistical literature, I set out to determine whether the intervals can at least be considered *approximate*. In multiple regression, I first describe the fundamental tension between model selection and estimation consistency inherent to the ℓ_1-penalty—in the pursuit of sparsity, the sampling distribution of the non-zero coefficients is necessarily compromised which translates into coverage far below nominal levels. With the foundation laid, I proceed to investigate coverage for non-zero relations in partial correlation networks. At best, average coverage was around 0.65 for 90% CIs. With increasing sample sizes, average coverage decreased to 0.30, perhaps approaching 0 if larger sample sizes were explored. Further, coverage was heavily influenced by the mere position of an edge in the network, ranging from essentially 0 to 0.90, with an average of around 0.50. Meanwhile, for the same simulation conditions, simply bootstrapping the sample covariance matrix provided coverage at the nominal level. In light of the results, I then demonstrate how to judiciously use the bootstrap in both regularized and non-regularized networks: the former can provide a useful summary of data-mining, whereas the latter allows for making inference on network parameters. To ensure network researchers have the option of computing valid CIs, I implemented a non-regularized bootstrap for various types of partial correlations in the `R` package **GGMnonreg**.

Keywords: partial correlation network, confidence intervals, frequentist inference, bootstrap, ℓ_1-regularization

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> ...there is a substantial price to be paid for sparsity...
>
> — Leeb and Pötscher (p. 203, 2008)

In the social-behavioral sciences, network theory has emerged as an increasingly popular framework for understanding psychological constructs (Borsboom, 2017; Jones, Heeren, & McNally, 2017). The underlying rationale is that a group of observed variables, say, self-reported symptoms, are a dynamic system that mutually influence and interact with one another (Borsboom & Cramer, 2013). The observed variables are "nodes" and the featured connections between nodes are "edges." This work focuses on partial correlation networks, wherein the edges represent conditionally dependent nodes—pairwise relations that have controlled for the other nodes in the network (Epskamp, Waldorp, Mottus, & Borsboom, 2018). This powerful approach has resulted in an explosion of research; for example, network analysis has been used to shed new light upon a variety of constructs including personality (Costantini et al., 2015), narcissism (Di Pierro, Costantini, Benzi, Madeddu, & Preti, 2019), and hypersexuality (Werner, Štulhofer, Waldorp, & Jurin, 2018).

Recently, the foundation of network psychometrics was improved when the default methodology was revisited (Williams & Rast, 2019; Williams, Rhemtulla, Wysocki, & Rast, 2019). In the network literature, ℓ_1-regularization (a.k.a., "least absolute shrinkage and selection operator" or "lasso") emerged as the default approach for detecting conditionally dependent relations. One motivation for adopting lasso was by the thought that it reduces spurious relations. It was recently demonstrated, however, to have an inflated false positive rate that depends on many factors, including the sample size, edge size, sparsity, and the number of nodes (see Figure 6 in Williams et al., 2019). Further motivation was the thought that ℓ_1-regularization is needed to mitigate overfitting. This was shown to be overstated in Williams and Rodriguez (2020). In both cases, non-regularized methods were

more than adequate for the goals of reducing false positives and quelling concerns of overfitting.

In this work, I seek to further improve network analysis by shedding much needed light upon the default measure of parameter uncertainty, that is, "confidence intervals" (CI) that are computed from bootstrapping ℓ_1-regularized partial correlations (Epskamp, Borsboom, & Fried, 2018). However, due to the nature of the ℓ_1-penalty, bootstrapping does not provide an accurate sampling distribution. This is summarized in section 3.1, "Why standard bootstrapping and subsampling do not work," of Bühlmann, Kalisch, and Meier (2014):

> The (limiting) distribution of such a sparse estimator is non-Gaussian with point mass at zero, and this is the reason why standard bootstrap or subsampling techniques do not provide valid confidence regions or $p-$values (pp. 7-8).

This particular issue is most pressing for *true* zero and small relations (see section 3 in Knight & Fu, 2000). For the former, especially as the sample size increases, the bootstrap distribution will converge to a "spike" at zero, resulting in the CI covering zero too often. Indeed, there is proof that the standard errors (and thus intervals) are inconsistent for null associations (theorem 6.1 on p. 397, Kyung, Gilly, Ghoshz, & Casellax, 2010). On the other hand, when gradually moving away from zero, the distribution changes form. It is now compromised of "mixture of a singular normal distribution and of an absolutely continuous part" (p. 375, Kyung et al., 2010). In both cases, the distributions are far from normal which presents challenges for obtaining an accurate sampling distribution. This is illustrated in Figure 1 (panel A).

Pragmatically, it may be tempting to think covering zero too often is not problematic, given this translates into fewer type I errors. However, the issue surrounding null associations hints at a deeper problem with the estimator. A limitation of lasso is that the penalty increases linearly with the size of the relation (p. 523, Fan, Feng, & Wu, 2009),

a peculiarity that does not diminish with more data. Accordingly, "it produces substantial biases in the estimates for large regression coefficients" (p. 18, Goeman, Meijer, & Chaturvedi, 2018). Hence, even for those edges that are clearly non-zero, the bootstrapping strategy may produce a severely compromised sampling distribution, thereby calling into question its usefulness for assessing parameter uncertainty.

Defining Confidence Intervals

At this point, it is important to consider the definition of a CI (a.k.a uncertainty interval, Gelman & Greenland, 2019). The basic idea is to construct an interval for a parameter of interest, including a lower and upper limit, such that, on average, it will cover the true value $100(1-\alpha)\%$ of the time (Neyman, 1937). Importantly, this is inherently a frequentist concept that refers to hypothetical replications (or future random samples) from the assumed population model. Notice that this definition does not privilege a particular value, rather, when using the CI for significance testing, this is merely inspecting whether zero is covered. By definition, however, *all* values within are not rejected at the chosen α level (see p. 7 in Kruschke & Liddell, 2015). Hence, when computing a CI with a particular procedure (including the estimation method), the implicit claim by the researcher is that "frequency of correct results will tend to α" (p. 349, Neyman, 1937).

Furthermore, in models with many effects, it is possible to infer the proportion of relations that will be covered. A network with 20 nodes has 190 partial correlations. With 90% CIs, the expectation is that 171 ($190 \cdot 0.90$) will be cover *true* value. Note again this is a long run average, but it indicates nonetheless that most intervals should contain the *true* value for a given sample, tending to $100(1-\alpha)\%$ of the relations.

Why it Matters. As an illustrative example, Figure 1 (panel B) includes 95% CIs for three partial correlations. The non-regularized CI for the relation between nodes A and B excluded zero (95% CI = [0.22, 0.38]), which is therefore "statistically significant" (it was not covered). Further, values less than 0.22 and greater than 0.38 can also be rejected

(they were not covered). Herein lies an issue with the ℓ_1 interval. Notice that it is almost completely in the rejection region of the valid CI. This means that the vast majority of values contained in the ℓ_1 based interval should be rejected. A point of emphasis is that lasso provides an estimate of the population value, yet almost the entire sampling distribution could be ruled out by a valid measure of uncertainty. Indeed, as noted in Waldorp, Marsman, and Maris (2019), "Once the parameters are obtained it turns out that inference on network parameters is in general difficult with ℓ_1-regularization" (p. 53).

Misconceptions About Confidence Intervals

From surveying the network literature, various misconceptions have emerged in an attempt to interpret the "CIs" computed from bootstrapping ℓ_1-regularized estimates. In my view, these are a by-product of the ℓ_1-penalty wreaking havoc on the sampling distribution (e.g., Figure 1).

In network psychometrics, researchers are advised against using regularized "CIs" for significance testing. The rationale is that ℓ_1-penalized estimates are biased towards zero, and thus an edge may differ from zero, even when it is included in the interval. Although this statements could be correct, it is important to note that the interpretation of a valid CI is not a function of which value the researcher is interested in rejecting. To make sense of this, consider inspecting the CI to determine whether, say, 0.1 is covered, which is a significance test for a non-nil null hypothesis. This again relates to coverage, in that significance testing with a CI is merely inspecting whether a value of interest is covered, with no special consideration given to zero. If moving the goal post compromises the CI, this hints at an underlying issue with the employed estimator and alternatives should be explored.

Further, there seems to be some confusion surrounding both bootstrapping and frequentist inference more generally. In Fried et al. (2019), it was stated that "'these [regularized] sampling distributions are not CIs centered on the true (unbiased) parameter

value" (in the supplementary material). In this context, bias is also a frequentist concept that is defined on average. Accordingly, for any given sample, the bootstrap sampling distribution (and the corresponding CI) of the sample estimate will not be centered on the *true* (and unknown) value. I refer to an excellent introduction to bootstrapping:

> Each bootstrap distribution is centred around the sample estimate, not the population value...Moreover, bootstrap CIs, like any other CIs, vary across experiments. Therefore, if we perform a single experiment, the CI we obtain does or does not contain [cover] the population value we're trying to estimate (p. 12 Rousselet, Pernet, & Wilcox, 2019)

This applies to both regularized and non-regularized estimators: regardless of which is used, or whether they are centered at the *true* value for a given sample, CIs are expected (within reason) to cover the *true* value $100(1-\alpha)\%$ of the time—the definition does not change when using lasso.

