Although methods for latent variable modeling that allow a joint analysis of measurement and theory have become popular, they are not without difficulties. As these difficulties have become more apparent, several researchers have recently called for a "two-step approach" to latent variable modeling in which measurement is evaluated separately from theory. This implies that programs for covariance structure analysis are not needed because factor analysis and regressions would suffice for analysis. Before a return to earlier practice using seemingly simpler analysis tools can be recommended, it seems prudent to consider the assumptions underlying a two-step approach. At least four implicit assumptions can be identified: (a) theory and measurement are independent, (b) results of factor analysis specifications can be generalized to other specifications, (c) the estimators have desirable statistical properties, and (d) the statistical test in one step is independent of the test in the other. The authors show that these assumptions cannot be met in general and that some of them are logically inconsistent. Thus any wholesale adoption of a two-step approach could have serious consequences.

Assumptions of the Two-Step Approach to Latent Variable Modeling

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Programs for latent variable modeling such as LISREL (Jöreskog and Sörbom 1984), EQS (Bentler 1985), COSAN (McDonald 1978), RAM (McArdle and McDonald 1984) and PLS (Lohmöller 1984) have been widely used in behavioral research during the past decade (Bentler 1986; Bollen 1989; Long 1983). For example, Borgatta and Bohrnstedt (1988) reported that covariance structure models have been used in 12.4% of articles published in *Sociological Methods & Research* during its first 15-year period (from

AUTHORS' NOTE: The authors wish to thank Richard Bagozzi and Hans Baumgartner for their helpful comments on an earlier draft of this article. Please direct correspondence to Claes Fornell or Youjae Yi at School of Business Administration, University of Michigan, Ann Arbor, MI 48109.

SOCIOLOGICAL METHODS & RESEARCH, Vol. 20, No. 3, February 1992 291-320

1972 to 1987).[1] Along with the widespread application of these programs, it has become common practice to analyze theoretical specifications as well as their measurement counterparts. In EQS, for example, the analyst does not have to make a distinction between measurement and structural (theory) parameters. In other words, these programs have made it feasible to estimate both types of parameters either simultaneously (via LISREL, COSAN, EQS, or RAM) or at least where information from one set of parameters is considered in the estimation of the other set (PLS).

Before the advent of computerized programs for latent variable models, social sciences followed a long-standing practice of trying to establish acceptable measurement quality prior to using the measures in a substantive context. To a large extent, such procedures are still followed in test theory and general scale development and are also typically suggested in textbooks on methodology in the social sciences (e.g., Nunnally 1978). However, the increasing application of covariance structure analysis has led many researchers to de facto merge what used to be separate analyses of measurement and theory into a single context (Bentler 1978; Borgatta and Bohrnstedt 1988).

The joint estimation of theoretical and measurement submodels is, after all, what distinguishes the new programs of latent variable modeling from the old order of factor analysis for measurement items and thereafter some sort of regression analysis (including path analysis and simultaneous equations) for the substantive relationships. Estimation of measurement and structure within a single context has been emphasized by Bentler (1978) in his paper "The interdependence of theory, methodology, and empirical data," which can be traced back to Cronbach and Meehl (1955), who emphasized the importance of making substantive theory relevant to the process of test construction. It is also clear that simultaneous estimation can offer certain well-known benefits in terms of statistical properties. Nevertheless, the joint estimation of theory and measurement is not without difficulties (e.g., Bentler and Chou 1987; Cliff 1983).

As these difficulties have become more apparent, several researchers are starting to question the joint analysis, proposing that we return to a separation of measurement and theory.[2] For example, Anderson and Gerbing (1988) suggested that, although it is possible to estimate measurement and theoretical models simultaneously, it is not necessarily preferable to do so. Similarly, Lance, Cornwell, and Mulaik (1988) argued for separation of theory and measurement. In the context of detecting specification errors, Herting and Costner (1985) suggested a two-step analysis procedure in which one starts with a confirmatory factor analysis model and when the measurement model has been respecified to achieve a satisfactory fit to the data, one moves back to the original structural model and evaluates the overall fit. In general, the following reasons are given for separating measurement from theory.

First, interpretational confounding, a term coined by Burt (1973, 1976) and referred to by Anderson and Gerbing (1988) as well as Lance et al. (1988), is said to occur "as the assignment of empirical meaning to an unobserved variable which is other than the meaning assigned to it by an individual a priori to estimating unknown parameters" (Burt 1976, p. 4). Interpretational confounding occurs when the extent to which different forces determine the estimation of loadings is not carefully assessed. There are two types of forces: an epistemic force that relates theoretical constructs to observed measures, and a structural force that represents relationships among theoretical constructs. In a typical covariance structure analysis, all the parameters in the model, including the factor loadings that link an unobservable variable to its indicators, are estimated to minimize the difference between the observed and the expected covariance matrices. As interpreted by Anderson and Gerbing (1988), interpretational confounding "is reflected by marked changes in the estimates of the pattern coefficients when alternate structural models are estimated" (p. 418).

Another argument for separate analysis is misspecification, because the effects of incorrect specification are not confined to the misspecified portion of the model in case of simultaneous estimation. As is well-known from econometrics (e.g., Johnston 1984), the effects of misspecification (bias and inconsistency) are spread throughout the system of equations. This is the primary argument advanced by Lance et al. (1988).

Various forms of separate analysis, which have been suggested to avoid these problems,[3] will be collectively termed "two-step approaches," because they consist of two separate steps: an examination of the measurement portion isolated from the structural part, and a test of the substantive theory (e.g., Anderson and Gerbing 1988). The first step of the two-step approach is typically a confirmatory factor analysis of all measured variables where the factors are allowed to intercorrelate freely (e.g., Anderson and Gerbing 1988; Lance et al. 1988). Once an acceptable measurement model is found from a series of respecifications (as determined by covariance structure fit), attention turns to the second step of estimating the theoretical model.

Although Burt (1973, 1976) was the first to suggest a two-step analysis, his approach is very different from that of Anderson and Gerbing (1988). In the first step of assessing measurement validity, Burt (1976) proposed a separate factor analysis for each unobservable variable, whereas Anderson and Gerbing (1988) suggest confirmatory factor analyses for all latent variables. In the second step, Burt (1976) fixed the measurement parameters to the values obtained in the first step and estimated structural parameters only. In contrast, Anderson and Gerbing (1988) reestimated the measurement parameters as well as the structural parameters. Throughout this article, we will use "confirmatory factor analysis" for Anderson and Gerbing's approach and "separate factor analysis" for Burt's approach whenever a distinction needs to be made between them. However, a major point is that there are a number of implicit assumptions with both approaches. This is also true for the following statements by proponents of various two-step approaches. In the next section, we will identify and discuss these assumptions.

