Testing non-nested structural equation models

Edgar C. Merkle and Dongjun You
University of Missouri

Kristopher J. Preacher
Vanderbilt University

Abstract

In this paper, we apply Vuong’s (1989) likelihood ratio tests of non-nested models to the comparison of non-nested structural equation models. Similar tests have previously been applied in SEM contexts (especially to mixture models), though the non-standard output required to conduct the tests has limited the tests’ previous use and study. We review the theory underlying the tests and show how they can be used to construct interval estimates for differences in non-nested information criteria. Through both simulation and application, we then study the tests’ performance in non-mixture SEMs and describe their general implementation via the R package lavaan. The tests offer researchers a useful tool for non-nested SEM comparison, with barriers to test implementation now removed.

Researchers frequently rely on model comparisons to test competing theories. This is especially true of structural equation models (SEMs), where models are typically complex enough to accommodate a large variety of theories. When competing theories can be translated into nested SEMs, the comparison is relatively easy: one can compute likelihood ratio statistics using the results of the fitted models (e.g., Steiger, Shapiro, & Browne, 1985). The test associated with this likelihood ratio statistic yields one of two conclusions: the two models fit equally well, so that the simpler model is to be preferred, or the more complex model fits better, so that it is to be preferred. As is well known, however, the likelihood ratio statistic does not immediately extend to situations where models are non-nested.

In the non-nested case, researchers typically rely on information criteria for model comparison, including the Akaike Information Criterion (AIC; Akaike, 1974) and the Bayesian Information Criterion (BIC; Schwarz, 1978). One computes an AIC or BIC for the two models, then selects the model with the lowest criterion as “best.” Thus, the applied conclusion differs slightly from the likelihood ratio test (LRT): we conclude from the information criteria that one or the other model is better, while we conclude from the LRT...
either that the complex model is better or that there is insufficient evidence to distinguish model fits.

While information criteria can be applied to non-nested models, the popular “select the model with the lowest” decision criterion can be problematic. In particular, Preacher and Merkle (2012) showed that BIC exhibits large variability at the sample sizes typically used in SEM contexts. Thus, the model that is preferred for a given sample often will not be preferred in new samples. Preacher and Merkle studied a series of nonparametric bootstrap procedures to estimate sampling variability in BIC, but no procedure succeeded in fully characterizing this variability.

A problem with the “select the model with the lowest” decision criterion involves the fact that one can never conclude that the models are equivalent. There may often be situations where the models exhibit “close” values of the information criteria, yet one of the models is still selected as best. To handle this issue, Pornprasertmanit, Wu, and Little (2013) developed a parametric bootstrap method that allows one to conclude that the two models are equally good (in addition to concluding that one or the other model is better). Their results indicated that the procedure is promising, though it is also computationally expensive: one must draw a large number of bootstrap samples from each of the two fitted models, then refit each model to each bootstrap sample.

In this paper, we study formal tests of non-nested models that allow us to conclude that either model is better, that neither model is better, or that the two models are equivalent for the observed data. The tests are based on the theory of Vuong (1989), and one of the tests is commonly applied to the comparison of mixture models with different numbers of components. These mixture models often occur in count regression (comparing zero-inflated models to single-component models; e.g., Smithson & Merkle, 2013) and in latent class models (e.g., the Vuong-Lo-Mendell-Rubin test in Mplus; Lo, Mendell, & Rubin, 2001; Nylund, Muthén, & Asparouhov, 2007). While the machinery required to carry out the resulting tests is non-standard, we have implemented the tests for general models via the R package lavaan (Rosseel, 2012).

Levy and Hancock (2007) (see also Levy & Hancock, 2011) have previously studied the application of Vuong’s (1989) theory to structural equation models, describing relevant background and proposing steps by which researchers can carry out tests of non-nested models. Levy and Hancock bypass an important step of Vuong’s theory due to the non-standard model output required, instead requiring researchers to algebraically examine the candidate models and to potentially carry out difference tests between each candidate model and a constrained version of the models. This procedure accomplishes the desired goal, but it also requires a considerable amount of analytic and computational work on the part of the user. We instead study the tests as Vuong originally proposed them, using the non-standard model output that is required.