Revisiting the Regularization Literature

How could it be network psychometrics routinely employs a measure of uncertainty that leaves something to be desired? In my view, this is partially due to somewhat conflicting information in the statistical literature. For example, Hastie, Tibshirani, and Wainwright (2015), a definitive source for regularization, used the bootstrap in the section titled "Statistical Inference." Yet, when the bootstrap was employed, a CI was never computed and the full range of estimates was visualized in a box plot (Figure 6.4 therein).[1] Further, the bootstrap was also suggested in Tibshirani (1996, p. 272) and Tibshirani (2011, p. 281). Perhaps while strictly invalid, the bootstrap strategy can provide an *approximate* CI. This possibility is investigated with simulation.

There are few examples that use the bootstrap to compute CIs. The results are not very promising, in that "for nonzero true parameter values, the coverage *might* [emphasis

[1] Table 2.2 includes bootstrap standard errors.

added] be very poor" (p.541, Dezeure, Bühlmann, Meier, & Meinshausen, 2015). In Van De Geer, Bühlmann, Ritov, and Dezeure (2014), the de-sparsified lasso was compared to the residual bootstrap of Chatterjee and Lahiri (2011). For the latter, coverage of non-zero regression coefficients was often far below nominal levels (see the Tables on pp. 22 - 33). These approaches are specifically looking at high-dimensional data (e.g., $p < n$), where the maximum likelihood estimate does not exist and therefore regularization is necessary. In psychology, however, the more typical network includes around 20 variables and hundreds of observations (see Table 2 in Wysocki & Rhemtulla, 2019). In these situations (low-dimensional data), CIs are easily computed with non-regularized estimation (Drton & Perlman, 2004; Williams & Rast, 2019; Williams et al., 2019). This was noted in Javanmard and Montanari (2014):

> In classical [low-dimensional] statistics, generic and well accepted procedures are available for characterizing the uncertainty associated to a certain parameter estimate in terms of confidence intervals...(p. 2870).

Overview

In what follows, I delve into computing "CIs" via bootstrapping ℓ-regularized partial correlations, with the intent of fully understanding their coverage properties. To my knowledge, no such work has been done in the psychological literature. I begin with multiple regression and progress to partial correlation networks. These sections include focused numerical experiments, each of which are informed by the statistical literature. The goal is to determine whether bootstrapping regularized partial correlations is salvageable: given their ubiquity in network analysis, it would be ideal if they were not too far off the mark. By way of example, the next section provides recommendations for using the bootstrap in regularized and non-regularized networks.

The Gaussian Graphical Model

For multivariate normal data, the Gaussian graphical model (GGM) captures conditional relationships that are typically visualized to infer the underlying dependence structure (i.e., the partial correlation "network"; Højsgaard, Edwards, & Lauritzen, 2012; Lauritzen, 1996). There is an undirected graph that is denoted $G = (V, E)$, which includes a vertex set $V = \{1, ..., p\}$ and an edge set $E \subset V \times V$. The former refers to "nodes" and the set represents, say, items in a questionnaire, whereas the latter set contains the estimated network structure. Let $\mathbf{y} = (y_1, ..., y_p)^\top$ be a random vector indexed by the graph's vertices that is assumed to follow a multivariate normal distribution, $\mathbf{y} \sim \mathcal{N}_p(\mathbf{0}, \mathbf{\Sigma})$, where $\mathbf{\Sigma}$ is a $p \times p$ positive definite covariance matrix. I use $\mathbf{Y}$ to denote the $n \times p$ data matrix, where each row corresponds to the observations from a given individual. Further, without loss of information, the data are considered centered with mean vector 0.

The undirected graph is obtained by determining which off-diagonal elements of the precision matrix, $\mathbf{\Theta} = \mathbf{\Sigma}^{-1}$, are non-zero. That is, $(i, j) \in E$ when node i and j are determined to be conditionally dependent and set to zero otherwise. Note that the edges (or "connections") in a GGM are partial correlations $\rho_{ij \cdot \boldsymbol{z}}$ that are computed directly from $\mathbf{\Theta}$ with

$$\rho_{ij \cdot \boldsymbol{z}} = \frac{-\theta_{ij}}{\sqrt{\theta_{ii}\theta_{jj}}} \tag{1}$$

Hence, estimating partial correlation networks can be accomplished by testing whether each relation in Equation (1) is "significantly" different from zero. This is described in Drton and Perlman (2004) and Williams and Rast (2019), both of which relied on an analytic solution, whereas a more general alternative is to use the non-parametric bootstrap (Williams et al., 2019)

A Brief Note on Generality

In this work, I assume that the data are continuous and normally distributed, that is, multivariate Gaussian. Accordingly, I rely heavily upon the Pearson partial correlation coefficient to keep the exposition manageable. This does not limit the generality of this work, in that all ideas can seamlessly be applied to polychoric (Pearson, 1900), Spearman's rank (Kim, 2015), the so-called Gaussian rank estimator (i.e., based on Van Der Waerden scores, see references in Boudt, Cornelissen, Croux, & Boudt, 2012), and Kendall's tau based partial correlations (Johnson, 1979), each of which are commonly used in the Gaussian graphical modeling literature (Hoff, 2007; Liu, Han, Yuan, Lafferty, & Wasserman, 2012; Mohammadi & Wit, 2015). This far-reaching applicability is due to requiring only an estimate of the covariance matrix when bootstrapping the partial correlations.

Multiple Regression

I begin studying coverage in multiple regression. The relatively simple case of regression can provide a foundation to begin understanding why lasso ℓ_1 is problematic—-a motivating example of sorts. This is further justified by the direct correspondence between the elements of $\mathbf{\Theta}$ and multiple regression (Kwan, 2014; Stephens, 1998). Suppose that the jth column $\mathbf{Y}_j$ is predicted by the remaining $(p-1)$ nodes $\mathbf{Y}_{-j}$. For nodes i and j, the resulting coefficients and error variances are defined as

$$\beta_{ij} = \frac{-\theta_{ij}}{\theta_{ii}} \quad \text{and} \quad \sigma_j^2 = \frac{1}{\theta_{ii}}, \tag{2}$$

where i and j denote the corresponding row and column of $\mathbf{\Theta}$, β_{ij} is the regression weight for the jth node $(i \neq j)$, and σ_j^2 is the residual variance. This allows for recovering all elements of $\mathbf{\Theta}$ (and $\mathbf{\Sigma}$) with j multiple regression models. In relation to Equation (1), the

regression coefficients also have a direct mapping to the partial correlation, that is

$$\beta_{ij} = \rho_{ij \cdot \boldsymbol{z}} \sqrt{\theta_{ii}/\theta_{jj}}. \tag{3}$$

This relationship is often utilized in the GGM literature. For example, there are a variety of approaches that use multiple regression to estimate the elements of $\boldsymbol{\Theta}$ (Liu & Wang, 2017; Yuan, 2010) or that focus on the partial correlation matrix (Krämer, Schäfer, & Boulesteix, 2009). This is known as "neighborhood selection" (Meinshausen & Bühlmann, 2006).

In the familiar context of multiple regression, ℓ_1-regularization is similar to the ordinary least squares (OLS) solution, but with an added penalty to the residual sum of squares (RSS), that is,

$$\underbrace{\sum_{i=1}^{n} \left(y_i - \sum_{j=1}^{p} x_{ij} \beta_j \right)^2}_{\text{RSS}} + \underbrace{\lambda \sum_{j=1}^{p} |\beta_j|}_{\ell_1\text{-penalty}}. \tag{4}$$

In this equation, λ is the "tuning parameter" that determines the extent to which the penalty affects the estimates. When $\lambda = 0$, no penalty is imposed and the resulting estimates are equal to the OLS. When a very high value of λ is chosen, all the estimates will be pushed to zero. Thus, some criterion is typically used to choose the value of λ. The default choice in network psychometrics is the extended Bayesian information criterion (EBIC, Chen & Chen, 2008) . This is given by

$$\text{EBIC} = \underbrace{n \cdot \log\left(\frac{\text{RSS}}{n}\right) + k \cdot \log(n)}_{\text{BIC}} + 2 \cdot k \cdot \gamma \cdot \log(p) \tag{5}$$

where k is the number of selected parameters, n the sample size, p the number of predictors, and γ ($0 \leq \gamma \leq 1$) an additional hyperparamter (p. 3 in Chen & Chen, 2012). Note that, when $\gamma = 0$, Equation 8 reduces to the BIC. In network analysis, the focus is

typically on a conservative model that includes few false positives. Accordingly, the default is $\gamma = 0.5$ with the goal of providing a relatively sparse model compared to BIC, as a result of selecting a larger value for λ in Equation (4).