With respect to the advantage of the two-step approaches, Lance et al. (1988) concluded that: "Distinct analysis of the measurement and structural portions of latent variable or mixed manifest and latent variable models are desirable because construct validities of manifest measures are evaluated prior to evaluating hypotheses about relations about constructs" (p. 185). One may wonder how it is possible to evaluate construct validity of variables without reference to how they relate as constructs, but Anderson and Gerbing (1988) followed the same line of reasoning: "A researcher can build a measurement model that has the best fit from a content and statistical standpoint, where respecification may have been employed to accomplish this, and still provide a statistical assessment of the adequacy of the theoretical model of interest" (p. 419). They conclude with the following advice: "Structural equation modeling, properly employed, offers great potential for theory development and construct validation in the social sciences. If substantive researchers employ the two-step approach recommended in this article and remain cognizant of the basic principles of scientific inference that we have reviewed, the potential of these confirmatory methods can be better realized in practice" (p. 422).

These are strong recommendations from methodologists with long records of published papers in the area. Chances are that these recommendations will be followed in substantive research. Nonetheless, before returning to earlier forms of analysis and, in essence, discarding the basic distinct feature of LISREL, EQS, RAM, COSAN, and PLS, it seems prudent to consider the assumptions associated with a two-step approach. To date, very little attention has been given to identifying exactly what these assumptions are and examining the extent to which they can be met. Consequently, this is what we will attempt to do in this article. As will be discussed, we find that the assumptions implicitly imposed under a two-step approach are difficult to meet. Under some versions of the two-step approach, they are also logically inconsistent and statistically questionable.

THE ASSUMPTIONS OF A TWO-STEP APPROACH

Any two-step approach to covariance structure modeling that attempts to isolate measurement from the theoretical context (in which it is to be employed) assumes that:

- A1. Theory and measurement are independent of one another or can be treated as such.
- A2. Measurement validity established in the first step (via confirmatory factor analysis or separate factor analysis) can be generalized to other model specifications.
- A3. The estimators of a two-step approach are (asymptotically) unbiased, consistent, and efficient.
- A4. The statistical test in one step is independent of the test in the other.

Although these assumptions are not mentioned by advocates of the two-step approach, they are implicitly imposed when a two-step approach is used.

How reasonable are these assumptions? This question is central to ascertaining the soundness of two-step approaches. We will begin by discussing each of the above assumptions in detail and providing illustrations when appropriate. Let us start with the overriding assumption of independence of theory and measurement (data).

A1. THEORY-MEASUREMENT INDEPENDENCE

A fundamental assumption of any two-step approach is that measurement and substantive theory can be taken apart and treated separately. The basic tenet is that the structural portion of the model can be prevented from influencing the measurement properties (e.g., loadings). However, it is well known from philosophy of science that it is not possible to provide theory-neutral observations against theories to be tested (Passmore 1967; Pawson 1980). Theories in physics, for example, are not discovered by inductively generalizing from data, but by retroductively inferring probable hypotheses from conceptually organized data (Hanson 1958). It is not possible to interpret an observation independent of any consideration of theory.

There are many examples to illustrate why the separation of measurement from substantive theory is an artificial exercise that is not likely to enhance our knowledge about the phenomena under study. This is so for all sciences. The implication of theory-data interdependence is not necessarily that observation changes "reality" (as in quantum mechanics), but that the *interpretation* of an observation is always done in the context of some theoretical framework. Thus different theories may well provide different interpretations. Economic theory, for example, often interprets consumer behavior observations differently from behavioral theory. Gestalt and perception psychology also show that neutral observation is an illusion (see Sakahian 1982). Indeed, all empirical analyses rest on some (implicit or explicit) theoretical assumptions about phenomena and involve (implicit and/or explicit) structural models. Thus theoretical specification should be recognized as a necessary part of any research design, and such theoretical content should be explicitly stated and evaluated (Blalock 1969; Horan 1989).

It is generally agreed that measurement does not take place separately from substantive theory, but rather is part of an interlocking of observations and ideas (Anderson 1970; Cliff 1972; Fornell 1989; Guttman 1971; Luce 1972; Zinnes 1969; see also Duncan 1984 for an eloquent discussion on the connection between theory and measurement). Measurement properties do not have a status independent of a supporting network of relations. Indeed, the most advanced methods of measuring length rely on physical principles rather than merely geometric ones. For example, it is proposed that the standard definition of a meter be defined in terms of the assumed constancy in the period and wavelength of a laser (Hall 1978).

Theory will always guide measurement; at the very least, it suggests what to measure. But the interplay goes much deeper. As pointed out by Achinstein (1968): (1) observation (measurement), if it is to be relevant, must be interpreted; (2) that in terms of which interpretation is made is always theory; and (3) the theory not only serves as a basis of interpretation but also determines what is to be counted as an observation, problem, method, solution, and so on. That is, measurement without theory is analogous to an interpreter without language. Yet this is what the two-step approach claims to do.

If we acknowledge that our measures are theory-laden (in one way or another), we may begin to understand why Theory X produces different measurement properties (e.g., loadings) than Theory Y. In this light, it would not be useful to label this property "interpretational confounding" and to pretend to get rid of it by a two-step approach. Instead, we would probably be better off if the theory-data interdependence were explicitly acknowledged and dealt with by the appropriate methodology.

An Illustration

It is easy to find examples in which measurement is interdependent with substantive theory. We will use a study by a proponent of a two-step approach to show that if the theory proposed is altered, the measurement model is affected. In other words, when the structural model is changed, the parameters of the measurement model also change.

Building on work from social exchange theory and channels of distribution, Anderson and Narus (1984) presented a model of the distributors' perspective of the distributor-manufacturer working relationships, which is summarized in Figure 1A. Two constructs were used as exogenous variables: outcomes given the comparison level (O|CL) and outcomes given the comparison level for alternatives (O|CL_A). The endogenous variables were manufacturer control and cooperation/satisfaction constructs. Underlying theories and description of measures will not be given here (see Anderson and Narus 1984). The model in Figure 1A gives a satisfactory fit to the data: $\chi^2(74) = 89.15, p = .11$ (AGFI = .894, RMR = .058).

Suppose we are interested in evaluating the two measures of O|CL: X_5 and X_6. As seen in Figure 1A, the standardized factor loadings are .47 (.18) and .82 (.29) for X_5 and X_6, respectively (with the standard errors in parentheses). Their associated reliabilities are .22 and .68. What would be the effects of changing the theory in which these measures are used? One way to demonstrate such effects is by positing an alternative theory such that manufacturer control and cooperation/satisfaction constructs are interchanged. This alternative theory is illustrated in Figure 1B. Results show that the model fit is still satisfactory: $\chi^2(74) = 87.81, p = .13$ (AGFI = .897, RMR = .066). However, the standardized loadings are now .66 (.18) and .59 (.16), with the reliabilities of .43 and .34 for X_5 and X_6, respectively.

