Commentary

Issues and Opinion on Structural Equation Modeling

By Wynne W. Chin

In the past few years, the IS field has seen a substantial increase in the number of submissions and publications using structural equation modeling (SEM) techniques. Part of the reason may be the increase in software packages to perform such covariance-based (e.g., LISREL, EQS, AMOS, SEPATH, RAMONA, MX, and CALIS) and component-based (e.g., PLS-PC, PLS-Graph) analysis. Viewed as a coupling of two traditions—an econometric perspective focusing on prediction and a psychometric emphasis that models concepts as latent (unobserved) variables that are indirectly inferred from multiple observed measures (alternately termed as indicators or manifest variables)—SEM has allowed social scientists to perform path analytic modeling with latent variables (LVs), which in turn has led some to describe this approach as an example of "a second generation of multivariate analysis" (Fornell 1987, p. 408).

When applied correctly, SEM-based procedures have substantial advantages over first-generation techniques such as principal components analysis, factor analysis, discriminant analysis, or multiple regression because of the greater flexibility that a researcher has for the interplay between theory and data. Specifically, SEM provides the researcher with the flexibility to: (a) model relationships among multiple predictor and criterion variables, (b) construct unobservable LVs, (c) model errors in measurements for observed variables, and (d) statistically test a priori substantive/theoretical and measurement assumptions against empirical data (i.e., confirmatory analysis). Thus, SEM involves generalizations and extensions of first-generation procedures. The application of certain constraints or assumptions on an SEM analysis would then yield a first-generation analysis with correspondingly less flexibility in modeling theory with data. For example, Knapp (1978) has shown how various constraints applied on a canonical correlation analysis would be equivalent to performing a multiple regression, multiple discriminant analysis, analysis of variance or covariance, or principle components analysis. In turn, canonical correlation has been shown to be a special case of partial least squares (Wold 1975) and covariance-based SEM (Bagozzi et al. 1981).

Yet, along with the benefits that SEM provides comes a higher level of complexity requiring greater knowledge about the conditions and assumptions for appropriate usage. Without due consideration, the results and conclusions based on its application can be seriously flawed or invalid. Whereas most of the requirements for SEM use can be found in textbooks (e.g., Bollen 1989; Hayduk 1987; Hoyle 1995) and are commonly known among mathematicians, statisticians, and other SEM experts, a significant number of recent article submissions as well as published articles in various journals suggest that they are still not as well understood among IS researchers, often leading to serious flaws in their application.

Although inappropriate application of SEM is not unique to the IS discipline (e.g., Breckler 1990; Cohen et al. 1990), it is important to highlight some of the major issues that IS researchers need to consider when using it. This write-up should not be construed as a recommendation for using SEM techniques or a detailed description on how to go about performing such analyses. Rather, the intent of this discussion is to point out briefly what IS researchers need to consider and what to do or not do when applying SEM analysis as part of their study. Appropriate references are provided for each issue addressed. While the list of issues provided are not necessarily comprehensive, they should go a long way toward improving articles submitted to *MIS Quarterly* and other IS journals. Unless otherwise noted, the issues and discussion presented pertain primarily to the covariance-based procedures as represented by software such as LISREL.

Clear Reporting

As in the case of all statistical analyses, clear communication of the SEM study can assist both during the review process and in building a cumulative tradition for the IS field. Enough information needs to be provided to understand (a) the population from which the data sample was obtained, (b) the distribution of the data to determine the adequacy of the statistical estimation procedure, (c) the conceptual model to determine the appropriateness of the statistical models analyzed, and (d) statistical results to corroborate the subsequent interpretation and conclusions. In addition, the computer program and version number, starting values for the analysis if different from the program's default, number of iterations and other computational options, and difficulties and anomalies that were encountered need to be reported. According to Steiger (1988), reliable replication would not be possible without a clear discussion in each of these areas.

In terms of replicating the statistical analysis itself, it is critical that the covariance matrix of items used in all the models be provided. Item means must also be provided in the case of a latent means analysis. An advantage of the SEM approach is that any researcher can replicate the analysis given the matrices and information on the specific models that were analyzed in the study. In other words, all the parameter estimates (e.g., structural paths and item loadings), overall model fit indices, R-square, and other analytical results can be reproduced given the covariance matrix and details of the statistical model. This can be extremely useful for initial screening by AEs or reviewers during the review process. It also provides reviewers the ability to detect and possibly offer solutions to problems in the analysis. Once published, this information provides for a cumulative tradition where researchers have the opportunity to learn through replication of the analysis or to perform further studies or reanalyses with alternative models (possibly combining additional data). Therefore, the covariance matrix or, as Hoyle and Panter (1995) recommend, the correlation and standard deviations of the items to the third significant digit (which can then be used to derive the covariance matrix) should be provided.