There is a potential problem, however, in that a fundamental tension exists between selecting the *true* model and parameter estimation. The theoretical results in Fan and Li (2001) demonstrated that the ℓ_1-penalty can be consistent for model selection and consistently estimate the parameters, but it cannot satisfy both properties *simultaneously* (see Theorem 2 and Remark 1 in Fan & Li, 2001). For the former, this requires that $\sqrt{n}\lambda \to \infty$, whereas, for the latter, root-n consistency requires that $\lambda = O(1/\sqrt{n})$ (p. 1353, Fan & Li, 2001). It should be noted that alternative penalties have been developed that can achieve both at the same time. I refer interested readers to Williams (2020a, see references therein).

This suggest that when erring on the side of caution there is a price to be paid—estimation accuracy and this cannot typically be overcome with more data. It follows that adding to BIC in the pursuit of sparsity, as in EBIC with $\gamma > 0$, will further compromise the non-zero parameter estimates due selecting a larger value for λ. This logic extends to the Akaike information criterion that will typically select a smaller value for λ than BIC. As a result, while there will be fewer relations pushed to zero, less harm is done to the parameter estimates themselves.

This should not be taken to mean that a more liberal information criterion should be used instead of EBIC (or BIC). Framing it this way highlights a general limitation of the ℓ_1-penalty that is particularly salient for computing CIs of non-zero relations. This is due to the sampling distribution of a consistent estimator concentrating around the *true* value with increasing data. Because the ℓ_1-based bootstrap sampling distribution will not necessarily concentrate around the population value, it is possible that coverage actually deteriorates as n increases. This insight is not entirely new:

> ...bootstrapping the Lasso does not lead to a consistent estimate of the

> underlying sampling distribution which in turn could be used for constructing confidence statements (p. 350, Buhlmann, 2017).

What remains to be determined, however, is just how inaccurate coverage is in low-dimensional settings that are common to the network literature.

Numerical Experiment 1

In this experiment, I intentionally focus on an unrealistic situation that can be understood as the best case scenario, that is, large coefficients that will be detected with more data, orthogonal covariates, and favorable signal-to-noise ratio (SNR). This is meant to satisfy two important assumptions of lasso for consistent model selection:[2] (1) the beta-min condition, which requires that "the non-zero regression coefficients are sufficiently large (since otherwise, we cannot detect the variables in S_0 [the active set or non-zero relations] with high probability)" (p. 1214, Bühlmann, 2012). In reference to Figure 1, this ensures that any issues are not driven exclusively by effects that have point mass at zero; and (2) the irrepresentable condition (IRC), such "that the total amount of an irrelevant covariate represented by the covariates in the true model is not to reach 1" (p. 2545 Zhao & Yu, 2006).[3] In other words, the correlation between relevant and irrelevant predictors is not too large, which is automatically satisfied with orthogonal covariates. Together, this experimental design allows for isolating the effect of ℓ_1-regularization on the sampling distribution.

The simulation procedure was as follows:

1. Set $\boldsymbol{\beta}_{1:10} = (0.1, 0.2 \ldots, 1)$ and $\boldsymbol{\beta}_{11:20} = (0, 0, \ldots, 0)$, such that the first 10 coefficients were non-zero and the last were *truly* zero. These non-zero values could all be detected with increasing data.

[2] There are additional assumptions of ℓ_1-regularization. Figure 1 in Van De Geer, Bühlmann, and others (2009) describes how they are (often) directly related to one another.

[3] The IRC was checked following Equation 2 in Zhao and Yu (2006), whereas satisfying the beta-min condition was inferred from the effects being detected.

2. Generate $p = 20$ variables $\mathbf{X} \sim \mathcal{N}(\mathbf{0}, \mathbf{I}_p)$, where $\mathbf{I}$ is a $p \times p$ identity matrix, thereby ensuring that the IRC is satisfied.

3. Set $\sigma = \sqrt{\frac{\boldsymbol{\beta}' \mathbf{I}_p \boldsymbol{\beta}}{1}}$, where 1 is the SNR. Note that $R^2 = \frac{\text{SNR}}{\text{SNR}+1}$, such that variance explained was 0.50.

4. Generate observations for $n = \{250, 500, 1,000\}$ from the model

$$y = \mathbf{X}\boldsymbol{\beta}' + \boldsymbol{\varepsilon},\ \boldsymbol{\varepsilon} \sim \mathcal{N}(0, \sigma) \tag{6}$$

5. Obtain the sampling distribution with a non-parametric bootstrap

6. Determine whether the *true* values were covered by 90% CIs. The CIs were computed as the 5th and 95th quantile of the bootstrap distribution.[4]

For ℓ_1-regularization, λ was selected with EBIC, including $\gamma = \{0, 0.5, 1.0\}$. The idea here is to show the effect of increasing the penalty (recall that larger γ values provide more regularization), with the expectation that coverage should get worse with larger values. To obtain the sampling distribution for each coefficient, a model was selected for each bootstrap sample, $b = 1, ..., 500$, resulting in the estimated coefficients for a given bootstrap sample (i.e., $\hat{\boldsymbol{\beta}}_b$). This procedure is described in Hastie et al. (2015, see Section 6.2). I also employed OLS regression with a non-parametric bootstrap. Data-driven model selection was *not* performed for each bootstrap sample. Thus, the bootstrap sampling distributions were obtained from the full, non-regularized, model. Coverage for 90% CIs was computed from 500 simulation trials.

All aspects of this work were implemented in `R` (version 4.0.2, R Core Team, 2017). The regularized regression models were fitted with `R` package **glmnet** Friedman, Hastie, and Tibshirani (2010) and the figures were made with **ggplot** (Wickham, 2016).

[4] Epskamp, Borsboom, and Fried (2018) suggested to use type 6 with the quantile function in `R`, whereas I used the default of type 7. The results do not change according to the method used for computing the quantiles.

Results. Figure 2 (panel A) includes intervals computed from 100 replications ($n = 1,000$, $\beta = 1$, $\gamma = 0.5$). The idea here is to further clarify the definition of a CI and frequentist inference more generally. Notice that the sampling distributions for OLS (denoted "Non-reg") are not centered around the *true* value for a given sample. Indeed, the estimates are larger or smaller than the *true* value, but hardly ever centered directly upon it. Even with 100 replications, coverage for the non-regularized estimator was close to the nominal level of 0.90, as indicated by 85% of the CIs covering the *true* value. The ℓ_1-based intervals, on the other hand, had extremely poor coverage: only 65% of the "CIs" covered the *true* value. By definition, this translates into rejecting the *true* value nearly a third of the time.

Figure 1 (panel B) includes coverage of the non-zero coefficients for all 500 simulation trials ($\gamma = 0.5$ is the default in psychology).[5] The box plot depicts the interquartile range and median coverage. Recall that, based on the tension between sparsity and parameter estimation, coverage should get incrementally worse with more penalization. This can be seen clearly, in that, with larger γ values, coverage was very low for lasso. Said another way, coverage deteriorates when moving away from non-regularized estimation. The sampling distribution is not only inaccurate when there is a point mass at zero, but even when the effects are easily detected. This is particularly striking because all of the standard assumptions were satisfied (for lasso in particular), the sample size was large ($n = 1000$), the coefficient was always selected, such that there was no mass at zero distorting the sampling distribution (e.g., Figure 1), and setting $\gamma = 0.5$ is the default in network psychometrics (coverage was never above 0.70).

There is, of course, the question of acceptable coverage. In the robust statistical literature, it is common to follow the guidelines of (Bradley, 1978). There it was suggested that "The most liberal criterion that I am able to take seriously is $0.5 \cdot \alpha \leq \rho \leq 1.5 \cdot \alpha$" (p.146 Bradley, 1978), where ρ is the actual error rate. With $\alpha = 0.10$ (90% CIs), this

[5] Coverage for the true zero coefficients was nearly 100%.

translates into coverage not being lower or higher than 0.85 and 0.95, respectively, which was never the case for lasso.

Numerical Experiment 2

This experiment aims to more directly understand the relation between λ and coverage. This was previously inferred from increasing γ in Equation (8). There has been considerable work investigating optimal regularization without selecting the tuning parameter. In Belloni, Chernozhukov, and Wang (2011), for example, it was shown that $\lambda_{TC} = \sqrt{\log(p)/n}$ is a theoretically consistent regularization parameter for the square root lasso. This value has also been used in GGMs (see Rong, Ren, & Chen, 2017; Wang et al., 2016). I adopt this approach and scale λ_{TC} such that there are 20 values ranging between 0 (OLS) and $2 \cdot \lambda_{TC}$. The simulation procedure is the same as above, but with only one sample size ($n = 1,000$).