Note that the factor loadings and reliabilities of measures X_5 and X_6 are substantially different from those obtained under the original theory. For example, X_6 would probably be considered a reliable measure (.68) under the original theory, but of questionable reliability (.34) under the alternative theory. Also, X_6 is more reliable than X_5 under the original theory, whereas X_5 is more reliable than X_6 under the alternative theory. Consequently, this example illustrates that measurement properties could vary with the theoretical context in which the measures are employed.

A2. MEASUREMENT VALIDITY FROM THE FIRST STEP CAN BE GENERALIZED

Confirmatory Factor Analysis

Confirmatory factor analysis is most frequently employed for examining measurement properties in the first step of the two-step approach (e.g., Anderson and Gerbing 1988; Lance et al. 1988).[4]

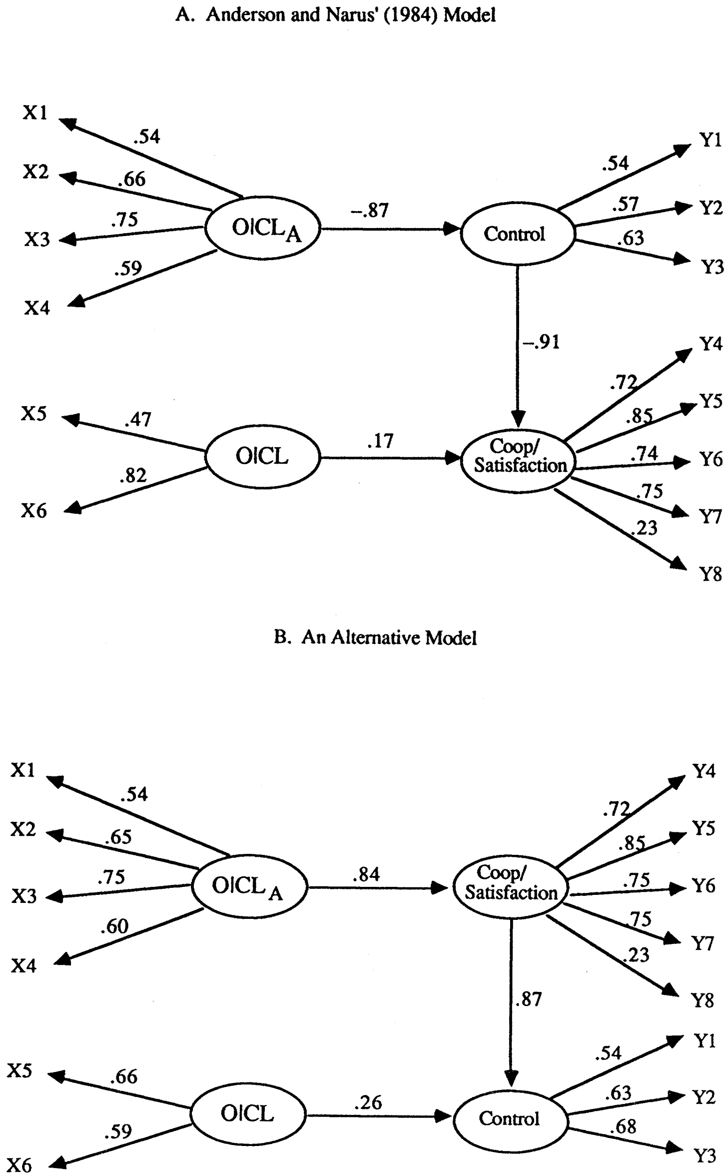

Figure 1: An Illustration of Theory-Measurement Interdependence

NOTE: $O|CL_A$ = outcome given the comparison level for alternatives; O|CL = outcome given the comparison level

However, unless separation of theory from data is admissible, there is a major problem in applying it. One could perhaps reason in the following manner. We know that the assumption of theory-data independence is false, but for purposes of simplification, let us proceed as if we could isolate data from theory. That is, we follow the advice of the proponents for a two-step approach and use confirmatory factor analysis of manifest variables. The criterion for evaluating such a confirmatory factor analysis model can be expressed as:

$$\rho_{ad} = \rho_{a\xi}\, \rho_{\xi\xi*}\, \rho_{\xi*d}$$

where *a* is any indicator of a latent variable ξ, and *d* is any indicator of another latent variable ξ^* (Anderson and Gerbing 1988, p. 415). This equation shows that the fit of the model depends not only on the correlations between a latent variable and its indicators, but also on correlations between latent variables and between a latent variable and indicators of other latent variables.

If the assessment of measurement quality is independent of any substantive theory governing the relationships among the latent variables, we may also define quality in terms of measurement error: the difference between a recorded value and the corresponding true value. It is then logically implied that the existence of measurement error in a variable x is epistemologically independent of the presence of measurement error in another variable y, defined independently of x (Hoppe 1980). However, as can be seen from the above equation, confirmatory factor analysis is not consistent with this. The measurement quality of a variable (assessed with the magnitude of measurement error) may become mathematically dependent on the measurement of other variables included in the analysis. If we have truly isolated the evaluation of measurement from the evaluation of theory, one must ask: How is it possible that the quality of measure x is dependent on the quality of measure y? There is something amiss in logic here.

The two-step approach assumes that data can be separated from theory but fails to establish the quality of measurement of a variable independent of other variables. One implication of this is that assessment of measurement quality for a particular variable may vary with the exclusion and inclusion of other variables in the system. If the quality of measurement of variable x is, in part, determined by the quality of measurement of variable y, what is it that can explain this dependence if not the theory? That is, confirmatory factor analysis *does* incorporate substantive theory in the evaluation of measurement. At the minimum, the theory implies which variables are relevant for the phenomenon under study, because the set of variables chosen for analysis will affect the quality of measurement of a particular variable.

Confirmatory factor analysis is often interpreted as a "measurement" model void of theory about the latent variable relationships (Anderson and Gerbing 1988). However, a specific theory is nevertheless implicitly embedded in such confirmatory factor analysis, as will be illustrated later. The fact that the theory is loosely or implicitly specified does not eliminate its existence or role in affecting measurement properties in analysis; neither does the fact that there are no degrees of freedom associated with the latent variable relationships with respect to the covariance structure. Having no overidentifying restrictions for the "structural portion" of the confirmatory factor analysis does not mean that a substantive theory is not represented by the model. It only means that the theory, as is the case with theories tested by multiple regression, cannot be evaluated in terms of its covariance structure.

An Illustration

Suppose we are interested in testing the theory of reasoned action (e.g., Ajzen and Fishbein 1980) with covariance structure analysis. Figure 2A illustrates the key relationships predicted by the model, which is henceforth referred to as Model 1. That is, behavioral intentions (BI) mediate all the effects of attitudes toward an act (Aact) and subjective norms (SN) on behavior (B). Note that no direct paths from Aact or SN to B are posited by the theory. In other words, intentions are sufficient to predict behavior.