In the following pages, we first describe the relevant theoretical results from Vuong (1989). We also show how the theory can be used to obtain confidence intervals for differences in BICs (and other information criteria) associated with non-nested models. Next, we apply the tests to data on teacher burnout, which were originally analyzed by Byrne (1994). Next, we describe the results of two simulations that illustrate test properties in the context of SEM. Finally, we discuss recommendations, extensions, and practical issues.
Theoretical Background

In this section, we provide an overview of the theory underlying the test statistics. The overview is largely based on Vuong (1989), and the reader is referred to that paper for further detail. For alternative overviews of the theory, see Golden (2000) and Levy and Hancock (2007).

We generally consider situations where two candidate models, \(M_A\) and \(M_B\), are fitted via Maximum Likelihood (ML) to a dataset \(X\) with \(n\) cases and \(p\) manifest variables under assumed multivariate normality. That is, each model’s parameters (\(\theta_A\) and \(\theta_B\)) are chosen so that

\[
\hat{\theta}_A = \text{argmax}_{\theta_A} \ell(\theta_A; x_1, \ldots, x_n) \\
\hat{\theta}_B = \text{argmax}_{\theta_B} \ell(\theta_B; x_1, \ldots, x_n),
\]

where, e.g.,

\[
\ell(\theta_A; x_1, \ldots, x_n) = \sum_{i=1}^{n} \ell(\theta_A; x_i) = \sum_{i=1}^{n} \log f_A(x_i; \theta_A)
\]

and \(f_A(x_i; \theta_A)\) is the pdf of the multivariate normal distribution. The log-likelihood \(\ell(\theta_B; x_1, \ldots, x_n)\) is defined in a similar manner.

Note that, instead of defining the ML estimates via (1) and (2), we could instead define them via the gradients

\[
\sum_{i=1}^{n} s(\hat{\theta}_A; x_i) = 0 \\
\sum_{i=1}^{n} s(\hat{\theta}_B; x_i) = 0,
\]

where the above equations sum scores across individuals (the casewise contributions to the gradient). Assuming that \(M_A\) has \(k\) free parameters, the associated score function may be explicitly defined as

\[
s(\theta_A; x_i) = \left( \frac{\partial \ell(\theta_A; x_i)}{\partial \theta_{A,1}}, \ldots, \frac{\partial \ell(\theta_A; x_i)}{\partial \theta_{A,k}} \right)',
\]

with the score function for \(M_B\) defined similarly (where the number of free parameters for \(M_B\) is, say, \(q\) instead of \(k\)).

Overlapping Models

The statistics described here can be used in general model comparison situations, where one is interested in which of two candidate models (\(M_A\) and \(M_B\)) is closest to the data-generating model in Kullback–Leibler distance. Relationships between candidate models may be characterized in at least three ways. Familiarly, nested models are those for which one model is a special case of another; we can arrive at one model by constraining parameters
Figure 1. Path diagram reflecting the models used in the simulation. Model A is the data-generating model, with the loading labeled ‘A’ varying across conditions.

of the other model. Similarly, non-nested models are those for which neither model is a special case of the other. Aside from these two broad classifications, however, we may also define two models to be overlapping for the population of interest. In this situation, for the focal population, the best-fitting model is a special case of both $M_A$ and $M_B$. For example, consider the models characterized by Figure 1, where $M_A$ has a free path from $F_1$ to $X_4$ and $M_B$ has a free path from $F_2$ to $X_3$. If, for the population of interest, the parameters associated with these paths equal zero, then $M_A$ and $M_B$ are overlapping. These two paths would not equal exactly zero in sample datasets, however, so that we must test for overlapping models when they are fit to sample data.

Whereas the overlapping characteristic is easy to see in simple models like that of Figure 1, it can be difficult to observe in more complex SEMs. In these more complex cases, however, the overlapping property is still very important for model comparison: if we cannot reject a hypothesis that models overlap in the population, then there is no point in further model comparison. This is especially true when using information criteria for comparing non-nested models, where one is guaranteed to select one candidate model as better (at least, using the standard decision criterion).

Test Statistics

Vuong’s (1989) tests for overlapping models and for general model comparison utilize the terms $\ell(\hat{\theta}_A; x_i)$ and $\ell(\hat{\theta}_B; x_i)$ for $i = 1, \ldots, n$, which are the casewise likelihoods evaluated at the ML estimates. For the purpose of SEM, we focus on two separate statistics that Vuong proposed. One statistic tests whether or not models are overlapping, and the other tests the equivalence of non-nested, non-overlapping models.