Results. Figure 2 (panel C) includes coverage when fixing λ. The theoretically consistent value is $\lambda = 0.054$, which is positioned directly in the middle of the x-axis. These results reveal the not so gradual effect of moving away from $\lambda = 0$ (OLS) to an increasingly sparse model. For example, with a larger penalty coverage reduced for the non-zero relations, whereas, for the null associations, coverage approached 1.0. When using λ_{TC} as the tuning parameter, coverage was around 0.80 for the non-zero coefficients (still far below 0.90). Recently, λ_{TC} was shown to have an inflated false positive rate error rate that did not diminish with more data (Williams, 2020a). Accordingly, a harsher penalty would be required to reduce the false selections, that, in turn, is just where coverage is particularly bad. Again, this is the tension between "pushing" values to zero and parameter estimation with the ℓ_1-penalty.

Summary

These experiments highlighted a fundamental issue with ℓ_1-regularization: in pursuit of sparsity, accuracy of the sampling distribution is necessarily scarified. Although some

degree of inaccuracy is expected (due to biased estimates), the extent to which this compromised coverage of cannot be understated. Further, rather than an issue for *truly* zero and small relations, coverage was also very poor for the large effects. This was especially remarkable because very stringent assumptions were satisfied in both experiments. In what follows, coverage is investigated in a setting representative of the network literature.

Partial Correlation Networks

Extended to multivariate settings, the ℓ_1-penalized likelihood for the precision matrix is defined as

$$\underbrace{\log\det\boldsymbol{\Theta} - \text{tr}(\boldsymbol{S}\boldsymbol{\Theta})}_{\text{log-likelihood}} - \underbrace{\lambda||\boldsymbol{\Theta}||_1}_{\ell_1-\text{penalty}} \tag{7}$$

where $\boldsymbol{S}$ is the sample covariance matrix, $\boldsymbol{\Theta} = \boldsymbol{S}^{-1}$ the precision matrix, and λ is the turning parameter parameter. The graphical lasso (glasso) method applies a penalty on the sum of absolute values for the off-diagonal elements of $\boldsymbol{\Theta}$. Recall that these elements have a direct correspondence to multiple regression (i.e., $\beta_{ij} = -\theta_{ij}/\theta_{ii}$). Indeed, obtaining the glasso estimate of $\boldsymbol{\Theta}$ can be seen as "a p coupled lasso [regression] problems." (p.). This implies much of the same issues that plague ℓ_1 regression also apply to estimating psychological networks, given that the conditional dependence structure is encoded in the off-diagonal elements of $\boldsymbol{\Theta}$.

In network psychometrics, the default choice for selecting λ is again EBIC, that is,

$$\text{EBIC} = -2 \cdot l(\boldsymbol{\Theta}) + k \cdot \log(n) + 4 \cdot \gamma \cdot k \cdot \log(p), \tag{8}$$

where $l(\boldsymbol{\Theta})$ is the (simplified) Gaussian likelihood function that is given by

$$l(\boldsymbol{\Theta}) = \frac{2}{n}\Big[\log \det \boldsymbol{\Theta} - \text{tr}(\boldsymbol{S}\boldsymbol{\Theta})\Big]. \tag{9}$$

Note that $\boldsymbol{\Theta}$ is the glasso estimate. In Equation (8), k is the number of selected edges (off-diagonal elements of $\boldsymbol{\Theta}$), and γ ($0 \leq \gamma \leq 1$) that governs the addition to BIC (the default in network analysis is $\gamma = 0.5$). The selected network then minimizes EBIC with respect to λ. This is typically accomplished by assessing a large number (e.g., 100) of λ's and selecting the one for which EBIC is smallest. The network is then obtained by computing the partial correlations from $\boldsymbol{\Theta}$ (Equation 1). Hence, just as in regression, the sampling distribution of the partials correlations should be increasingly compromised with larger values of γ.

Issues Specific to Network Analysis

Over and above the conflict between model selection and parameter estimation, there are additional issues specific to the psychological network literature. At its crux, recall that the IRC states that the important and unimportant predictors cannot be correlated (at least not too much). There is an analogous assumption that similarly applies in GGMs (see Equation 28 in Ravikumar, Wainwright, Raskutti, & Yu, 2011). Two examples provided in Ravikumar et al. (2011, see Sections 3.1.1 and 3.1.2) suggest that the irrepresentable condition can be more difficult to satisfy for networks than multiple regression. However, in network analysis it is common to estimate the conditional dependence structure of items from scales that, by construction, contain highly correlated variables. It follows that the IRC will likely be violated, perhaps egregiously so. As shown in Hastie et al. (2015, Figure 11.6 therein) and Zhao and Yu (2006, Figure 2 therein), the degree to which it is violated has a direct bearing on the performance of ℓ_1-regularization.

Further, it is also the case that edges are often small in effect size (see Table 2 in Wysocki & Rhemtulla, 2019). This suggests that the beta-min condition may not always be

satisfied. This is perhaps less of concern, because it translates into some edges escaping detection and not false selections. However, as shown in Figure 1 (panel A), small relations in particular can have a severely distorted sampling distribution.

Numerical Experiment 1

In this experiment, I follow a common strategy for simulation in the network literature (e.g., Epskamp, 2016; Williams et al., 2019). The *true* network structure was obtained by first estimating the partial correlation matrix from 20 PTSD symptoms (Armour, Fried, Deserno, Tsai, & Pietrzak, 2017) and then absolute values less than 0.05 were set to zero. Following Ravikumar et al. (2011, Equation 28), the IRC was violated and exceeded the upper bound by a factor of five. This indicates that glasso cannot recover the true model. In psychology, the failure of glasso for these kinds of data was recently highlighted in Williams and Rast (2019), Williams (2020a), Williams et al. (2019). It should be noted that the IRC is unlikely to hold with many variables, unless the ground truth is extremely sparse (see Table 1 in Zhao & Yu, 2006), which is not typically the case in psychological applications (see Table 2 in Wysocki & Rhemtulla, 2019). Accordingly, the setting for this experiment more closely reflects the network literature.

The simulation procedure was as follows. Multivariate normal data were generated for $n = \{250, 500, 1,000, 2,500, 10,000, 25,000, 50,000\}$, given the true network structure obtained from the 20 PTSD symptoms. These large samples allowed for determining whether coverage became worse with more data. A non-parametric bootstrap was employed for glasso with the tuning parameter selected with EBIC ($\gamma = 0$ and 0.5). To obtain the sampling distribution for each partial correlation, a model was selected for each bootstrap sample, $b = 1, ..., 500$, resulting in the estimated relations for a given bootstrap sample. I also bootstrapped a non-regularized model, which amounts to a non-parametric bootstrap for correlations. Importantly, data-driven model selection was again *not* performed for each bootstrap sample. Average coverage for 90% CIs (non-zero relations)

was computed from 500 simulation trials.

These regularized models were fitted with `R` package **GGMncv** (Williams, 2021). Note that **bootnet** is typically used in network analysis for bootstrapping the glasso. However, using **GGMncv** in combination with **boot** (Canty & Ripley, 2020) expedited the simulations. The results do not change appreciably when using **bootnet** (Epskamp, Borsboom, & Fried, 2018).

Results. Because 90% CIs were used for each relation, the proportion of intervals containing the true value for a given network should also be 0.90 (a long run average, of course). This corresponds to coverage averaged across the network. To emphasize this point, Figure 3 (panel A) includes intervals from one simulation trial ($n = 1,000$ and $\gamma = 0.5$), where 0.62 and 0.92 denote the proportion of intervals that covered the *true* value. Although just one random sample, there are red flags for ℓ_1-regularization. For example, several sampling distributions are truncated at zero which is indicative of a point mass at zero (e.g., Figure 1, panel A). Even for edges separated from zero, say, corresponding to at least small in effect size (> 0.1), there is a discernible difference compared to non-regularized estimation that passes the inter-ocular trauma test—it hits between the eyes.

Of course, there is a chance that one simulation trial was not representative of the long run average. Figure 3 (panel B) includes the results for average coverage of the non-zero relations. Here, for $n = 1,000$, the non-regularized method was right at 0.90 and average coverage was round 0.60 for glasso$_{EBIC}$ ($\gamma = 0.5$). That is, just over half of the edges were covered for a given network. Unfortunately, this indicates that the ℓ_1-based "CIs" in panel A were not a fluke.

In general, the default in network psychometrics had very poor coverage ($\gamma = 0.5$). In the smaller sample sizes, the intervals often only covered around half of the values—average coverage was around 0.50 when it should be 0.90. Initially, coverage improved with increasing data, at best reaching nearly 0.70 ($n = 2,500$). This was due to

having more "power" to detect the edges, resulting in there being less (incorrect) zeroes in the bootstrap sampling distribution. The improvement was short lived, however, as coverage actually plummeted with even more data to 0.30 ($n = 50,000$). Together, at no sample size could the default "CIs" in network analysis be considered acceptable.