If one follows a two-step approach, one may start by evaluating measures with the confirmatory factor analysis. The confirmatory factor analysis model is given in Figure 2B, and this model will be referred to as Model 2. In this confirmatory analysis model, the four factors are free to correlate (e.g., Anderson and Gerbing 1988). It is

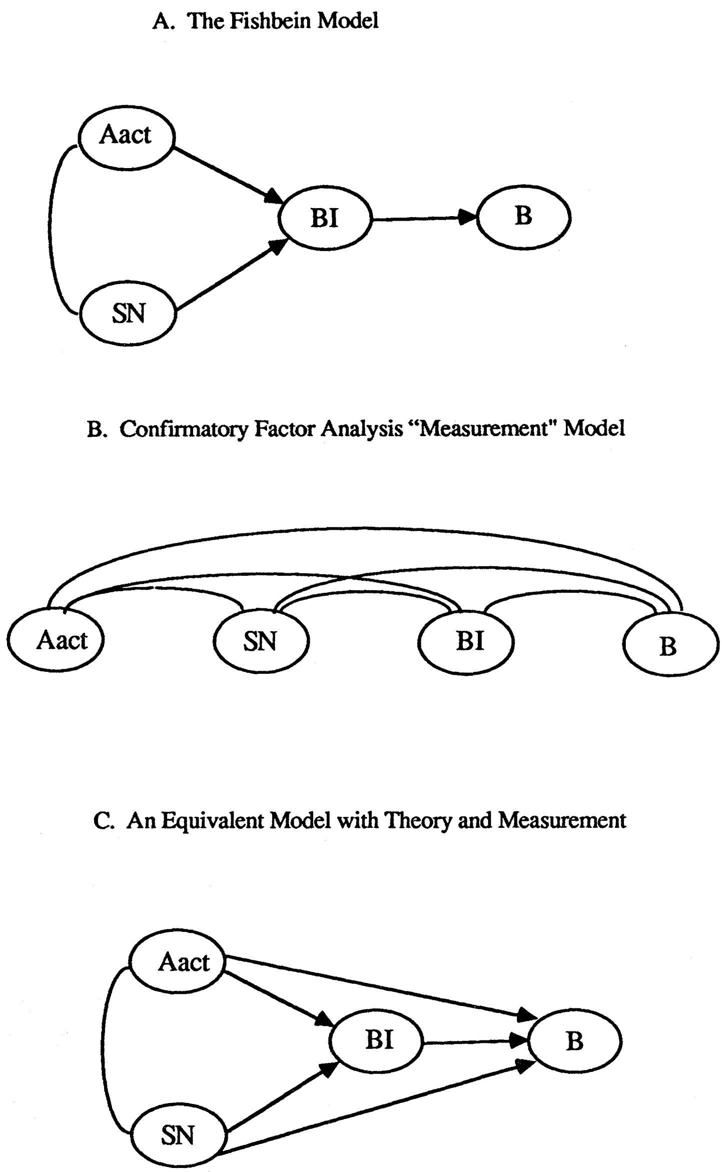

Figure 2: An Illustration of a Theory Embedded in Confirmatory Factor Analysis

often claimed that this type of confirmatory factor analysis tests measurement properties only—no substantive theory is supposed to be involved (e.g., Anderson and Narus 1984). However, the so-called "measurement model" in Figure 2B (i.e., Model 2) is, in fact, equivalent to an alternative theoretical model (i.e., Model 3) in Figure 2C that posits the direct paths from Aact and SN to B as well (Bagozzi and Yi 1989; Bentler and Speckart 1979, 1981). That is, all the parameters of one model are functions of the parameters of the other (see the appendix for proof of equivalence between Models 2 and 3). Because the two models are equivalent, they will produce the identical covariance matrices and the same fit to the data (see Bentler 1978).

Therefore, in assessing the confirmatory factor analysis model (Model 2), one is testing: (a) theory (that all the variables are directly and indirectly associated), and (b) measurement. However, this is equivalent to testing implicitly an alternative theoretical model (Model 3). In other words, a substantive theory is implicitly specified in confirmatory factor analysis. Consequently, a major problem with the two-step approach is that it does *not* do what it purports to do. It does not test "measurement" independent of "theory." Furthermore, it introduces ambiguity into the interpretation of results because the substantive theory governing the structure of relationships is not made explicit. A confirmatory factor analysis model is not simply a measurement model because measurement is evaluated in the context of a certain substantive theory. In fact, what is being tested are both measurement *and* a theory that is not only implicit but also (in this case) *different* from the researcher's original hypothesis. In addition, the two-step approach introduces ambiguity into the interpretation of results because the substantive theory governing the structure of relationships is not made explicit. It is often argued that structural models should be made explicit, because empirical results are necessarily affected by theoretical specifications (Horan 1989).

This leads to another problem of the two-step approach with confirmatory factor analysis. Because the analysis implicitly imposes a structure that may be different from that of the original theory, it yields an evaluation of measures in the "wrong" context (see Figure 2). In fact, the two-step approach consists of both validating measures in the context of one theory (i.e., a theory different from the theory of interest) and employing these "validated" measures in the context of another theory (the substantive theory posited by the researcher). It is difficult to understand why measures should be validated in the "wrong" context before they are employed in the "right" context. Rather, measurement validation should be done in a theoretical context to which it belongs. Otherwise, one is likely to draw inappropriate conclusions.

Suppose, for example, that a poor fit is obtained from a confirmatory factor analysis model. The analyst may either (a) conclude that the measures are not valid and discard them, or (b) attempt to improve the fit by modifying the measures (through respecification, exclusions, or inclusions of measures). In the first case, measures that might have been appropriate in the context of the original theory may be falsely discarded, resulting in a Type I error. In the latter case, measures are being improved in a context different from the one in which they eventually will be used. Hence the results may not be very relevant for the subsequent analysis of the original theory.

Suppose, on the other hand, that one obtained a satisfactory fit in confirmatory factor analysis. One may now conclude that the measures are satisfactory. However, it is still possible that these measures may turn out to be poor in the context of the theory of interest. Let us illustrate the case where a validation of measures via confirmatory factor analysis leads to a rejection of the measures, but when the same measures are used in conjunction with a substantive model, the overall result is acceptable.

Suppose one is interested in testing the Fishbein model of behavioral intentions, as shown in Figure 2A. Assume that each construct is measured with three indicators, except for behavior (B), which has two indicators. The observed moment matrix for this example is shown in Table 1. A two-step approach suggests that one should test the measurement first by confirmatory factor analysis. If the measures of the four constructs (Aact, SN, BI, and B) are validated with a confirmatory factor analysis model, the results are: $\chi^2(38) = 50.84$, $p = .08$. Suppose we use a rule of thumb, $p > .10$. This model would be rejected, and one might conclude that the measures are poor. One may then either discard these measures or attempt to modify them (e.g., either by adding or deleting items).