To gain an intuitive feel for these tests, imagine that we fit both $M_A$ and $M_B$ to a data set and then obtain $\ell(\theta_A; x_i)$ and $\ell(\theta_B; x_i)$ for $i = 1, \ldots, n$. To test whether or not
the models overlap, we can look at the variability of casewise likelihoods from $M_A$ to $M_B$: if models do not overlap, then each individual’s likelihood should be nearly the same under both fitted models. This implies that, if we compute a likelihood ratio for each individual $i$, the variability in individual likelihood ratios should be close to zero. If the models do not overlap, then the variability in casewise likelihoods characterizes sampling variability in the likelihood ratio between $M_A$ and $M_B$, allowing for a formal model comparison test that does not require the models to be nested.

To formalize the ideas in the previous paragraph, we characterize the population variance in individual likelihood ratios of $M_A$ vs $M_B$ as

$$\omega^2 = \text{var} \left[ \log \frac{f_A(x_i; \theta_A)}{f_B(x_i; \theta_B)} \right],$$

(7)

where this variance is taken with respect to the true distribution of the $x_i$; that is, there are no assumptions that either candidate model is the true model. Using (7), hypotheses for a test of overlapping models may then be written as

$$H_0: \ \omega^2 = 0 \quad (8)$$

$$H_1: \ \omega^2 > 0, \quad (9)$$

with a sample estimate of $\omega^2$ being

$$\hat{\omega}^2 = \frac{1}{n} \sum_{i=1}^{n} \left[ \log \frac{f_A(x_i; \hat{\theta}_A)}{f_B(x_i; \hat{\theta}_B)} \right]^2 - \frac{1}{n} \sum_{i=1}^{n} \log \frac{f_A(x_i; \hat{\theta}_A)}{f_B(x_i; \hat{\theta}_B)}^2.$$  

(10)

Vuong shows that, under (8) and mild regularity conditions (ensuring that second derivatives of the likelihood function exist, observations are i.i.d., and the ML estimates are unique and not on the boundary), $n\hat{\omega}^2$ is asymptotically distributed as a particular weighted sum of $\chi^2$ distributions. This result immediately allows us to test (8) using results from the two fitted models, though the output that we need to carry out the test is somewhat non-standard. In particular, the asymptotic distribution of $n\hat{\omega}^2$ involves the fitted models’ scores (defined in (6)) and information matrices (see Appendix A for details). The difficulty in obtaining this non-standard output led Levy and Hancock (2007) to bypass the test of overlapping models and instead conduct an algebraic model comparison to determine whether or not the models could be overlapping. Whereas this algebraic comparison is reasonable, it is also more complicated for the applied researcher who wishes to use these tests (assuming that an implementation of the statistical test is available).

Assuming that the null hypothesis from (8) is rejected (i.e., that the models do not overlap), we may compare the models via a non-nested LRT. We may write the hypotheses associated with this test as

$$H_0: \ E[\ell(\hat{\theta}_A; x_i)] = E[\ell(\hat{\theta}_B; x_i)] \quad (11)$$

$$H_1: \ E[\ell(\hat{\theta}_A; x_i)] \neq E[\ell(\hat{\theta}_B; x_i)], \quad (12)$$

where the expectations are taken with respect to the true distribution of the $x_i$. This implies
that we are focusing on the Kullback-Leibler distance between each candidate model and the true model.

For non-nested, non-overlapping models, Vuong shows that

\[ n^{-1/2} \sum_{i=1}^{n} \log \frac{f_A(x_i; \hat{\theta}_A)}{f_B(x_i; \hat{\theta}_B)} \overset{d}{\rightarrow} N(0, \omega_\star^2) \]  

(13)

under (11) and the regularity conditions noted above. Thus, we obtain critical values and p-values by comparing the non-nested LRT statistic to the standard normal distribution. In practice, we would use \( \hat{\omega}_\star^2 \) instead of \( \omega_\star^2 \) in (13), implying that we should compare our test statistic to a \( t \) instead of a \( z \) distribution. For the sample sizes observed in most SEM applications, however, this is inconsequential.

Although the test statistics described above require some non-standard model output (scores of fitted SEMs) and non-standard statistical distributions (weighted sum of \( \chi^2 \) distributions), we have implemented them for general multivariate normal SEMs using the R packages lavaan (Rosseel, 2012) for SEM estimation and score extraction and dr (Weisberg, 2002) for approximation of the weighted sum of \( \chi^2 \) distributions (the latter package implements the approximation proposed by Bentler & Xie, 2000)). In the following sections, we describe ways in which these ideas can be extended to test nested models and to test information criteria.