In the smaller sample sizes, coverage was notably better for glasso$_{EBIC}$ with ($\gamma = 0$). At best coverage was nearly 0.80 when it should be 0.90 ($n = 1,000$) and it also decreased with more data. Although 0.80 is certainly an improvement, it is important to note that this is coverage averaged across all edges in the network— coverage could be much lower for individual edges.

Numerical Experiment 2

As noted in the introduction, there have been some simulations looking at "CIs" computed from bootstrapping the lasso. Perhaps the most extensive is Dezeure et al. (2015), where some troubling results emerged:

> Depending on the ordering of the variables and the correlation structure of the design, coverage can be poor for the non-zero coefficients or a few zero coefficients (p. 1, online supplement).

By the "ordering of the variables" they are referring to the position of the relevant variables in the predictor matrix. Further, recall that the regression example assumed the covariates were not correlated, whereas it appears the correlation structure can result in poor coverage. This could be particularly relevant to psychological networks, because the nodes are highly correlated to one another. In Dezeure et al. (2015), for non-zero relations, coverage was shown to range from 0.98 (Figure 12, online supplement) to 0 (Figure 15, online supplement). In other words, the true value could be covered just about all of the time or never ($\alpha = 0.05$). A reasonable question is why exactly such a method would be entertained? Context is key. The focus was explicitly situations in which non-regularized

methods cannot be used (e.g., $p = 500$ and $n = 100$). In these dimensions, developing methods for parameter uncertainty is an active area of research.

In the context of networks, the results of Dezeure et al. (2015) imply that mere position of an edge can have an appreciable influence on coverage. Teasing this apart in a representative network will take come creativity. If there were several true values of, say, 0.10, within the same network, coverage should be at nominal levels for each relation. Then if coverage was not the same, this would indicate that the position and likely the covariance structure within a given row of $\mathbf{\Theta}$ matters. Of course, in a plausible structure, there are a range of edge weights that should ideally be preserved, as doing so keeps the covariance structure in tact. To this end, given a true matrix (e.g., estimated from PTSD symptoms), rounding the edges to the first decimal place would ensure many have the same effect size, differing only in their position within the network.

The simulation procedure was the same as above, but with the rounding modification after setting absolute values less than 0.05 to zero. This created edges that were equal to $\{-0.3, -0.2, \ldots, 0.5\}$. Also, two sample sizes ($n = 500$ and 2500) and one hyperparameter value for EBIC were included in this experiment ($\gamma = 0.5$). The larger sample size was chosen because this is where average coverage was best for ℓ_1-regularization (Figure 3, panel B). Coverage for 90% CIs was computed from 1,000 simulation trials.

Results. Figure 4 includes these results. The non-regularized method had coverage near the nominal level of 0.90. On the other hand, coverage for glasso$_{EBIC}$ was extremely troubling. For example, with an edge weight equal to 0.10 coverage was anywhere between basically 0 (the interval never covered the true value) to the nominal level of 0.90, with an average of perhaps around 0.50. For large edges, coverage also showed a substantial range, say, for a true edge weight of 0.20, coverage was 0.10 (at worse) and 0.90 (at best). There did seem to be some improvement with large edge weights, although for the largest edges of 0.50 coverage was below 0.60.

These findings need some perspective. For correlations, coverage can be a bit below

nominal levels for smaller sample sizes and large correlations in particular (e.g., Table 2 in Bonett & Wright, 2000). However, coverage for the exact same effect size should not differ all that much, and certainly not ranging from 0 to 0.90. This indicates that the bootstrap distribution obtained from glasso$_{EBIC}$ is heavily influenced by the mere position of an edge in the network—unruly, for the lack of a better word.

Illustrative Example

In this section, I illustrate how to judiciously use the bootstrap in both regularized and non-regularized networks. To this end, a non-parametric bootstrap was employed to estimate Spearman's partial correlations from 10 PTSD symptoms. For glasso$_{EBIC}$, I used the `R` package **GGMncv** and followed the default approach in network psychometrics ($\gamma = 0.5$). The non-regularized model was fitted with the `R` package **GGMnonreg** (Williams, 2019b).

Regularized Network

When starting this project, I planned to visualize the bootstrap distribution obtained from glasso$_{EBIC}$ with a box plot that shows the entire range of estimates (e.g., Figure 6.4 in Hastie et al., 2015). However, upon closer inspection, even intervals based on the minimum and maximum values often missed the true edge. Figure 4 (panel A) thus includes *all* of the estimates, where summaries (e.g., error bars based on quantiles) of the bootstrap distribution have been avoided altogether. Note also the x-axis is titled "Data-Mined Edge Weights" and the term "confidence interval" is not mentioned. On the one hand, researchers may find this confusing due to not lending itself nicely to interpretation. But on the other, given the implications of this paper, there does not seem to be a reasonable interpretation other than a summary of data mining. Further, the plot also includes the proportion of bootstrap samples that each relation was selected (e.g., Figure 6.4 in Hastie et al., 2015).

Non-Regularized Network

In network analysis, researchers typically know exactly what *variables* should be in the model (e.g., items in a psychopathology scale) and what remains is determining their interrelations, including a measure of parameter uncertainty. For the non-regularized model, a relation was therefore included in the network if the 95% CI did not include zero (Figure 4, panel B). These same 95% CIs allow for making statistical inference about the network parameters, over and above detecting non-zero effects.

In Figure 4, notice the shaded region that spans from ±0.1. This is an equivalence region that can be used determine if a meaningful edge size (subjectively defined), sometimes called the smallest effect size of interest (SESOI, Lakens, 2017), can be rejected at the level α. This is especially important for those relations that did not reach statistical significance, as it provides a frequentist approach to assess null associations, although it "does not imply that there is no effect at all" (p. 260, Lakens, Scheel, & Isager, 2018) Rather, if the SESOI is rejected, this implies that two nodes are conditionally independent for all practical purposes. Because none of the CIs were completely within ±0.1, we cannot reject edges weights greater than 0.1 and less than -0.1.[6] In other words, it is not possible to rule out a fully connected network with edges that are at least small in effect size. This is a reminder that network plots provide a mere visualization of those effects that happened to be detected.

Discussion

In this work, I investigated the default measure of uncertainty in psychological networks, that is, "confidence intervals" obtained from bootstrapping ℓ_1-regularized partial correlations. Although it has long been known that this approach does not provide valid CIs in the statistical literature, I set out to determine whether they could at least be

[6] Note that positive effects are often expected in networks (Borsboom, Cramer, Schmittmann, Epskamp, & Waldorp, 2011). In this case, a one-sided hypothesis test can be employed that rules out the possibility of negative relations a priori.

considered approximate. This would have been the ideal, given that they are used in most substantive applications. Based on the results from several simulations, however, it unfortunately appears that there are grave issues with default measure of parameter uncertainty in network analysis.

For the default in network psychometrics, my simulations demonstrated the following:

1. At best, average coverage was around 0.65 for 90% CIs. With increasing sample sizes, average coverage for the non-zero relations *decreased* from 0.65 to 0.30. Because average coverage is the proportion of relations that are covered, this indicates that the vast majority of the true edge weights were not contained within the intervals for a given network.

2. Coverage was heavily influenced by the mere position of an edge in the network. For example, with many true edges weights of 0.1 that differ in only their placement, coverage ranged from essentially 0 (almost never included in the interval) to the nominal level of 0.90, with an average of around 0.50.

3. Meanwhile, for the same simulation conditions, simply obtaining 90% CIs from bootstrapping a non-regularized estimator (i.e., the sample covariance matrix) provided coverage at the nominal level.

I primarily focused on non-zero relations. This is because regularized based "CIs" are typically thought to be most problematic for truly zero or small relations (Knight & Fu, 2000). However, because that ℓ_1-penalty can substantially bias large relations, I reasoned that coverage could be compromised for large effects as well. Indeed, the simulations revealed that coverage is poor in general, with no instances that could be considered remotely acceptable. Together, even for edges that are easily detected, the sampling distribution obtained from bootstrapping glasso$_{EBIC}$ does not provide an adequate measure of parameter uncertainty.

Another key aspect of this work highlighted a fundamental tension between model selection and accurate parameter estimation. In pursuit of sparsity, parameter estimation is scarified to some degree, that is, "sparsity of an estimator leads to undesirable risk properties of that estimator" (p. 209, Leeb & Pötscher, 2008). This was demonstrated for coverage by adjusting the the amount of regularization, which can be understood as investigating the sampling distribution across the continuum between discovery (less regularization) and caution (more regularization). At each step, coverage deteriorated and actually got worse with more data regardless of the degree of penalization. The overall pattern indicated that coverage could ultimately approach zero. Again, there were no redeeming qualities to be found.