With the covariance matrix in hand, one still needs exact information of how the statistical models were specified as input to running the analysis. This entails providing information to the reader regarding (a) the measurement model, which links LVs to items and (b) the structural model connecting LVs. The researcher should avoid the use of statistical jargon or mathematical/Greek symbols in describing these relationships. Furthermore, there is no need for a rehash of the mathematical models underlying the SEM approach. While this was common for the initial set of articles that applied SEM analysis, a simple citation at most should suffice. To do otherwise will lead only to confusion for the average reader.

Another practice that should be avoided is explicitly providing hypothesis statements for each structural path in the model. Whereas each proposed relation or path in a model (including zero or absent paths) should be theoretically justified and explained in the text of the article, the act of stating a null and/or alternative hypothesis for each path is not only redundant and wasteful of journal space, but can be confusing to the reader.

In general, all statistical models tested can be easily described through graphical representation and simple language. What needs to be done is to clearly present the model paths and indicate which parameters are being estimated and which are fixed or constrained. Certain paths (structural or loadings) and variances (for items, LVs, and unique/error terms) are allowed to be estimated, whereas others are fixed to a specific number (typically at 1) or constrained to equal another parameter. All such specifications need to be made explicit.

Finally, a complete description of the results of an SEM analysis requires a full discussion of the parameter estimates, fit statistics, and logic behind any modifications or set of models examined. Furthermore, it would be extremely helpful if, during the review process, all questionnaire items in both form and content are provided to reviewers. This allows better understanding and interpretation of the model and results. New or reworded items from existing instruments should always be included in an appendix. Researchers interested in further information or planning to write up an SEM study are encouraged to read both Steiger's (1988) and Hoyle and Panter's (1995) articles.

Formative vs. Reflective Indicators

An underlying assumption for SEM analysis is that the items or indicators used to measure an LV are *reflective* in nature. Such items are viewed as affected by the same underlying concept (i.e., the LV). Yet a common and serious mistake often committed by researchers is to inadvertently apply *formative* indicators (also known as cause measures) in an SEM analysis. Formative indicators, first introduced by Blalock (1964), are measures that form or cause the creation or change in an LV. An example is socio-economic status (SES), where indicators such as education, income, and occupational prestige are items that cause or form the LV SES. If an individual loses his or her job, the SES would be negatively affected. But to say that a negative change has occurred in an individual's SES does not imply that there was a job loss. Furthermore, a change in an indicator (say income) does not necessarily imply a similar directional change for the other indicators (say education or occupational prestige).

Another example of formative measures would be the amount of beer, wine, and hard liquor consumed as indicators of mental inebriation.[1] Potential reflective measures might be blood alcohol level, driving ability, MRI brain scan, and performance on mental calculations. If truly reflective, an improvement in the blood alcohol level measure for an individual would also imply an improvement in the MRI activity and other measures since they are all meant to tap into the same concept or phenomenon. Conversely, for the formative measures, an increase in beer consumption does not imply similar increases in wine or hard liquor consumption. Thus, while it may occur, formative indicators need not be correlated nor have high internal consistency such as Cronbach's alpha (Bollen 1984; Bollen and Lennox 1991).

Because SEM techniques such as LISREL attempt to account for all the covariances among its measures, the inclusion of formative measures becomes problematic. All items must be reflective to be consistent with the statistical algorithm that assumes that the correlations among indicators for a particular LV are caused by that LV. For this to be true, one needs to look at all the items and determine whether they are tapping into the same underlying issue or factor. Alternatively, one can do the following thought exercise: Is it necessarily true that if one of the items (assuming all coded in the same direction) were to suddenly change in a particular direction, the others will change in a similar manner? If the answer is no and the items are in fact formative, the resulting estimates would be invalid.