**Testing Nested Models**

In the situation where \( M_A \) is nested within \( M_B \), the likelihood ratio and the variance statistic \( n\omega_\star^2 \) can each be used to construct a unique test of \( M_A \) versus \( M_B \). For nested models, the null hypotheses (8) and (11) can be shown to be equivalent to the traditional null hypothesis:

\[ H_0: \theta_B \in g(\theta_A) \]  

(14)

\[ H_1: \theta_B \notin g(\theta_A), \]  

(15)

where \( g() \) is a function translating the \( M_A \) parameter vector to an equivalent \( M_B \) parameter vector. In the general case, where \( M_B \) is not assumed to be correctly specified, the statistics from Equations (10) and (13) both strongly converge to a weighted sum of \( \chi^2 \) distributions under (14). This result differs from the usual multivariate normal SEM derivations (e.g., Amemiya & Anderson, 1990; Steiger et al., 1985), which employ either an assumption that \( M_B \) is correctly specified or that the population parameters drift toward a point that is contained in \( M_B \)'s parameter space. Under the assumption that \( M_B \) is correctly specific, the statistics from (10) and (13) both weakly converge to the usual \( \chi^2_{df=q-k} \) distribution under (14). Hence, the framework here provides a more general characterization of the nested LRT than do traditional derivations.

**Testing Information Criteria**

Model selection with AIC or BIC (i.e., selecting the model with the lowest) involves adjustment of the likelihood ratio by a constant term that penalizes the two models for
complexity. Thus, as Vuong (1989) originally described, the above results can be extended to test differences in AIC or BIC. To show this formally, we focus on BIC and write the BIC difference between two models as:

$$\text{BIC}_A - \text{BIC}_B = (k \log n - q \log n) - 2 \sum_{i=1}^{n} \log \frac{f_A(x_i; \hat{\theta}_A)}{f_B(x_i; \hat{\theta}_B)},$$

(16)

where $k$ and $q$ are the number of free parameters for $M_A$ and $M_B$, respectively. This shows that we are simply adding and multiplying constants to the usual likelihood ratio, so that the test of (8) and the result from (13) applies here. In particular, if models are overlapping, then one should generally select the model that BIC penalizes the least (i.e., the model with fewer parameters). If models are not overlapping, then we may formulate a hypothesis that $\text{BIC}_A = \text{BIC}_B$. Under this hypothesis, the result from (13) can be used to show that

$$n^{-1/2} \left[ ((k - q) \log n) - 2 \sum_{i=1}^{n} \log \frac{f_A(x_i; \hat{\theta}_A)}{f_B(x_i; \hat{\theta}_B)} \right] \xrightarrow{d} N(0, 4\omega^2).$$

(17)

A $100 \times (1 - \alpha)\%$ confidence interval associated with the BIC difference is then

$$(\text{BIC}_A - \text{BIC}_B) \pm z_{1-\alpha/2} \sqrt{4n\omega^2},$$

(18)

where $z_{1-\alpha/2}$ is the variate at which the cdf of the standard normal distribution equals $(1 - \alpha/2)$. To our knowledge, this the first analytic confidence interval for a non-nested difference in information criteria that has been presented in the SEM literature. This confidence interval is simpler to calculate than bootstrap intervals (see, e.g., Preacher & Merkle, 2012, for a discussion of bootstrap procedures), and, as shown later, its coverage is comparable.

Relation to the Nesting and Equivalence Test

Bentler and Satorra (2010) described a Nesting and Equivalence Test (NET) that is somewhat similar in aim to the “overlapping” test of (8) described in the previous section. NET is based on the fact that equivalent models can precisely reproduce one another’s implied moments. Thus, if $M_B$ can exactly fit $M_A$’s implied mean vector and covariance matrix (or vice versa), the two models are either nested or equivalent (equivalent if the models differ in degrees of freedom, and nested otherwise). Due to numerical issues from software computations, the phrase “exactly fit” is defined as $M_B$’s discrepancy function, $\hat{F}_B$, being less than some number $\epsilon$, where $\epsilon$ is very close to zero.

