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Choosing PLS path modeling as analytical method in European management research: A realist perspective

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ABSTRACT

Today, there is heightened controversy about the value of partial least squares (PLS) path modeling as a quantitative research method, including within the domain of European management research. Critical lines of argument within the management and psychology literature assert that there is no reason to use PLS path modeling at all. At the same time, authors using PLS path modeling continue to advance fallacious arguments to justify their choice of method. This paper identifies flaws on both sides—invalid arguments in favor of using PLS path modeling and invalid arguments opposing its use—within the context of a unifying framework and a realist philosophy of science.

You are in Rome. Something wonderful is waiting for you in Brussels. Unfortunately, there are only two flights available to you. One goes to Frankfurt, and the other goes to Paris. As you ponder this choice, some people confront you. “Frankfurt?” they scoff. “Why fly to Frankfurt? Brussels is in Belgium—Frankfurt is not even in the same country. It would be *ridiculous* to fly to Frankfurt, because the city you are trying to reach is Brussels.” They confidently conclude, “Frankfurt doesn't work. Fly to Paris.”

1. Introduction

Perhaps there has always been controversy between different approaches to structural equation modeling (SEM), ever since Herman Wold unveiled a composite-based alternative to Karl Jöreskog's common factor-based innovation. In the last few years—perhaps in response to a new vibrancy within the partial least squares (PLS) path modeling community—the tenor of this controversy has become sharper. Antonakis, Bendahan, Jacquart, and Lalive (2010, p. 1103) declared, “… there is no use for PLS whatsoever… We thus strongly encourage researchers to abandon it.” Referring to PLS path modeling, Rönkkö and Evermann (2013, p. 19) assert, “… it is very difficult to justify its use for theory testing over [factor-based] SEM…” Writing in a psychological journal, Rönkkö, McIntosh, and Antonakis (2015, p. 82) conclude, “… PLS should not be adopted as a tool for psychological research.”

Perhaps the most recent and impactful contribution in this vein is an editorial from the editors in chief of *Journal of Operations Management* (Guide & Ketokivi, 2015, p. vii), who warned, “We are desk rejecting practically all PLS-based manuscripts, because we have concluded that PLS has been without exception the wrong modeling approach in the kinds of models OM researchers use.” However, Guide and Ketokivi went further, clarifying that desk rejection was primarily a response to researchers making incorrect claims about PLS path modeling (p. vii): “Consequently, we will automatically desk reject a manuscript that makes incorrect claims about the applicability of the estimator (obviously, any estimator, not just PLS).”

Guide and Ketokivi's (2015) editorial points to two different problems related to the use and understanding of PLS path modeling. Today, too many researchers offer a flawed rationale for choosing PLS path modeling as their method, citing strengths or advantages for PLS path modeling that do not exist. At the same time, critics offer flawed reasons to avoid PLS path modeling. Some of these critical arguments falsely ascribe advantageous properties to the factor-based approach to SEM that do not exist, while some

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are based on flawed evidence about the performance of PLS path modeling.

The aim of this paper is to review and correct both types of errors—both alleged strengths or advantages and alleged weaknesses of PLS path modeling which have not been supported with valid evidence, despite publication in well-regarded academic journals. This paper presents an alternative understanding of structural equation modeling, one which is consistent with aspects of factor-based and composite-based approaches to SEM which both users and critics of PLS path modeling have tended to ignore.

It is easy enough to find oneself embracing the beliefs and biases of one school of thought, to the point where contrary arguments and perspectives seem not only wrong but nonsensical and even dangerous. Unsuspecting researchers may quietly succumb to a "methodological tribalism" (Saunders & Bezzina, 2015, p. 298). While Saunders and Bezzina (2015) studied the implications of a broad qualitative vs quantitative orientation among (primarily) European management researchers, the same divide arises between researchers with differing quantitative backgrounds. As with Saunders and Bezzina (2015, p. 303), this is not a call for "methodological relativism," that is, for simply withholding judgment. Rather, the aim here is to overcome misunderstandings by embracing a pluralism of quantitative methodologies which are all rigorously consistent with a single framework. This paper is written in the hope that it will (a) help researchers to make better design and methods choices, (b) help writers to avoid crucial errors in explaining their choices, and (c) help to move the SEM dialog forward.

2. A framework for understanding structural equation modeling

Some disputes are particularly immune to resolution because the different sides are operating on the basis of fundamentally different assumptions, philosophies or worldviews. In such cases, the "Yes, it is" from the one side and the "No, it isn't" from the other side may both be correct (or incorrect) because the two positions are referring to different realities. It may therefore be helpful to briefly outline a framework within which one might understand arguments about different approaches to structural equation modeling.