Additionally, there is no immediate need to throw the ℓ_1-based bootstrap out with the bathwater. But extreme caution is warranted. As shown in Figure 4 (panel A), the estimates can be presented in a way that discourages misinterpretation (error bars should be avoided altogether) and certainly the term "confidence interval" should be not be used. Rather, the bootstrap distribution is a mere summary of data mining. Moreover, given the choice to compute a valid measure of parameter uncertainty, it seems likely that researchers would often choose to do so. To this end, I have implemented the non-regularized bootstrap in the `R` package **GGMnonreg** (Williams, 2019b).

A Note on Model Selection

There appears to be an emerging trend in quantitative psychology of developing automated search algorithms, many using regularization. However, as shown in this work, there can be issues when attempting to do more than select a model. Shalizi (2013) refers to this as going *within* the selected model:

> Ignore it [model selection]. This can actually make sense if you don't really care about doing inference within your selected model, you just care about what model is selected. Otherwise, I can't recommend it (p. 75).

In my experience, however, researches typically want to make inference within their networks, including a measure of uncertainty, interpreting which edges are the strongest, and obtaining the sampling distribution of centrality indices.

Although model selection is a customary practice in psychology (automated or otherwise), what is less appreciated (or not considered at all) is that "[after model selection] such inference enjoys none of the guarantees that classical statistical theory provides for tests and confidence intervals" (p. 802, Berk, Brown, Buja, Zhang, & Zhao, 2013). A critical assumption of both p-values and CIs is that the model is fixed (pre-data), that is, it was not determined from the data (post-data). When selecting a model, however, this creates the problem of *conditional* (on the selected model) inference (see Section 2.1 in Devezer, Navarro, Vandekerckhove, & Buzbas, 2020). Corrections are then needed to make post-selection inference in both regularized and non-regularized models (e.g., Berk et al., 2013; Lee, Sun, Sun, & Taylor, 2016; Meir & Drton, 2017). This is the reason that I did not compute CIs after selecting a model with non-regularized method.[7] As a result, there was a demonstrably valid measure of uncertainty. For an overview of this topic, I refer interested readers to Taylor and Tibshirani (2015) and Berk, Brown, and Zhao (2010).

Some Thoughts for Network Analysis

In network psychometrics, it is common to do large scale simulations to compare various approaches for model selection, including automated search algorithms and inferential statistics. General recommendations are then put forward based on the results, without considering whether a given method can provide an accurate sampling distribution. One exception can be found in Williams et al. (2019):

> First, although we argued that inferences from regularized methods are not straight forward, the nonregularized [search] methods did not explicitly address

[7] As shown in Drton and Perlman (2004); Williams and Rast (2019); Williams et al. (2019), the edge set can be determined by noting which CIs exclude zero. The key is that the interval is being used to detect the relations.

> these limitations. As such, a limitation of the present work is that we have only provided one method for making inference about individual partial correlations—the bootstrap approach (p. 735).

This is referring to a non-regularized bootstrap, as used in this current work. This is important because researchers typically go *within* the selected model to make inference about the network parameters. The default is bootstrapping ℓ_1-regularized relations. Although this is done with good intentions, given that parameter uncertainty is important to investigate (Fried, van Borkulo, & Epskamp, 2020), what is even better is a demonstrably valid measure of uncertainty (recall another name for a CI is an uncertainty interval).

Herein lies a problem: automated searches and frequentist inference can both be used to select a model, but only the latter provides a valid CI (that was used to detect relations in the first place). Accordingly, if wanting to make inference about the parameters, over and above that they were detected, the non-regularized approach presented in this work provides a straightforward way to do just that. Not withstanding Bayesian inference,[8] in fact, I am not really sure how else inference could be made, other than to have a demonstrably valid CI.

Together, this leads to a straightforward recommendation. When the goal is to only select a model, then either automated searches or frequentist inference can be used—simulation studies comparing the two are useful in this context. For going within the model to make inference on network parameters, then a method that provides an accurate sampling distribution must be used. Of course, there are assumptions for computing CIs and frequentist inference more generally (see here for an overview, Greenland et al., 2016). What is discomforting about bootstrapping ℓ_1-penalized relations is that, even when the standard assumptions are met, coverage is still very poor. In practice, this recommendation amounts to doing nothing, as in searching the data, but instead (1) deciding on the

[8] To make Bayesian inference, see Williams and Mulder (2020), and Williams (2019a)

psychometric scale; and (2) using well-established methods to characterize parameter uncertainty of those relations. As shown above (Figure 4, panel B), this allows for doing more than detecting non-zero effects.

Recent Advances in Regularization

Over the years, a variety of approaches have been developed to overcome limitations of the ℓ_1-penalty. There are too many to summarize here (Williams, 2020b), so I focus on two that are most relevant for this work. One approach to make statistical inference is the de-sparsified (or de-biased) lasso (Javanmard & Montanari, 2014; Van De Geer et al., 2014; Zhang & Zhang, 2014). The basic idea is to *undo* ℓ_1-regularization to some degree which removes sparsity, reduce parameter bias, and can provide confidence intervals either analytically (p. 1217, Janková & van de Geer, 2015) or in conjunction with a bootstrap (Dezeure, Bühlmann, & Zhang, 2017). The de-sparsified estimator was recently extended to GGMs in Janková and van de Geer (2015). Importantly, they considered settings that are not common to network analysis (e.g. $p = 100$ and $n = 200$). In the Appendix (Figure A1), there are results for a small simulation that indicates coverage is *not* at nominal levels in low-dimensional data ($p < n$).[9]

Second, there are alternative penalties that can achieve consistency for both parameter estimation and model selection. Recall, with more data, this requires that the tuning parameter gets very large for *truly* zero relations and zero for non-zero relations (Remark 2 in Zou, 2006). Such penalties are said to have the "oracle properties," that is, they work as though the true model was known in advance. There is a caveat. These are inherently asymptotic properties and their finite sample properties have been called into question for estimation. I refer interested reader to Leeb and Pötscher (2008), where the following was stated "Even worse, estimators possessing the sparsity property (which often entails the oracle property) necessarily have dismal finite sample performance" (p. 202). In

[9] The de-sparsified glasso is implemented in the `R` package **GGMncv** (Williams, 2020b).

other words, a lot can happen on the way towards infinite data. In the Appendix (Figure A1), there are results for a small simulation using the smoothly clipped absolute deviation penalty of Fan and Li (2001, SCAD).[10] The results indicate that coverage is very poor for non-zero relations, unless n is very large and then it reaches nominal levels. This is an improvement over lasso, but that is not saying much at all.[11] Of course, for the same conditions, non-regularized estimation readily provides a valid measure of uncertainty.

Future Directions

In the simulations, all assumptions for the non-regularized model were satisfied. Accordingly, an important future direction is investigating coverage in a range of conditions, say, ordinal data that are heavily skewed. Of course, these simulations should include only non-regularized methods that do not employ data-driven model selection (unless post-selection corrections are made). I encourage researchers to explore the correlation literature, as most of the findings would also apply to partial correlations. To this end, I refer to Bishara and Hittner (2017) for a thorough comparison of confidence intervals, including those constructed with various bootstrapping strategies.

Conclusion

This work delved into the default measure of uncertainty in network psychometrics. As it turns out, there are serious issues with "confidence intervals" computed from bootstrapping ℓ-regularized relations. To ensure network researchers have the option of computing confidence intervals, I implemented a non-regularized bootstrap for various types of partial correlations in the `R` package **GGMnonreg**.

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[10] The SCAD penalty is implemented in the `R` package **GGMncv** (Williams, 2021)

[11] The SCAD penalty and related penalties often outperform lasso for model selection (Williams, 2020b)

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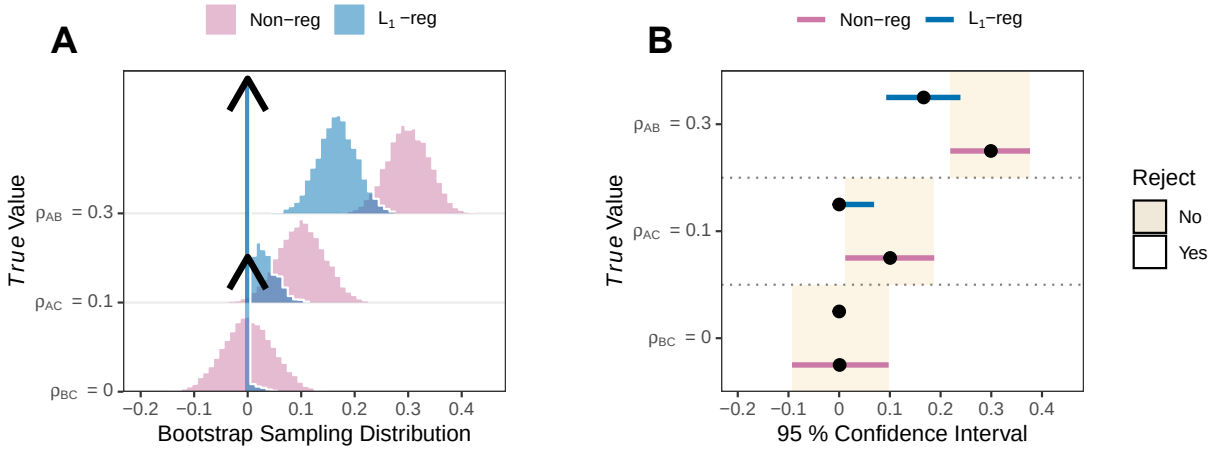

Figure 1. These data were generated such that the sample relations were equal to the true values ($p = 20$ and $n = 500$). All other relations in the network were set to zero. Panel A includes the Bootstrap sampling distributions. Notice that ℓ_1-reg has a point mass at zero for the smallest relations, especially the *truly* null association. For the edges ($\rho \neq 0$), the ℓ_1-based sampling distributions almost completely missed the target values. Panel B includes 95% confidence intervals (CI) obtained from the sampling distributions. ℓ_1-reg exudes the undesirable super-efficiency phenomenon: "the average length of the interval is often very close to zero..." (p. 22, Van De Geer et al., 2014). Further, for both edges the ℓ_1-based 95% "CIs" did not cover the true values.