The NET procedure is convenient and computationally simple, and it is generally suited to examining whether two models are globally nested or equivalent across large sets of covariance matrices (though see Bentler & Satorra, 2010, for some pathological cases). In contrast, the overlapping tests described here focus on whether or not two candidate models are distinguishable for the data at hand: models may overlap for one dataset but not for a second dataset. Thus, the two methods are complementary: we can use NET to determine whether one’s models can possibly be distinguished from each another, while we can use the test of (8) to determine whether one’s models can be distinguished based on
Figure 2. Path diagram of Byrne’s Models 4, 5, and 7. Only latent variables are displayed, and covariances between exogenous variables are always estimated.

Application: Teacher Burnout

Background

Byrne (1994) tested the impact of organizational and personality variables on three dimensions of burnout for elementary, intermediate, and secondary teachers. The application here is limited to the sample of elementary teachers only (n = 599) and utilizes some of the models in Chapter 6 of Byrne (2009).

Method

We make use of Byrne’s (2009) Models 4, 5, and 7, which are depicted in Figure 2. This figure displays Model 4 (solid lines), with dotted lines reflecting paths that are added and removed in Models 5 and 7. Whereas Model 4 is nested within Model 5, Model 7 is not nested within Model 4 or Model 5. Consequently, we use the non-nested test statistics to individually compare Model 4 to Model 7 and Model 5 to Model 7. Model 7 has the lowest BIC, and Byrne chose Model 7 as the best model (BIC_4 = 40040.7; BIC_5 = 40032.6; BIC_7 = 39975.8).

For Vuong’s “overlapping” test to be informative, it is useful to first establish that the models are not globally equivalent. To reiterate from the NET discussion, “equivalence” means that the models are the same regardless of the observed data, while “overlapping” means that the models are the same only for the particular dataset at hand. Thus, if either pair of models is equivalent, then they will naturally also be overlapping (rendering the overlapping test useless). To examine whether or not the models were equivalent, we applied
the NET procedure (Bentler & Satorra, 2010). We then applied the overlapping test (Equation (8) vs (9)) to non-equivalent models, followed by the non-nested LRT (Equation (11) vs (12)) to non-overlapping models.

**Results**

In applying the NET procedure, we found that neither $M_4$ nor $M_5$ was nested within $M_7$ ($F_{47} = 63.8; F_{57} = 50.6$). Next, we tested whether or not each pair of models was overlapping for these data, using the test of (8). We found that neither $M_4$ nor $M_5$ was overlapping with $M_7$ ($\hat{\omega}^2_{47} = 0.13, p < 0.01; \hat{\omega}^2_{57} = 0.11, p < 0.01$). Thus, we proceeded with the non-nested LRT of (11).

In comparing each pair of models with the non-nested LRT, we found that $M_7$ fit better than each of the other models at $\alpha = 0.025$ ($z_{47} = 2.55, p = 0.005; z_{57} = 1.97, p = 0.024$), with this $\alpha$ being taken due to the fact that we carried out two LRTs. We also examined confidence intervals associated with BIC differences. For $M_4$ vs $M_7$, the BIC difference was 64.9 with a 90% confidence interval of (35.4, 94.4). For $M_5$ vs $M_7$, the BIC difference was 56.9 with a 90% confidence interval of (30.8, 83.0). These results all lead us to prefer $M_7$, in a more convincing fashion than simple comparison of BIC statistics would permit.

In the next sections, we further study the tests’ abilities via simulation.

**Simulation 1: Overlapping Models**

In Simulation 1, our two candidate models potentially overlap with one another for the observed data. We study the overlapping test’s (of (8)) ability to pick up the overlapping models, and we also study the LR test’s (of (11)) ability to compare non-overlapping models. Finally, we compare the results obtained with these two novel tests to the use of BIC for model comparison.

**Method**

The two candidate models are displayed in Figure 1; both are two-factor models, and they differ in which loadings are estimated. The data-generating model, $M_A$, has an extra loading from the first factor to the fourth indicator (labeled ‘A’ in the figure). The second model, $M_B$, instead has an extra loading from the second factor to the third indicator (labeled ‘B’ in the figure).

To study the tests described in this paper, we set the data-generating model’s parameter values equal to the parameter estimates obtained from a two-factor model fit to the Holzinger and Swineford (1939) data (using the scales that load on the “textual” and “speed” factors). Additionally, we manipulated the magnitude of the ‘A’ loading during data generation. This loading could take values of $d = 0, 0.1, \ldots, 0.5$. In the condition where $d = 0$, the candidate models overlap. In other conditions, $M_A$ is preferable to $M_B$.