Cohen et al. (1990) have shown that this is a common mistake in psychological and sociological journals leading to serious questions concerning the validity of the results and conclusions. Reasonable goodness of fits can still result, often fooling the naïve user. Attempts to explicitly model formative indicators in an SEM analysis have been shown to lead to identification problems, with efforts to work

[1] I thank Cameron Mitchell for his suggestion for this label and discussion of the measures.

around them generally unsuccessful (MacCallum and Browne 1993). One way around this problem is to apply the components-based approach known as partial least square (PLS), which can model formative indicators (Chin forthcoming; Barclay et al. 1995; Fornell and Bookstein 1982). For further information on this issue, please refer to Cohen et al. (1990) and Chin and Gopal (1995).

Second Order Factor Models

A number of recent IS papers have presented second order factor models. Such models consist of a higher order LV that is modeled as causally impacting a number of first order LVs (i.e., standard LVs with measured indicators). Therefore, these second order LVs or factors are not directly connected to any measured items. One might view such analysis as akin to a confirmatory factor analysis, but at a higher level of abstraction where the indicators are actually latent variables.

When considering such models, several questions need to be considered. The first is the purpose of these models. Often, the initial answer is that a good model fit demonstrates that a general, more global factor exists that explains all the covariation among the first order factors. Upon deeper reflection, the question still remains as to what the purpose is for explaining these covariations. Is this second order factor expected to mediate fully the relationship of the first order factors when applied in a theoretical model? To postulate the existence of a second order factor that sits in a vacuum holds little value. Rather, it must be related to other factors in a conceptual model. Because a second order factor is modeled as being at a higher level of abstraction and reflected by first order factors, it needs to be related with other factors that are at a similar level of abstraction independent of whether these other factors are inferred from measured items or other first order factors. Therefore, it is imperative that this be demonstrated by embedding such second order factor models within a nomological network (i.e., used as a consequent and/or predictor of other LVs).[2]

Tests of validity for a second order factor model should, by analogy, follow the same process that is used to examine the validity of first order factors. The first step is to reapply the formative/reflective question at this higher order level. In essence, one asks whether the first order factors actually tap into the same underlying second order LV or are factors that form the LV. Analogous to the situation with formative indicators, second order factors modeled as being caused by first order LVs cannot be analyzed by SEM techniques such as LISREL. Instead, all first order LVs must be modeled as reflecting the second order LV. For further discussion on the various models linking first and second order factors, read Chin and Gopal (1995). The next step is to determine if the implicit constraints are realistic. For a second order factor model, an implicit equality constraint is placed among the ratio of the paths between the first and second order LVs. Otherwise a correlated first order or bi-factor model may be more appropriate. For further details on these different models, see Rindskopf and Rose (1988). One then needs to demonstrate the convergent validity of the first order factors by examining the strength of the paths connecting the second order LVs to the first order LVs. Overall, we should expect a large percentage of the paths to be at 0.70 and above as well as adequate model fit. To adequately test convergent validity, the number of first order factors should be four or greater (three while statistically adequate would represent a just identified model for congeneric models).[3] Finally, as discussed above, the paths and model fit should still hold when applied in a nomological network of other factors.

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[2]An alternative but rarely seen purpose for using a second order factor model would be to separate reliability and validity estimates. For further details, see Rindskopf and Rose (1988).

[3]This requirement is also true for first order confirmatory factor analysis. LVs that have two items per construct, for example, do not represent a strong test and would lead to identification problems where parameter estimates are not unique if they are not connected to other LVs.

In general, it can be argued that it is becoming harder to find research articles that present only a simple confirmatory factor analysis on a set of items. Rather, greater evidence in the form of the factor's application in a structural model is typically presented. Such an approach provides additional evidence of the convergent, discriminant and construct validity of the measures. It stands to reason that a similar approach need to be made when considering the existence of a second order factor.

Statistical Power

The statistical power (i.e., the ability to detect and reject a poor model) is especially critical in SEM analysis. In contrast to the traditional hypothesis testing, the goal in SEM analysis is to produce a non-significant result. The reason is that the researcher is attempting to develop a theoretical model that accounts for all the covariances among the measured items. Therefore, a non-significant difference between the implied covariances derived from the parameter estimates for the model and those of the sample data is argued to be suggestive of support for the model. Yet, a non-significant difference may also be due to a lack of ability (i.e., power) to detect model misspecification. Ideally, for example, if one failed to include a path between two constructs, the analysis should detect this and reject the null by yielding a poor outcome (i.e., a significant difference) for the proposed model relative to the data.