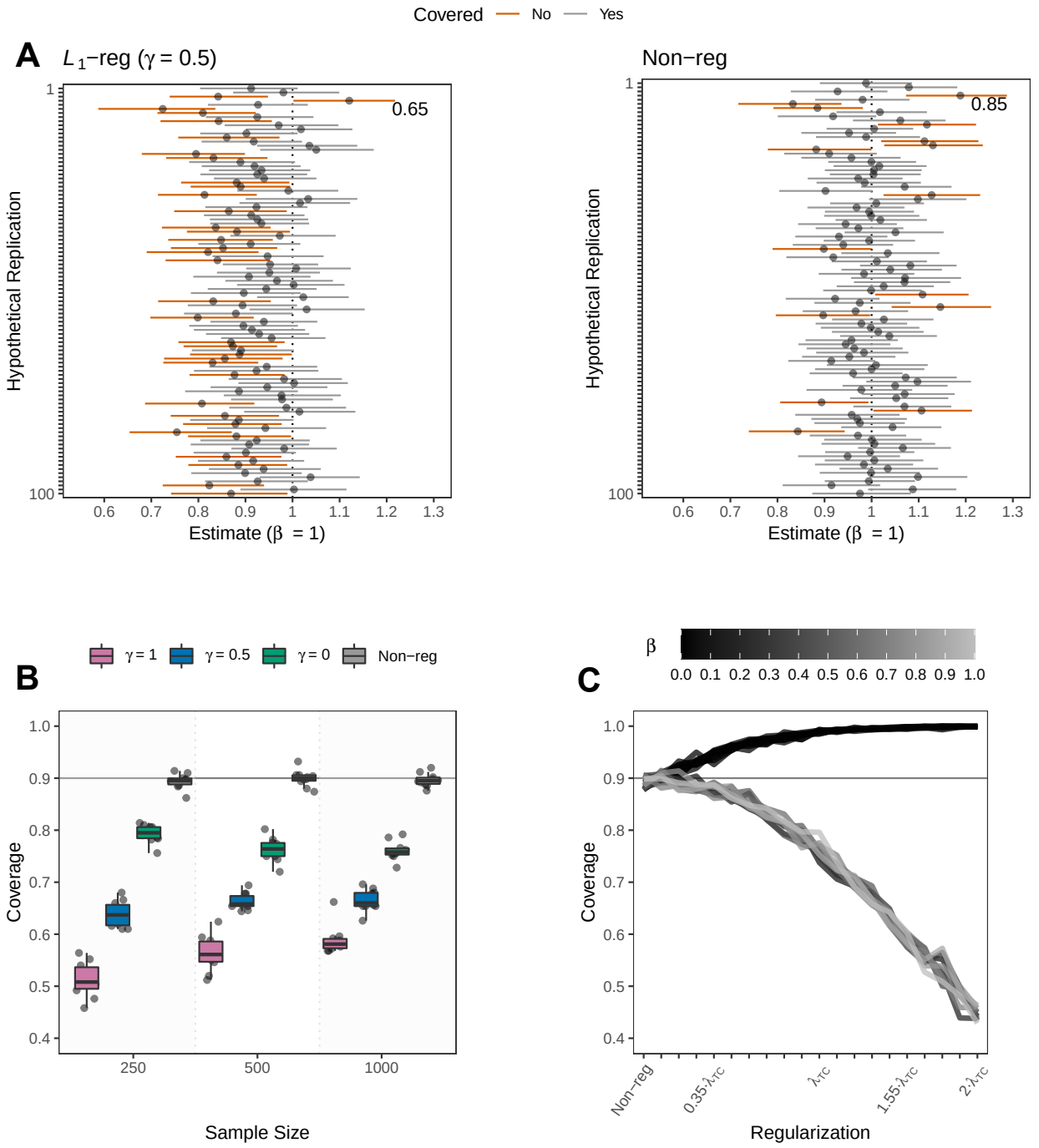

Figure 2. Simulation results (Multiple Regression). Panel A include 90% confidence intervals (CI) computed from 100 simulation trials ($n = 1,000$). Those covering the *true* value are grey, whereas those not covering the *true* value are red. On average, the CIs should cover the true value 90% of the time. Panel B includes coverage for the non-zero regression coefficients. γ corresponds to the hyperparameter in EBIC (Equation 8) that was used to select the tuning parameter in the ℓ_1-regularized regression models (Equation 4). In panel C, the tuning parameter was fixed to $\lambda = c \cdot \lambda_{TC} = c \cdot \sqrt{\log(p)/n}$, where c is a value that adjusts the amount of regularization, ranging from zero to twice λ_{TC} ($p = 20$, and $n = 1,000$).