Simulation conditions were defined by $d = 0, 0.1, \ldots, 0.5$ and by $n = 200, 500, 1,000$. In each condition, we generated 5,000 datasets and fit both $M_A$ and $M_B$ to the data. We then computed five statistics: the NET (Bentler & Satorra, 2010), the overlapping test of (8), the LRT of (11), and each model’s BIC. Each statistic was used to determine whether or not $M_A$ should be favored. To be specific, we counted each statistic as favoring $M_A$ if: (1) the
NET implied that models were not equivalent, (2) the overlapping test implied that models were not overlapping, (3) the LRT implied that model fits were not equal, and (4) the $M_A$ BIC was lower than the $M_B$ BIC. Of course, lack of equivalence and lack of overlappingness do not necessarily imply that $M_A$ should be preferred to $M_B$. However, the definitions above allow us to put the tests on a common scale for comparison of results.

**Results**

Simulation results are displayed in Figure 3. The x-axes display values of $d$, the y-axes display the probability that $M_A$ was preferred to $M_B$, and panels display results for different values of $n$. The lines within each panel represent the four statistics that were computed. We see that the NET procedure almost never declares the two candidate models to be equivalent, even in the condition where $d = 0$. This is because NET is generally a test for global equivalence, as opposed to the overlapping characteristic described in this paper. BIC, on the other hand, increasingly prefers the true model, $M_A$, as $d$ and $n$ increase. In fact, it prefers $M_A$ more often than either the overlapping test or the LRT. The problem, as mentioned earlier, involves the fact that there is no mechanism for declaring models to be equivalent via BIC. For example, in the $(d = 0.1, n = 200)$ condition, BIC prefers $M_A$ about 70% of the time. The other 30% of the time, we would prefer $M_B$. In contrast, the overlapping test and LRT provide a formal mechanism for preferring neither model.

Focusing on the overlapping test results in Figure 3, we observe “true” Type I errors in the $d = 0$ condition: models are declared to be non-overlapping approximately 5% of the time. Additionally, the test increasingly rejects the hypothesis that models are overlapping with both $d$ and $n$. Finally, focusing on the LRT results, we see near-zero Type I errors in the $d = 0$ conditions. This reflects the fact that the LRT should be used only when models are not overlapping (i.e., when the overlapping test is rejected). Conditional on a rejected overlapping test, the LRT Type I errors are closer to 0.05. More generally, the power of the LRT approaches 1 at a slower rate than the power of the overlapping test. The gap between the overlapping and LRT curves roughly reflects the proportion of time that the hypothesis of overlapping models is rejected but the hypothesis of equal-fitting models is not rejected.

Because the two candidate models in this simulation both had the same number of parameters, the LRT is equivalent to the BIC test that was described in Equation (17). In the following simulation, we study the BIC intervals using different models.

**Simulation 2: BIC Intervals**

As mentioned previously, Vuong’s theory supplies analytic confidence intervals for BIC differences between non-nested models. In this simulation, we study the properties of these interval estimates and compare them to intervals obtained from a nonparametric bootstrap.

**Method**

The simulation was set up in a manner similar to the simulation from Preacher and Merkle (2012), using the models pictured in Figure 4. One thousand datasets were first generated from Model D, then Models A–C were fit to each dataset and interval estimates of BIC differences obtained. We examined sample sizes of $n = 200, 500$, and 1,000 and
compared 90% interval estimates from Vuong’s theory to 90% interval estimates from the nonparametric bootstrap. Statistics of interest included interval coverage, mean interval width, and interval variability. The latter statistic is defined as the pooled standard deviation of the lower and upper confidence limits; for a given sample size and interval type, the statistic is computed via:

\[ s_{\text{int}} = \sqrt{\frac{999 \times (s_L^2 + s_U^2)}{1998}}, \]  

where \( s_L^2 \) is the variance of the lower limit, \( s_U^2 \) is the variance of the upper limit, and it is assumed that we are computing this variance across 1,000 replications.

For the code used for this simulation (and other analyses in this paper), see the “Computational Details” section.