Unfortunately, there continues to be a neglect of statistical power analysis in the behavioral sciences (Cohen 1992; 1988). In fact, according to Sedlmeier and Gigenrenzer (1989), the power of recent studies is still at the same low levels (i.e., .37) as those published in 1960. Furthermore, in their survey of 64 studies, Sedlmeier and Gigenrenzer note that only two mention the issue, and none estimate the power. The issue of power has been similarly raised in the IS field by Baroudi and Orlikowski (1989) and was recently shown to be low in studies trying to detect interaction effects (Chin et al. 1996).

There are at least two approaches for assessing the power of an SEM model. The first, presented by Saris and Stronkhorst (1984; see also Satorra and Saris 1985), works as follows: (a) take your focal model and develop an alternative model by adding an additional parameter (e.g., a standardized path of .2 between two LVs), (b) compute the implied covariance matrix based on this alternative model, (c) submit this covariance matrix to the focal model (which does not specify the existence of this additional parameter) to obtain the chi-square statistic (which now represents a non-centrality parameter), and (d) use this chi-square value and a given alpha level (typically .05) to calculate the power. This procedure thus requires the researcher to come up with various alternative models. A recent approach offered by MacCallum et al. (1996) does not require an alternative model. Instead, it uses the root mean square error of approximation to calculate power. The authors provide both tables and an SAS routine for calculating the power or minimum sample size to achieve the recommended level of .80. Until this new approach is better understood, one should attempt to use both procedures.

Thus, the question of appropriate sample size depends to a large extent on the power analysis. It is simply not acceptable to rely on simplified rules such as the number of parameters being estimated or on outdated Monte Carlo studies that provide sample size recommendations based on simple factor analytic models. Given that sample size is extremely dependent on the particular SEM model, explicit power calculations must be applied. While sample size is not an issue when the model is correct and the resulting fit is perfect, it becomes paramount for detecting misspecification. As Kaplan (1995) states, "Assessing power, therefore, must now become a routine part of establishing the statistical validity of an estimated model" (p. 117).

Capitalization on Chance

Researchers need to avoid the potential of slipping into an exploratory mode where the final results may be unduly influenced by the vagaries of the data at hand. SEM analysis works best in a confirmatory mode. Ideally, a set of a priori models are developed based on knowledge of the underlying substantive theories. But not surprisingly, the models that are initially tested are typically rejected. With modification indices and other such information, the researcher may follow a process of changing and re-estimating the model until it fits the data. The final model is often mistakenly believed to be correct, possibly due to the false assumption that finding the true model or a close approximation is inevitable if done in a correct exploratory manner. Another example of capitalizing on chance is the often used two-step process of filtering items through an initial set of exploratory factor analyses before submitting the remaining items to a "confirmatory" analysis. It should be clear that such a process should never be construed as confirmatory. Strong goodness-of-fit results in the second stage should be viewed primarily as an indicator of the skill of the researcher at deleting items during the exploratory stage.

A number of studies (MacCallum 1986; MacCallum et al. 1992; Spirtes et al. 1990) have shown that such post hoc modification procedures are unlikely to succeed. Within the IS discipline, Lee et al. (1997) have discussed this problem, while Chin and Todd (1995) provide an empirical example. Yet a large percentage of published research still performs such changes without concern toward either basing it on strong theoretical grounds (Breckler 1990; MacCallum et al. 1992) or performing appropriate cross-validation with a new data sample (see Browne and Cudek 1989; Cudek and Browne 1983). This has led MacCallum (1995) to formally state, "It would be appropriate for editors of journals publishing applications of SEM to reject papers employing the model generation strategy if authors ignore these concerns" (p. 33). Reviewers and editors in our discipline should be equally critical of such studies.

Given that much of the state of theoretical knowledge in the IS field is still in the formative stages where theoretical models and measures are often simultaneously developed, use of covariance-based SEM analysis is likely premature. For researchers interested in alternative SEM approaches geared more for exploration and model development, one should examine the TETRAD II (Scheines et al. 1994) and PLS (Barclay et al. 1995; Chin forthcoming; Falk et al. 1992; Fornell and Bookstein 1982; Lohmöller 1989) methodology.

Equivalent Models

For any given SEM model, there will often be alternative models that are equivalent in terms of overall model fit. Such models may produce substantially different explanations of the data. MacCallum et al. (1993) have shown that such equivalent models exist in published studies, often in large numbers. Breckler's (1990) survey indicates that all but one of 72 published research articles specifically acknowledge the possibility of alternative models. Researchers are encourage to read the articles by Hershberger (1994), Lee and Hershberger (1990), and Stelzl (1986), which describe procedures for generating equivalent models and MacCallum et al.'s (1993) paper, which offers advice on how to manage this problem.