Statistical tools that fall within the family of SEM methods potentially could be used for a variety of purposes, but here the focal purpose is better understanding of the behavior of unobserved conceptual variables. From a scientific realist perspective (Chakravartty, 2007; Haig & Evers, 2016; Leplin, 1984), these unobserved conceptual variables are defeasibly real entities, like the microorganisms that were once invisible or the subatomic particles that remain beyond perception today. While they may remain unobservable themselves, these conceptual variables are of interest because of their direct or indirect causal consequences in the observable world. From an empiricist perspective, in contrast, only the observable phenomena would be considered real, with unobserved conceptual variables being no more than labels for certain observed empirical regularities (Creath, 2014). An operationalist, in the tradition of P. W. Bridgman, Edwin Boring or S. S. Stevens, might define a "conceptual variable" to be nothing more than the application of a particular quantitative methodology (like factor analysis) to data collected under a particular protocol (Chang, 2009). But again, for the scientific realist, unobservable conceptual variables are themselves real and are independent of data, of statistical procedure, and of the researcher, though mistakes and misunderstandings can lead to incorrect beliefs about particular conceptual variables.

So far, this framework has described two types of variables—the unobserved conceptual variables found in theoretical models and the observed variables found in datasets. Structural equation modeling explicitly incorporates a third type of variable. In this framework, the common factors in factor-based SEM and the composites in composite-based SEM are together classed as "proxy variables" (Wickens, 1972; Woolridge, 2009), defined as variables that stand in for other variables which may be unobservable or simply unavailable. Factor-based SEM and composite-based SEM use common factors or composites to represent unobserved conceptual variables. These representations are formed out of the observed variables in a dataset. Because the representations are formed out of data, they share the strengths and weaknesses of data, to some degree. As such, the common factors or composites in SEM's statistical models are not equivalent to or identical with the conceptual variables that populate theoretical models. So, beyond the important statistical differences between the factor-based and composite-based approaches to SEM, there is a crucial underlying similarity.

3. Flawed arguments *in favor of* PLS path modeling

As Guide and Ketokivi's (2015) editorial confirms, a great many management research manuscripts that employ PLS path modeling do a very poor job of justifying their choice of statistical method. Unfortunately, many of the invalid arguments employed by these papers date to the origin of PLS path modeling, and they are stubbornly repeated in books and methodological manuscripts, despite contrary evidence, some of which is common knowledge in the factor-based SEM community. Granted, much statistical practice is justified using invalid arguments, when researchers even bother to offer a justification (Gigerenzer, 2004; Wasserstein & Lazar, 2016). Taking an approach that is less common or that deviates from the norm is more likely to provoke questions. Gigerenzer (e.g., 2004) points out how null hypothesis significance testing with p-values is mindlessly executed in paper after paper—and mindlessly described in textbook after textbook. When was the last time a writer was asked to justify use of a p-value? Reviewers have even been known to demand them—without providing a reason, and without regard to the potential to mislead (Ziliak & McCloskey, 2008). But when the editors of *Basic and Applied Social Psychology* took the opposite course and banned p-values from their journal (Trafimow & Marks, 2015), there was an uproar—followed by thoughtful reconsideration at the highest levels (Wasserstein & Lazar, 2016). It is probably true that many manuscripts employing the common factor-based approach to SEM either fail to justify their choice of technique or provide a justification incorporating invalid arguments (Cliff, 1983). But the factor-based approach is the most widely known approach to SEM, and so applications of that approach may face less scrutiny. Regardless, researchers who make unjustified claims in support of the use of PLS path modeling can expect an unwelcoming reception for their work.

3.1. *Low sample size*

Data weaknesses were a primary motivation for Wold to create a composite-based alternative to factor-based SEM. Wold touted the ability of PLS path modeling to "work"—that is, to produce parameter estimates and standard errors—even when there were fewer cases or observations than variables, and when the available observations were not even mutually independent (Wold, 1988). This stood in contrast to maximum likelihood-based factor analysis, which can fail entirely when sample size is low. In the context of linear regression, it has been noted that the same statistical method can imply different minimum sample sizes depending on the particular purpose or criterion that is being pursued—whether the research is looking for cross-validated R^2, or for sufficient statistical power, or for something else (Maxwell, 2000, pp. 434–5). Still, low sample size was cited by Ioannidis (2005) as a primary driver in the publication of false scientific findings—regardless of analytical method.