TABLE 1: Correlation Matrix for the Model in Figure 2

BI_1	1.00										
BI_2	.62	1.00									
BI_3	.41	.54	1.00								
B_1	.30	.31	.38	1.00							
B_2	.35	.32	.39	.62	1.00						
A_1	.35	.22	.27	.20	.18	1.00					
A_2	.31	.33	.36	.20	.17	.63	1.00				
A_3	.26	.27	.30	.18	.19	.52	.65	1.00			
SN_1	.20	.13	.15	.14	.12	.12	.19	.14	1.00		
SN_2	.23	.28	.26	.13	.14	.11	.12	.16	.62	1.00	
SN_3	.24	.22	.26	.11	.14	.23	.24	.21	.64	.58	1.00

However, when measurement and theory are tested jointly, the following results are obtained: $\chi^2(40) = 50.84, p > .11$. Using the same rule of thumb, this suggests that the measures are satisfactory in the context of the theory. As a consequence, one might have drawn the incorrect conclusion that measures are unacceptable. In this particular example, the difference in fit between the confirmatory factor analysis model and the original model is not that large. However, the difference could be much more substantial in other contexts, especially when the number of latent variables becomes large enough that confirmatory factor analysis introduces many irrelevant parameters into the original model (see our discussion of A3 later in the article).

Separate Factor Analyses

As mentioned earlier, Burt (1976) proposed separate factor analysis for each latent variable to assess measurement validity.[5] Note that one does not investigate the measurement properties of a construct in the context of other constructs in this approach. The quality of measurement for a given construct does not depend on other constructs or the relationships among them because each is validated separately.

Whereas this approach is logically consistent in the sense that only one set of measurement loadings is estimated and that these loadings cannot be affected by other constructs, it is also prone to yielding misleading conclusions about measurement and theory. For example, separate factor analyses can point to errors in the specification of substantive theory when, in fact, the theory is correct. Conversely, this type of analysis can also lead the analyst to conclude that the measurement portion of the model is acceptable when actually it is not.

An Illustration

Suppose the model in Figure 3A represents a correct model. The model specifies two causally related constructs (A and B). A latent variable B, measured with four indicators Y_1-Y_4, is determined by another latent variable A indicated by four items X_1-X_4. Assuming no sampling errors, the observed covariance matrix will be reproduced exactly with the true parameter values shown in Figure 3A. That is,

TABLE 2: Covariance Matrix for the Model in Figure 3A

Y_1	1.345							
Y_2	0.447	0.668						
Y_3	0.521	0.313	0.965					
Y_4	0.596	0.358	0.417	0.877				
X_1	0.350	0.210	0.245	0.280	1.00			
X_2	0.175	0.105	0.122	0.140	0.250	0.525		
X_3	0.210	0.126	0.147	0.168	0.300	0.150	0.680	
X_4	0.245	0.147	0.171	0.196	0.350	0.175	0.210	0.645

each element in the sample moment matrix can be expressed as a function of the factor loadings and the paths. The resulting covariance matrix is given in Table 2. A sample size of 200 has been assumed for this example. Obviously the fit is perfect ($\chi^2[19] = 0.00$, $p = 1.00$, AGFI = 1.00, RMR = 0.00) because the model specification is correct and no sampling errors are assumed.

Let us now create misspecifications to demonstrate the limitations of a two-step approach with separate factor analyses. In Figure 3B, the measurement models are now misspecified by interchanging one indicator of each construct (i.e., X_4 and Y_2). Let us now follow Burt's two-step approach by examining measurement first and theory subsequently. The results of separate factor analysis models for each construct, as shown in Figure 3B, suggest a perfect fit for both constructs ($\chi^2[2] = 0.00$, $p = 1.00$, AGFI = 1.00, RMR = 0.00), even though the model is misspecified. Given such findings, one may conclude that the measures are perfect, when in fact they are not. Thus separate factor analysis may fail to detect specification error in measurement and fail to reject misspecified items (e.g., X_4). However, given perfect fits for measurement models, the two-step approach would suggest that the next step of theory testing be taken by assessing the overall model shown in Figure 3C. When the model is estimated with the factor loadings and error variances fixed at the values obtained from the first step, as Burt (1976) suggested, this model gives an unsatisfactory fit to the data: $\chi^2(33) = 70.05$, $p = .00$ (AGFI = .06, RMR = .08). Even when the model is estimated with free measurement parameters, one gets similar results: $\chi^2(19) = 53.51$, $p = .00$ (AGFI = .83, RMR = .06). These results indicate the rejection of the full model in

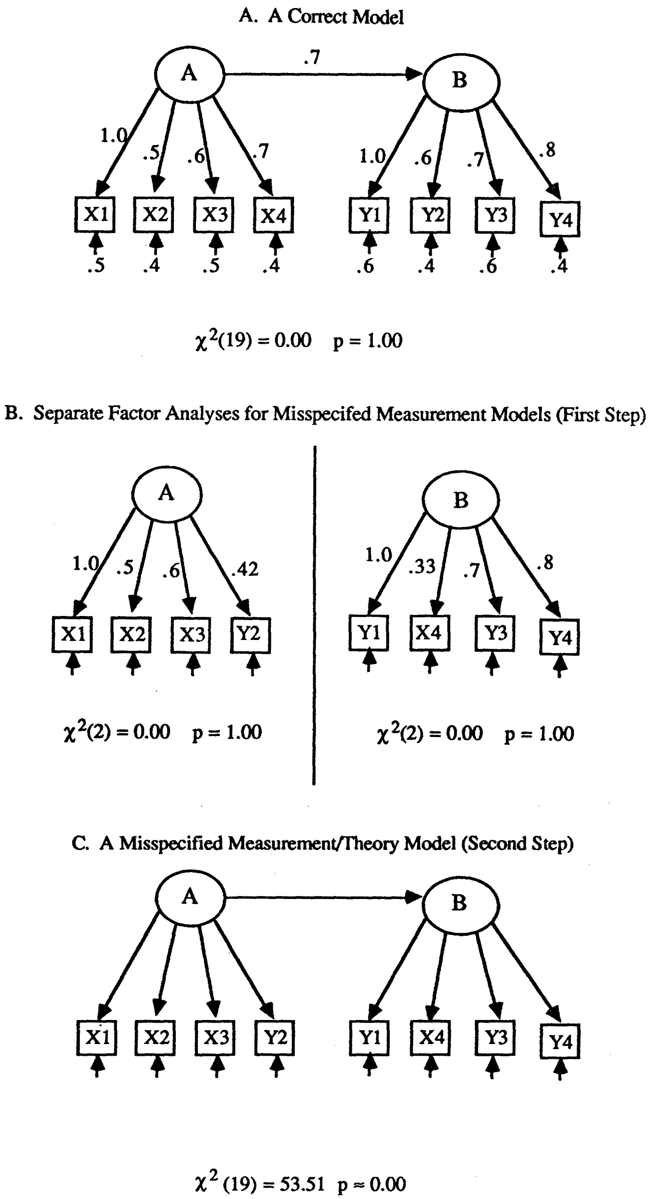

Figure 3: An Illustration of the Failure to Detect Misspecification in Measurement

Figure 3C. Given perfect fits for measurement models in the first step, the two-step approach would suggest that the theory is incorrect.