Results

Results are displayed in Table 1, with model pairs and sample sizes in rows and interval statistics in columns. The two columns on the right show that coverage is generally good for both methods; the coverages are all close to .9. The other columns show that the Vuong intervals tend to be slightly better than the bootstrap intervals: the Vuong widths are slightly smaller, and there is slightly less variability in the endpoints. These small advantages may not be meaningful in many situations, but the results at least show that the bootstrap intervals and Vuong intervals are comparable. Additionally, the Vuong intervals have a clear computational advantage, requiring only output from the two fitted models (and no extra data sampling or model fitting).

Given the reasonable performance of the Vuong tests in both simulations, we now proceed to discuss some further issues surrounding the tests’ use in practice.
Figure 4. Path diagrams reflecting the models used in Simulation 2.

Table 1
Average interval widths, variability in endpoints, and coverage of differences in non-nested BICs, Simulation 2.

| Models | n   | Avg Width | Endpoint SD | Coverage |
|--------|-----|-----------|-------------|----------|
|        |     | Vuong     | Boot        | Vuong    | Boot    |
| A-B    | 200 | 39.350    | 42.241      | 12.137   | 12.521  | 0.919 | 0.873 |
|        | 500 | 57.937    | 59.620      | 18.173   | 18.516  | 0.901 | 0.875 |
|        | 1000| 79.614    | 80.682      | 23.484   | 23.798  | 0.916 | 0.907 |
| B-C    | 200 | 45.469    | 47.805      | 14.508   | 14.885  | 0.899 | 0.912 |
|        | 500 | 69.064    | 70.317      | 20.832   | 21.036  | 0.901 | 0.905 |
|        | 1000| 95.699    | 96.403      | 29.665   | 29.953  | 0.893 | 0.891 |
| C-A    | 200 | 44.843    | 48.082      | 13.704   | 14.107  | 0.919 | 0.894 |
|        | 500 | 65.386    | 67.320      | 19.633   | 19.853  | 0.910 | 0.891 |
|        | 1000| 89.381    | 90.753      | 27.655   | 27.977  | 0.899 | 0.882 |
General Discussion

The framework described in this paper provides researchers with a general means to test pairs of SEMs for differences in fit. As a byproduct, researchers also gain a means to test whether pairs of SEMs overlap when they are applied to a particular dataset and to test for differences between models’ information criteria. In the discussion below, we provide detail on extension of the tests to comparing multiple (> 2) models. Additionally, we describe a recommended strategy for applied researchers.

Comparing Multiple Models.

The reader is likely to wonder whether or not the above theory extends to simultaneously testing multiple models. Katayama (2008) derives the joint distribution of LR statistics comparing \((m - 1)\) models to a baseline model (i.e., when we have a unique LR statistic comparing each of \((m - 1)\) models to the baseline model) and obtains a test statistic based on the sum of the \((m - 1)\) squared LR statistics. We do not describe all the details here but instead supply some informal intuition underlying the tests.

In a situation where one wishes to compare multiple models, we can obtain the case-wise likelihoods \(\ell_j(\theta; x_i), i = 1, \ldots, n, j = 1, \ldots, m\). We could then subject these casewise likelihoods to an ANOVA, where case \((i)\) is a between-subjects factor and model \((j)\) is a within-subjects factor. We should expect a main effect of case, because the models will naturally fit some individuals better than others. The main effect of model, however, serves as a test of whether or not all \(m\) model fits are equal. Additionally, the error variance informs us of the extent to which models overlap: if the error variance is close to zero, then we have evidence for overlapping models. The ANOVA framework could also be useful for posthoc tests, whereby one wishes to specifically know which model fits differ from which others.

We have not implemented the tests derived by Katayama, and the ANOVA described above is not equivalent to Katayama’s tests. For example, the ANOVA assumes sphericity for the within-subjects factor, whereas Katayama explicitly estimates covariances between model likelihoods. Simultaneous tests of multiple SEMs generally provide interesting directions for future research.

Recommended Use. Using NET and the methods described here (and also in Levy & Hancock, 2007, 2011), one gains a clearer picture of the extent to which one model fits better than another. These procedures give researchers the ability to routinely test non-nested models for global equivalence, local equivalence (i.e., overlappingness), and differences in fit or information criteria. We recommend the following non-nested model comparison sequence.