Given the focus on predictive validity that is widely shared among PLS path modeling users, the simulations of Dana and Dawes (2004) are especially relevant. Their study examined out-of-sample R^2 in regression. The authors created large populations of simulation data and then for each population, (1) drew a sample, (2) estimated model parameters from the sample, (3) predicted the dependent variable at population level using the parameter values estimated from the sample. They found that ordinary least squares (OLS) regression weights produced the highest out-of-sample R^2 only when both sample size was quite large and true predictability (the true population R^2 for the dependent variable) was high. At the other end, when sample size was small and/or true predictability was low, simple unit or equal weights—that is, just summing the (standardized) predictors—outperformed regression weights.

Becker et al.'s (2013) simulations confirmed this result in the context of PLS path modeling. Consistent with Dana and Dawes' (2004) results for regression, Becker et al. (2013) found that, at low sample sizes, researchers would do as well to simply sum their multiple indicators and forget about weights. Becker et al.'s (2013) simulations demonstrated bias in PLS parameter estimates when sample size is low. Yes, PLS path modeling will produce parameter estimates even when sample size is very small, but reviewers and editors can be expected to question the value of those estimates, beyond simple data description. With respect to both composite-based and factor-based approaches to SEM, if sample size is small, the best course is to get more data.

Sometimes (as an anonymous reviewer has pointed out), more data are not obtainable. Sometimes, indeed, evidence is not available to allow a researcher to make an impactful contribution in a given area. Before the advent of CERN's Large Hadron Collider, it was impossible to obtain experimental evidence for the existence of the Higgs boson subatomic particle (http://home.cern/topics/higgs-boson). Before the upgrading of the Laser Interferometer Gravitational-Wave Observatories (LIGO), no researcher had been able to demonstrate the existence of gravitational waves (Abbott et al. 2016). In other cases, a population itself may be countably finite. In some areas of statistics, adjustment procedures may be available to account for this deviation from the standard assumption of an infinite population (e.g., Royall, 1970). However, the generalizability of findings is limited to the population from which data are sampled. Making a significant contribution will require that this population be of particular interest in itself. So it will be the nature of the population that justifies the small sample size, and not the small sample size that justifies the choice of PLS path modeling. Whether PLS path modeling performs better than alternative approaches in analysis of data from finite populations is a little-explored research area.

3.2. Non-normal data

Jöreskog's (e.g., 1969) creation of an inferential basis for factor analysis initially relied on maximum likelihood (ML) estimation. Jöreskog (1969) proved analytically that, when its assumptions held, no method yielding unbiased estimates could be more statistically efficient than ML estimation. In creating PLS path modeling, Herman Wold was pushing back against the distributional assumptions that supported ML estimation (Dijkstra, 2010), which included an assumption of conditional multivariate normality (Finney & DiStefano, 2013). Since those early days, PLS users have linked the multinormality assumption not with ML estimation in particular but with factor-based SEM broadly: “CBSEM [factor-based SEM] generally requires a multivariate normal distribution of the sample data” (Peng & Lai, 2012, p. 470).

For many years now, there have been a variety of procedures available for estimating the parameters of factor-based models. The distributional requirements underlying these various procedures have ranged from strict to loose to almost none. ML estimation itself is somewhat robust to modest violations of multinormality, especially in regard to parameter estimates (Finney & DiStefano, 2013). While the earliest of these techniques—Browne's (1984) weighted least squares approach—required infeasible sample sizes numbering in the thousands, more recent alternatives have been shown to work well at sample sizes as small as 200 (Lei & Wu, 2012; Yuan & Bentler, 1998). Moreover, there is little cause to compare ML estimation of factor model parameters with PLS estimation of composite model parameters. The factor-based model and the composite-based model are two different models. Even if ML estimation did a poor job of estimating a factor-based model in a certain situation, this failure would argue for a change of estimation method, not for a change from factor-based SEM to composite-based SEM.

3.3. Formative/reflective

“Reflective measurement” is a term used to describe a situation where a set of observed variables are jointly dependent upon another variable which is not itself observed. With appropriate constraints on the residual variances of the observed variables, this arrangement describes a common factor model, so it is easy to understand how “reflective measurement” might represent the norm in factor-based SEM. The reverse arrangement, where the unobserved variable is modeled as dependent on the observed variables, is then known as “formative measurement.” The factor-based SEM literature struggles mightily with formative measurement, on both practical and conceptual grounds (e.g., Bollen & Bauldry, 2011; Diamantopoulos & Winklhofer, 2001; Edwards, 2011). Given this, researchers sometimes argue that they must use PLS path modeling because “their concepts are formative.”