Notice that we have a correct theory (i.e., A causing B) but faulty measures (i.e., Y_2 for A and X_4 for B) in this example. However, following the two-step approach via separate factor analyses leads to the erroneous conclusion that the measures are perfect but the theory is incorrect.[6]

There are also practical difficulties in applying separate factor analyses to measurement validation. If separate factor analyses are to be used for measure validation, each construct must have at least four measures. When there are fewer than three measures, the measurement model is not identified. If a construct has three measures, the factor analysis model for the construct is just identified ($df = 0$) and a statistical test is not possible (Bentler 1978). Although it is advantageous to have multiple measures of any latent variable, obtaining four or more measures of each latent variable is not always possible.

Good measures typically should be highly correlated not only with other measures of the same construct but also with measures of other related constructs (i.e., nomological validity). In separate factor analysis, one is evaluating measures in a vacuum by examining measures of each construct separately. However, measures that appear satisfactory in isolation may be poor in the context of a theory (e.g., measures may not be predictive of related constructs). Separate factor analyses evaluate measures by using within-construct covariances only. The information from across-construct covariances is ignored.

A3. ESTIMATORS ARE (ASYMPTOTICALLY) UNBIASED, CONSISTENT, AND EFFICIENT

Another presumable assumption (or, at least, desirable condition) implicit in the two-step approach is that the estimators obtained have desirable statistical properties. However, as we will soon show, the two-step approach fails on this score as well.

Let us first examine the statistical problems with a two-step approach using separate factor analyses. Because separate factor models limit the analysis to a portion of the system of equations, certain relevant variables and relationships may be omitted. A brief illustration can be given in this respect. Suppose a correct model is specified in Figure 4A. We have two latent variables, A and B, that are correlated with each other. Construct A and B share one measure, X_4, in addition to three unique measures. Let A and B be standardized to a variance at unity and a mean of zero. If we use factor analysis to validate each construct with a two-step approach, two separate factor analyses will be performed, as illustrated in Figure 4B. Then,

True Measurement Model: $X_4 = \lambda_4 A + \lambda_8 B + \varepsilon_4$
Separate Factor Analysis Model: $X_4 = \lambda_4{}^* A + e_4$.

If we multiply these two equations by A and take expectations, we get

$$\mathrm{Cov}\,(A, X_4) = \lambda_4 + \lambda_8\,\phi_{AB}$$
$$\mathrm{Cov}\,(A, X_4) = \lambda_4{}^*.$$

Thus, $\lambda_4{}^* = \lambda_4 + \lambda_8\,\phi_{AB}$.

Note that $\lambda_4{}^*$ differs from λ_4 by a factor of $\lambda_8\,\phi_{AB}$. That is, an estimator of $\lambda_4{}^*$ from a separate factor analysis will be biased except when λ_8 or ϕ_{AB} is zero. Similarly, it can be shown that $\lambda_8{}^*$ is different from λ_8; that is, $\lambda_8{}^* = \lambda_8 + \lambda_4\,\phi_{AB}$.

Thus the separate factor analysis model can yield biased estimates of loadings and thus induce misleading conclusions about reliabilities of the measures. The estimators will be biased more severely when the theoretical relationships among the constructs (e.g., ϕ_{AB}) are strong and when loadings omitted by conducting separate factor analysis (e.g., λ_8) are large.

Separate factor analysis also yields less efficient estimators than does a joint estimation procedure. Using a two-construct model in Figure 3 as an example, there are three submatrices in the observed moment matrix: Sxx, Syy, and Syx. In a joint estimation of theory and measurement, as in LISREL, the estimation of loadings uses the information about the variances and covariances among all variables. For example, we know that

$$Syx = \Lambda_y (I - B)^{-1}\,\Gamma\Phi\Lambda_x$$

where Λ_x and Λ_y are loadings for measures of latent variables (Jöreskog and Sörbom 1984, I. 8). Note that the estimates of the loadings for X_is