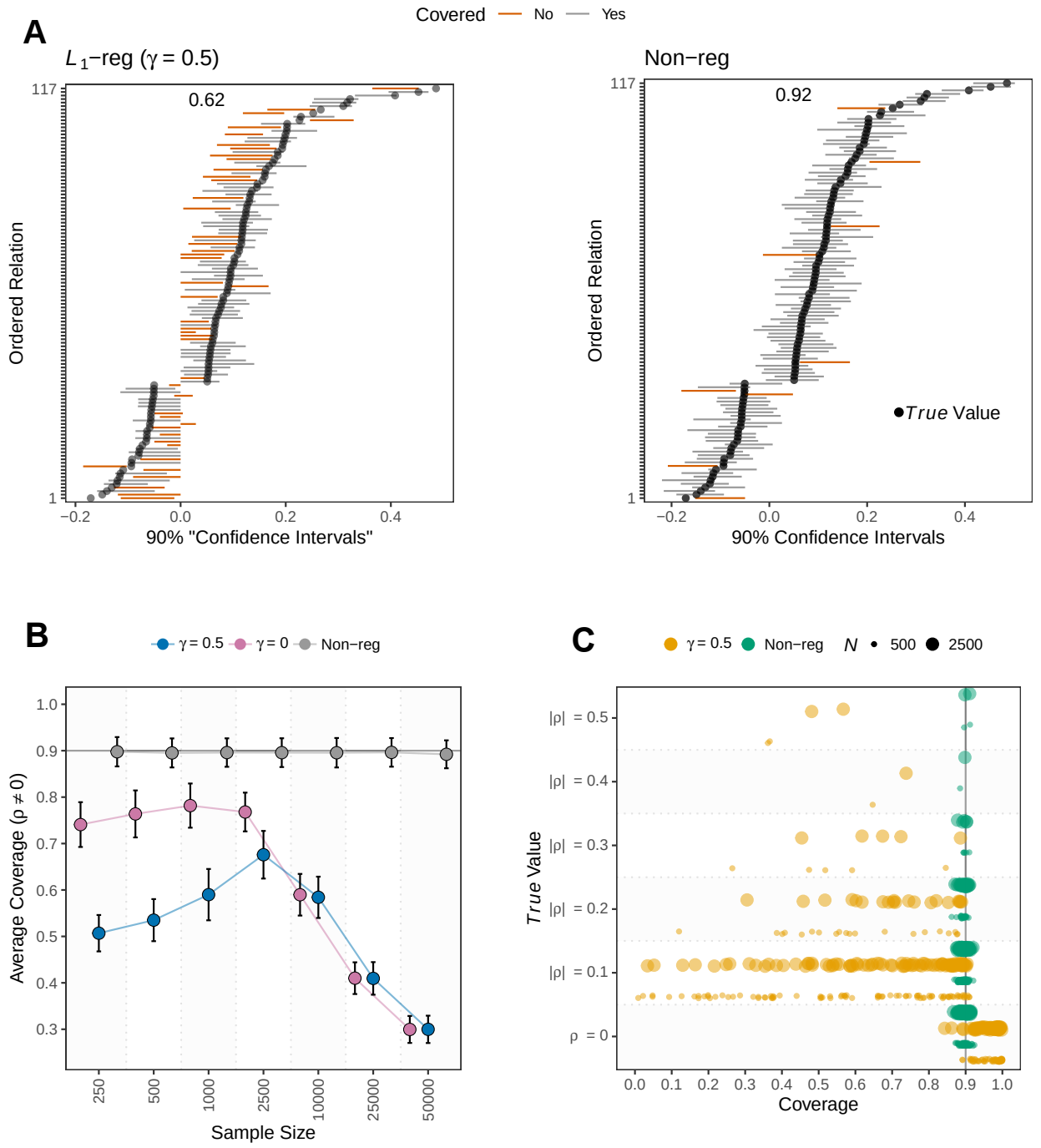

Figure 3. Simulation results (Partial Correlation Networks). Panel A include 90% confidence intervals (CI) for the edges computed from one simulation trial ($n = 1,000$). Those covering the true value are grey, whereas those not covering the true value are red. On average, the CIs should cover the true value 90% of the time. This corresponds to 90% of the relations covered in a given network (on average). Panel B is coverage averaged across all edges. The error bars denote ± 1 standard deviation. Panel C is coverage for the individual partial correlations within the same network. γ is the hyperparameter in EBIC (Equation 8) that was used to select the tuning parameter in the ℓ_1-regularized networks (Equation 4)

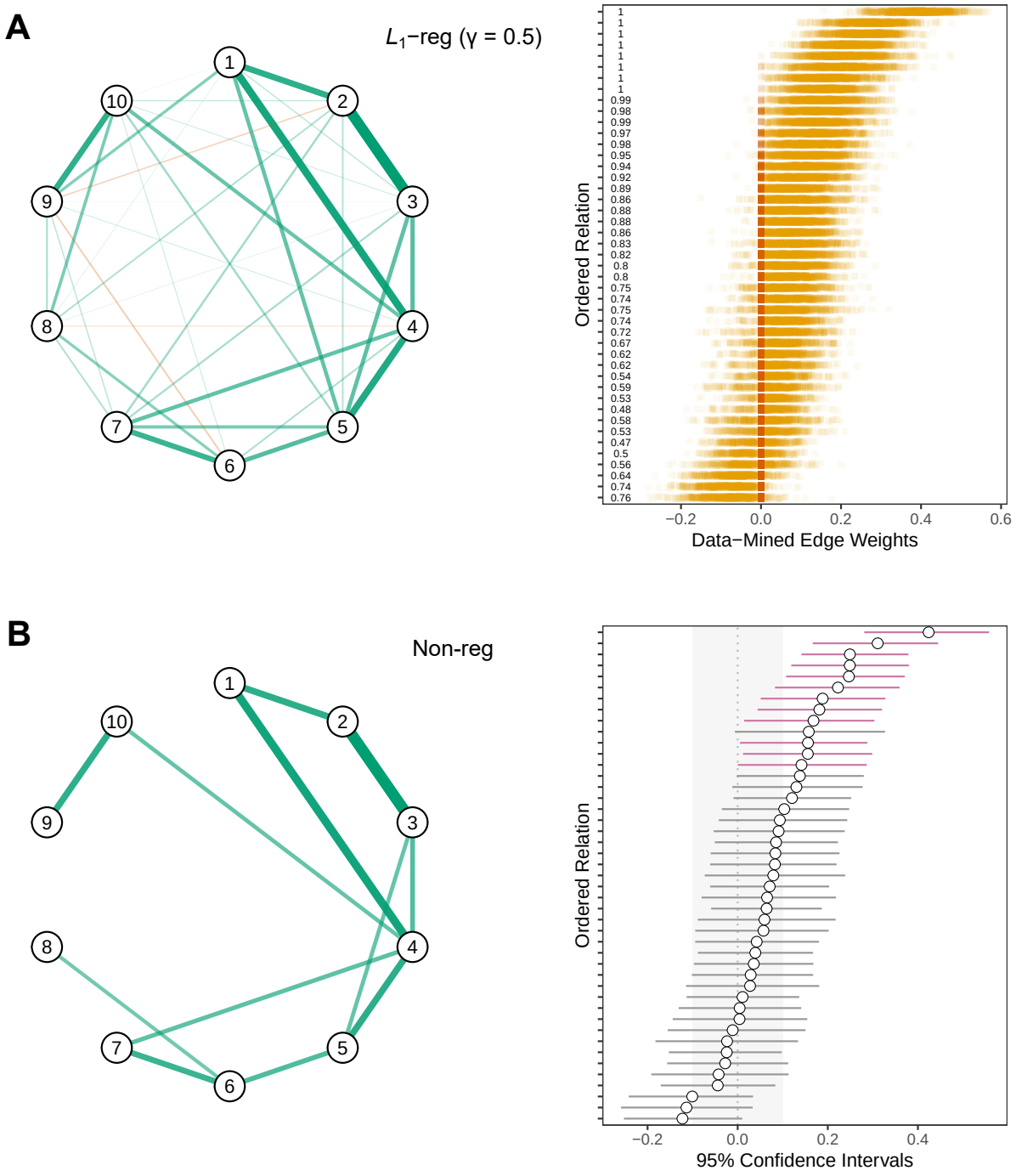

Figure 4. Panel A include the network of ten PTSD symptoms ($n = 221$) and the bootstrap estimates from glasso$_{EBIC}$. For the latter, the numbers in the plot correspond to the proportion of times each relation was selected. Notice here that *all* of the data-mined edge weights are visualized, the boostrap distribution is not summarized, and the term "confidence interval" (CI) is avoided altogether. Panel B include the network of ten PTSD symptoms and 95% CIs from the non-regularized method (computed from the bootstrap sampling distributions). The shaded region spans between ± 0.10.

Appendix

Supplementary Simulation

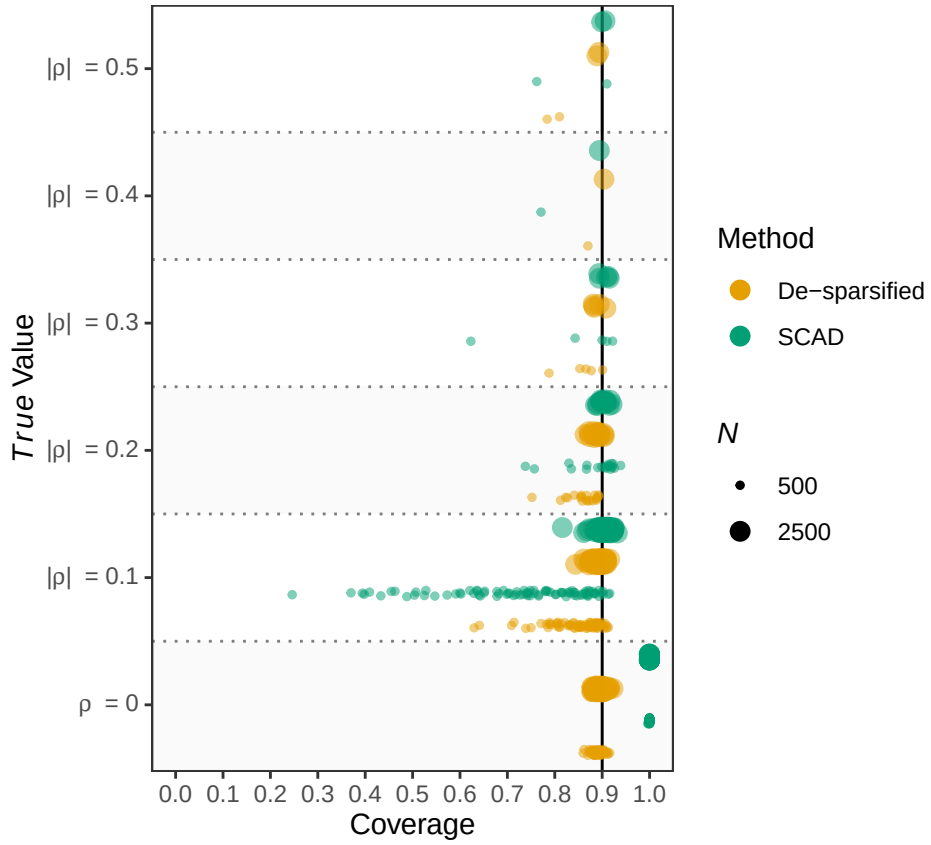

Figure A1. Supplementary simulation results (Partial Correlation Networks). Coverage for individual edges within the same network. In both the de-sparisifed lasso and SCAD, the tuning parameter was selected with EBIC ($\gamma = 0.50$). The "CIs" were computed from the bootstrap sampling distributions. The simulation procedure was identical to that in Figure 3 (panel C).