1. Using the NET, evaluate models for global equivalence or nesting (can be done prior to data collection). If models are not found to be equivalent or nested, proceed to 2.
2. Test whether or not models are overlapping, using the \(\omega^2\) statistic (data must have already been collected). If models are not found to be overlapping, proceed to 3.
3. Compare models via the non-nested LRT or via tests and intervals of BIC differences.
If one makes it to the third step, then the test or interval estimate may allow for the preference of one model. Otherwise, one cannot conclude that either model is better than the other. The sequence above provides more information about the models’ relative standings than do traditional comparisons via BIC, which should help researchers to favor a model only when the data truly favor that model. While our current implementation allows researchers to carry out the above steps using SEMs estimated under multivariate normality, work by Golden (2003) implies that the methods can also be applied to models estimated via alternative discrepancy functions (such as weighted least squares). Further work is needed to obtain scores associated with such discrepancy functions.

Finally, we note that the ideas described throughout this paper generally apply to situations where one wishes to declare a single model as the best. One may instead wish to average over the set of candidate models, drawing general conclusions across the set (e.g., Hoeting, Madigan, Raftery, & Volinsky, 1999). Though it is computationally more difficult, the model averaging strategy allows the researcher to explicitly acknowledge that all of the models in the set are ultimately incorrect. The topic of model averaging generally appears to be under-developed in SEM contexts.

### Computational Details

All results were obtained using the R system for statistical computing (R Development Core Team, 2013), version 3.0.2, employing the add-on packages *lavaan* 0.5-15 (Rosseel, 2012) for fitting of the models and score computation, *dr* 3.0.7 (Weisberg, 2002) for approximation of weighted sums of $\chi^2$ distributions, and *simsem* 0.5-3 (Pornprasertmanit, Miller, & Schoemann, 2013) for simulation convenience functions. R and the packages *lavaan*, *dr*, and *simsem* are freely available under the General Public License 2 from the Comprehensive R Archive Network at [http://CRAN.R-project.org/](http://CRAN.R-project.org/). R code for replication of our results and for applying the tests to general *lavaan* models (fitted under a multivariate normal likelihood) is available at [http://semtools.R-Forge.R-project.org/](http://semtools.R-Forge.R-project.org/).

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Appendix

Details of Variance Test

In this appendix, we describe technical details for testing the variance statistic $\omega^2_*$. As stated in the main text, under the hypothesis that $\omega^2_* = 0$, $n\hat{\omega}^2_*$ converges in distribution to a weighted sum of $(k + q)$ chi-square distributions (with 1 degree of freedom each), where $k$ and $q$ are the number of free parameters in $M_A$ and $M_B$, respectively. The weights are defined as the squared eigenvalues of a matrix $W$, which is defined below.
Let the matrices $U_A(\theta_A)$ and $V_A(\theta_A)$ be defined as

\[ U_A(\theta_A) = E \left[ \frac{\partial^2 \ell(\theta_A; x_i)}{\partial \theta_A \partial \theta_A'} \right], \]  
(20)

\[ V_A(\theta_A) = E \left[ \frac{\partial \ell(\theta_A; x_i)}{\partial \theta_A} \cdot \frac{\partial \ell(\theta_A; x_i)}{\partial \theta_A'} \right], \]  
(21)

which can be estimated from a fitted $M_A$’s information matrix and cross-product of scores (see Equation (6)), respectively. The matrices $U_B(\theta_B)$ and $V_B(\theta_B)$ are defined similarly. Further, define $V_{AB}(\theta_A, \theta_B)$ as

\[ V_{AB}(\theta_A, \theta_B) = E \left[ \frac{\partial \ell(\theta_A; x_i)}{\partial \theta_A} \cdot \frac{\partial \ell(\theta_B; x_i)}{\partial \theta_B'} \right], \]  
(22)

which can be obtained by taking products of $s(\hat{\theta}_A; x_i)$ and $s(\hat{\theta}_B; x_i)$. Then the matrix $W$, whose squared eigenvalues form the weights of the limiting distribution of $\omega^2_*$, is defined as

\[ W = \begin{bmatrix}
-V_A(\theta_A)U_A^{-1}(\theta_A) & -V_{AB}(\theta_A, \theta_B)U_B^{-1}(\theta_B) \\
V_{AB}(\theta_A, \theta_B)U_A^{-1}(\theta_A) & V_B(\theta_B)U_B^{-1}(\theta_B)
\end{bmatrix}. \]  
(23)

See Vuong (1989) for the proof and further detail.