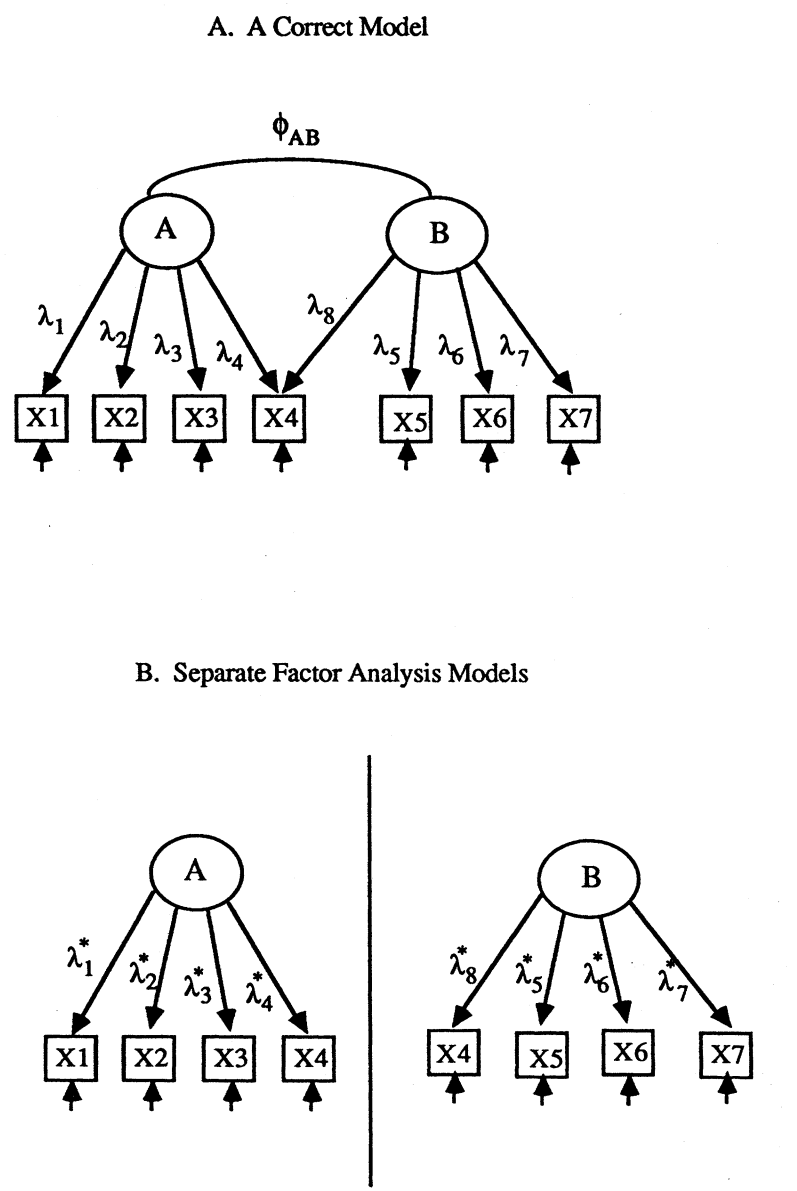

Figure 4: An Illustration of Biases in Estimators From Separate Factor Analysis

and Y_is are functions of **Syx** (as well as **Sxx** and **Syy**) in a joint (full-information) estimation. But the separate factor analysis uses only a submatrix of the observed moment matrix. Specifically, it uses only **Sxx** and **Syy** in estimating the loadings for X_is and Y_is, respectively, ignoring the covariance information **Syx**. By not using a substantial portion of information, the separate factor analysis procedure will yield less efficient estimators.

The two-step approach using confirmatory factor analysis of *all* factors suffers from inefficiency as well. Here, all factors are specified to correlate freely, which is equivalent to adding unnecessary parameters in one's model. For example, confirmatory factor analysis introduces two additional paths in the model of Figure 2. This is analogous to one type of specification error called adding irrelevant predictors in the econometric literature. It is well known in the context of a single-equation regression model that adding irrelevant parameters yields less efficient estimators by increasing standard errors (Johnston 1984). However, the same problem can occur for a simultaneous equation model when irrelevant parameters are added.

In the context of latent variable modeling, Bentler and Mooijaart (1989) have shown that when there are two competing nested models, the more parsimonious model yields an estimator of the common parameters that has smaller sampling variance. That is, the covariance matrix of the estimators under the more parsimonious model is always smaller than that under the less parsimonious one. Bentler and Mooijaart (1989) concluded that "From a statistical viewpoint, it is apparent that the more parsimonious model will be associated with parameter estimates that are more precise than estimates of the same parameters obtainable under a more general model that contains superfluous parameters" (p. 317).

Note that one's theoretical model is nested within a confirmatory factor analysis model which posits that all latent constructs covary with each other (see Figure 2). Suppose we have m constructs and n structural parameters in the original model. Then, by estimating the loadings via confirmatory factor analysis, the number of irrelevant parameters added to the model will be

$$\frac{m(m-1)}{2} - n.$$

To the extent that the confirmatory factor analysis introduces additional parameters, the estimators will become less precise. Therefore, the theoretically derived original model should be favored over the confirmatory factor analysis model in terms of precision in estimation.

A4. TESTS ARE INDEPENDENT

To the extent that statistical significance tests are used, covariance structure analysis is basically a method for confirmatory analysis. That is, the statistical theory used in covariance structure analysis is based on the premise that the model itself has been specified completely prior to any analysis of data (Bentler and Chou 1987). However, two-step approaches are exploratory in nature. The first step usually includes a trimming of measures such that data fit is improved. If one alters the model as a consequence of unacceptable fit to data (by dropping or adding measures), the analysis is no longer "confirmatory." Once one starts adjusting a model in the light of data (here, measurement part), the model loses its status as a hypothesis (Cliff 1983). When such data-driven model modification is done, the probability values given for the statistics may be incorrect and the "true model" may not be found (Bentler and Chou 1987; MacCallum 1986). In the absence of strong a priori rationale, such analysis would become a boundless exercise in empiricism that contributes little toward scientific progress (Fornell 1983).

Although empirical model modification may produce a model with a better fit to sample data, the analyst has no evidence that the fit is also improved with respect to the population. Any empirical search for model-data fit affects the probability levels of all subsequent tests based on the same data. This is what happens in the two-step approach: The *same* data set that has been used for deciding which measures to include is used again for testing the theoretical relations among the variables. Conducting such a sequence of model modifications may capitalize on chance and increase the risk of making a Type II error (Bentler 1978; Kaplan 1989). Cross-validation should be conducted whenever an initial model is modified on the basis of the data (Cliff 1983; Cudeck and Browne 1983).

Proponents of the two-step approach (Anderson and Gerbing 1988) defend their reliance on statistical testing by referring to Steiger, Shapiro, and Browne (1985), who show that sequential chi-square difference tests are asymptotically independent. Such chi-square difference tests would be appropriate when all models in the sequence are hypothesized a priori, but not when some of the models are developed after looking at the data. Furthermore, as mentioned by Steiger et al. (1985), sequential chi-square statistics calculated for nested models on the same data are still correlated. Therefore, the chi-square test for the final model (chosen through respecifications) would not be independent of the previous chi-square tests on the same data. A search process within the same data tends to yield inflated fits for modified models, increasing the probability of a Type II error. The implication is that the statistical tests for models that have gone through a series of empirical modifications cannot be interpreted as traditional inferential statistics.

The two-step approach has another problem associated with the chi-square test from both statistical and logical viewpoints. Let us examine this problem with the example given in Figure 2. The two-step approach would involve testing Model 3 of Figure 2C in the first step and then testing Model 1 of Figure 2A in the second step. The test statistic from Model 3 will follow a chi-square distribution, assuming that Model 3 is true. If Model 3 is correct, however, the test statistic resulting from Model 1 will follow a noncentral χ^2 distribution (Satorra and Saris 1985). That is, the chi-square value for Model 1 is, in fact, the noncentrality parameter of the noncentral χ^2 distribution under the alternative model (Model 3). In general, the chi-square test is based on the assumption that a given model is correct. So, by interpreting the chi-square value from Model 1 as a χ^2 statistic, a researcher implicitly assumes that a different model (Model 1) is correct, which contradicts the previous assumption that Model 3 is correct. The logic is difficult to follow, because either Model 1 or Model 3 is correct, but not both.

DISCUSSION

Even though methods are now available for the joint analysis of measurement and theory, several researchers have recently recommended a return to the old tradition of establishing measurement validity *before* the measures are used in a substantive context. Certainly, much empirical work is performed in this way, and, before the advent of programs such as LISREL, RAM, EQS, COSAN, and PLS, it was very difficult to carry out a joint analysis of measurement and theory. Before any wholesale recommendation can be made for returning to seemingly simpler and separate forms of analysis, however, one would first have to identify the assumptions associated with this approach and evaluate the extent to which they can be met.