Overall Model Fit at the Expense of Other Objectives

A final issue is the over-reliance toward overall model fit (or goodness of fit) indices. "Where is the goodness of fit measures?" has become the 1990s mantra for any SEM-based study. Yet, it should be clear that the existing goodness of fit measures are related to the ability of the model to account for the sample covariances and therefore assume that all measures are reflective. SEM procedures that have different objective functions and/or allow for formative measures (e.g., PLS) would, by definition, not be able to provide such fit measures. In turn, reviewers and researchers often reject articles using such alternate procedures because simply, these model fit indices are not available.

In actuality, models with good fit indices may still be considered poor based on other measures such as the R-square and factor loadings. The fit measures only relate to how well the parameter estimates are able to match the sample covariances. They do not relate to how well the latent variables or item measures are predicted. The SEM algorithm takes the specified model as true and attempts to find the best fitting parameter estimates. If, for example, error terms for measures need to be increased in order to match the data variances and covariances, this will occur. Thus, models with low R-square and/or low factor loadings can still yield excellent goodness of fit.

Therefore, pure reliance of model fit follows a Fisherian scheme similar to ANOVA that has been criticized as ignoring effect sizes (e.g., Cohen 1990, p. 1309). Instead, closer attention should be paid to the predictiveness of the model. Are the structural paths and loadings of substantial strength as opposed to just statistically significant? Most of the loadings should be at least 0.60 and ideally at 0.70 or above, indicating that each measure is accounting for 50 percent or more of the variance of the underlying LV. Standardized paths should be at least 0.20 and ideally above 0.30 in order to be considered meaningful. Meehl (1990) argues that anything lower may be due to what he has termed the crud factor where "everything correlates to some extent with everything else" (p. 204) because of "some complex unknown network of genetic and environmental factors" (p. 209). Paths of .10, for example, represents at best a 1 percent explanation of variance. Thus, even if they are "real," are constructs with such paths theoretically interesting?

In summary, a number of issues have been presented that should be addressed in all SEM-based papers. These issues are considered important because of their potential for invalidating the interpretation and conclusions of any SEM analysis. Technical issues such as identification and the assumptions and influence of data distributions on estimation functions (Bentler and Dudgeon 1996), while left out because of space considerations, are equally important and need to be addressed. Likewise, researchers interested in suggesting causality in their SEM models should consult the critical writings by Cliff (1983), Freedman (1987), and Baumrind (1983).

Luckily, over the past few years, an increasing number of excellent resources have appeared to support researchers interested in SEM analysis. First off, a number of general SEM textbooks are available (Bollen 1989; Dunn et al. 1993; Hayduk 1987; Loehlin forthcoming; Mueller 1996; Schumacker and Lomax 1996). Byrne's (1994) book is useful for explaining the interpretation of various SEM models (e.g., factor, multi-group, and multi-trait/multi-method analyses). Likewise, the EQS (Bentler 1995) and LISREL (Jöreskog and Sörbom 1993) manuals provide additional examples and discussion of the method. Hoyle's (1995) book is highly recommended for providing excellent chapters discussing most of the critical issues in SEM analysis in a relatively non-technical way. For more advanced issues, the reader should consult Bollen and Long (1993), Hayduk (1996), and Marcoulides and Schumacker (1996). There is also a journal, *Structural Equation Modeling: A Multidisciplinary Journal,* which began quarterly publication in January 1994. It is an excellent source for recent advances in SEM analysis. Unique to this journal is a teacher's corner that provides pedagogical discussion of various SEM issues. SEMNET is an e-mail discussion list that discusses SEM issues. Begun in February 1993, one can subscribe by sending an e-mail message to listserv@ua1vm.ua.edu. In the message, type "SUBSCRIBE SEMNET Jane Doe" where you would replace Jane Doe with your name. An FAQ ("Frequently Asked Questions") is maintained by Ed Rigdon as an "unofficial" supplement to SEMNET. This can be accessed through a web browser at http://www.gsu.edu/~mkteer/semfaq.html. Finally, another website with substantial SEM information can be accessed at http://students.gsm.uci.edu/~joelwest/SEM/index.html.

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