There are at least four categories of assumptions behind a two-step approach. It is assumed that (a) theory and data are independent, (b) results of factor analysis specifications can be generalized to other model specifications, (c) the estimators have desirable statistical properties, and (d) the statistical test in the second step can be treated as independent of the first. It was found that it is difficult to meet these assumptions.

Measurement and theory are not independent, no matter what method is used to separate the two. Measurement and theory are inextricably linked because theoretical concepts are defined not merely in terms of their empirical conditions (measurement) but also in terms of the theoretical context in which they occur (Hanson 1958; Kuhn 1970). As a consequence, measurement validity is always examined in the context of some substantive theory whether or not the analyst wants this to be the case. That is, even if a two-step approach is used, measurement is evaluated within some substantive theoretical framework. Attempts to artificially separate measurement from theory are likely to produce misleading conclusions. We provided several examples to illustrate this. From a purely technical point of view, sampling error and less-than-perfect model specification will always result in measurement-theory interdependence with respect to covariance structure analysis (Kumar and Dillon 1987).

It is also shown that parameter estimates cannot automatically be generalized from one model specification to another. At the very least, the consequences of the differences in specification must be considered. If the factor model is a correct specification, the structural model may not also be correct. Consequently, model specifications in any two-step approach are less than ideal. Generally, they exhibit bias and inconsistency as well as inefficiency. Steiger et al.'s (1985) findings that sequential chi-square difference tests of the covariance fit are independent do not provide a license for adjusting a model in light of the same data against which it is being tested. Not only will this make standard statistical inference difficult but it may also cause bias in estimates (Larson and Bancroft 1963; Selvin and Stuart 1966).

On the other hand, one cannot overlook the difficulties with a one-step approach. It requires that a researcher have an explicit theory before measurement validation. But when one does not have a substantive theory, such as in pretests, scale development, in the early stages of confirmatory studies, or in construct validation studies as an end, a two-step approach might be justified (cf. Bagozzi 1983). In such cases, the results should be interpreted as exploratory with the understanding that they may not generalize to other contexts. A one-step approach (especially with simultaneous estimation) may also be difficult to apply in the case of severe misspecification because the errors are spread throughout the parameter system.

It is important to realize that the issue of a one- versus a two-step approach is much broader than the choice of statistical estimation method. Regardless of what form of estimation is used, the estimation method does not distinguish between a measurement model and a structural model. The question is: Should information about one part of the system be ignored even though its inclusion might change the way we view the system as a whole? If the answer is no, the question with respect to latent variable modeling becomes: How do we best incorporate this information? This is a matter of estimation (which depends on many factors not discussed here).

However, we are not claiming that a one-step approach is superior to a two-step approach. It is important to have a balanced perspective on the issue of measurement validation and theory testing by understanding the relative strengths and weaknesses of a one-step and a two-step approach. Because the difficulties with a one-step approach are well documented in the literature, they have not been mentioned. On the other hand, the literature has not discussed the assumptions associated with the alternative approach. We believe that the choice between a one-step versus a two-step approach must be governed by the extent to which one can live with the assumptions of the latter as the price of overcoming the difficulties in applying the former. Consequently, we have attempted to pinpoint the implicit assumptions underlying the two-step approach, examine the extent to which the assumptions can be met, and illustrate the consequences of violating assumptions.

APPENDIX

This appendix shows that the two models in Figures 2B and 2C are equivalent. For simplicity, let us denote Aact, SN, BI, and B by A, S, I, and B. Assuming that they are standardized, we can denote the intercorrelations among four latent variables in Figure 2B with ϕ_{AS}, ϕ_{AI}, ϕ_{AB}, ϕ_{SI}, ϕ_{SB}, and ϕ_{IB}, where ϕ_{AB} refers to the correlation between A and B. Similarly, the model in Figure 2C can be represented with the parameters of ϕ^*_{AS}, γ_{11}, γ_{12}, γ_1, γ_2, and β; for example, γ_{11} designates the path from A to I. Then we have the following equations.

$$\phi_{AS} = \phi^*_{AS}$$
$$\phi_{AI} = \gamma_{11} + \gamma_{12}\phi^*_{AS}$$
$$\phi_{AB} = \gamma_{21} + \gamma_{11}\beta + (\gamma_{22} + \gamma_{12}\beta)\phi^*_{AS}$$
$$\phi_{SI} = \gamma_{12} + \gamma_{11}\phi^*_{AS}$$
$$\phi_{SB} = \gamma_{22} + \gamma_{12}\beta + (\gamma_{21} + \gamma_{11}\beta)\phi^*_{AS}$$
$$\phi_{IB} = \beta + \gamma_{11}\gamma_{21} + \gamma_{12}\gamma_{22} + \gamma_{11}\phi^*_{AS}(\gamma_{22} + \gamma_{12}\beta) + \gamma_{12}\phi^*_{AS}(\gamma_{21} + \gamma_{11}\beta)$$

All the parameters in the model in Figure 2B are functions of the parameters of the model in Figure 2C. Therefore, the two models are equivalent.

NOTES

1. Special issues are omitted in calculating the percentage because the publication of a special issue may overrepresent an area. When articles published in special issues are included, the percentage becomes 9.7%.

2. We will use the term "measurement" to describe the set of epistemic relationships between manifest indicators and latent variables, and the term "theory" or "structure" to describe any kind of relationship, whether a causal relation or covariation, among latent variables.

3. There are many other important problems in latent variable modeling, such as improper solutions empirical underidentification and use of categorical variables (e.g., Bollen, 1987; Muthen and Kaplan, 1985; Rindskopf, 1984). However, these are not considered here because they are the problems common to both one-step and two-step approaches.

4. We use the term "confirmatory factor analysis" model to include not only the relationships of the indicators to their underlying factors but also the intercorrelations of the factors with one another, which is consistent with the recommendation by Anderson and Gerbing (1988). This is also called a "group-factor" model by Rindskopf and Rose (1988).

5. Separate factor analysis has also been referred to as "within-block factor analysis" (Burt, 1976).

6. We have also examined the possibility that the pattern of normalized residuals would show the nature of misspecification. In the first step, all the normalized residuals were zero. When we looked at the normalized residuals for each factor in the second step, none of them were large. Specifically, within-construct normalized residuals ranged from –0.011 to +0.005 among the four indicators for Construct B (i.e., Y_1, X_4, Y_3, and Y_4), and from –.003 to .000 for the four indicators of Construct A (i.e., X_1, X_2, X_3, and Y_2). These results suggest that the measurement part of the model is correct. On the other hand, six across-construct normalized residuals were large. These results altogether seemed to suggest that the theoretical part is wrong, whereas the measurement part is correct. Thus the pattern of normalized residuals could still be misleading as to the nature of misspecification.

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