A realist perspective clarifies these issues significantly. If conceptual variables transcend data, then it is impossible to “form” conceptual variables out of data. By definition, conceptual variables are themselves the causes of data. Both the factor-based and the composite-based approaches to SEM form *proxies*, not conceptual variables, out of data. Because PLS path modeling is a composite-based method, it creates proxies as weighted composites (Rigdon, 2012), consisting of one or more variables. In the factor-based approach to SEM, proxies are formed as common factors (except for single indicators), even when it may appear otherwise (Rigdon et al. 2014).

Among its estimation options, PLS path modeling includes two which have been known for decades as “Mode A” and “Mode B.” Many writers associate Mode A estimation with “reflective measurement” and associate Mode B with “formative measurement” (e.g., Hair, Hult, Ringle, & Sarstedt, 2014, pp. 42–43). This is an illusion. Both modes create composite proxies, because PLS path modeling cannot do anything else. Using Mode A instead of Mode B means using correlation weights (Waller & Jones, 2010) instead of OLS regression weights (Becker, Rai, & Rigdon, 2013; Rigdon, 2012). Unlike OLS regression weights, correlation weights ignore collinearity among predictors. This means that users of correlation weights do not experience parameters having unexpected signs due to the impact of collinearity on weights, and will not be misled into deleting indicators based on collinearity-driven signs and inflated standard errors. As confirmed by Becker et al. (2013) in the PLS context, Dana and Dawes (2004) demonstrated that, while correlation weights yield a somewhat lower in-sample R^2 than OLS regression weights, they yield a higher out-of-sample R^2 when sample size and true predictability are moderate, potentially covering a much larger range of practice than the special conditions required for OLS regression weights to excel. So there can be good reason to choose Mode A or Mode B within a PLS path model, but this has nothing to do with a choice between "formative" and "reflective." Thus, researchers do face real choices, between common factor proxies and composite proxies, and between regression weighted composites and correlation weighted composites. In contrast, the terms "formative" and "reflective" only obscure the statistical reality.

3.4. Exploratory

Wold recognized that factor-based SEM required the researcher to have a well-developed statistical model in mind. He also saw that there were circumstances where data were available but the prior knowledge was not. A poor factor model may produce no results whatsoever, while a PLS path modeling analysis is very likely to produce parameter estimates and bootstrapped standard errors. Thus, Wold (1985, p. 589) recommended his composite-based method for situations that were "data-rich but theory-primitive." This association between PLS path modeling and exploratory analysis continues in the literature (Hair et al. 2014, p. 2, 14).

From a realist perspective, there is a substantial problem with using any SEM method in an exploratory context. Findings drawn from SEM analysis face a substantial validity challenge, relating specifically to the proxies that represent unobserved conceptual variables. For findings to be judged valid, the proxies themselves must be valid representations of particular conceptual variables. In an exploratory context, a researcher may have no idea what sorts of conceptual variables may be at work, and thus may be in no position to make any statements, pro or con, about the validity of the proxy variables in a model. So, as in the case of low sample size, PLS analysis can be executed in an exploratory environment, but it is unlikely to lead to a significant contribution—unless the dataset alone is so interesting and unique that data description itself amounts to a contribution. That could happen, if data are rare, difficult to obtain, and (within the scope of this journal) relate directly to constituencies and issues of especially keen or urgent interest to European management researchers. More likely, researchers who attribute their choice of PLS path modeling to the exploratory nature of their research will be declaring, in effect, that their work has no contribution to make.

4. Flawed arguments *against* the use of PLS path modeling

Clearly, a substantial number of researchers are building justifications for choosing PLS path modeling out of flawed arguments. Just as clearly, a phalanx of incorrect or unsupported claims are being presented to back a rejection of PLS path modeling as an analytical method. Some of these invalid arguments are tied to weaknesses in the factor-based approach which the factor-based SEM community has long ignored, while some of these issues have only become clear in recent years. The position here is not necessarily that these arguments are incorrect but that they are invalid as arguments favoring the factor-based approach to SEM over PLS path modeling, particularly from a realist perspective.

4.1. Biased parameter estimates

From the birth of PLS path modeling, it has been known and acknowledged that the method yields biased estimates of factor model parameters. Wold (1982, p. 27, 28) spoke of his method as being a "deliberate approximation" to ML factor analysis, accepting less accuracy in exchange for greater speed and relaxed assumptions. A number of studies have used simulation to demonstrate this bias (e.g., Aguirre-Urreta & Marakas, 2013; McDonald, 1996; Rönkkö & Evermann, 2013). As various PLS path modeling texts note, estimates of factor model loadings will tend to be biased upwards (away from 0), while estimates of paths between factors will tend to be biased downwards (toward 0).

However, all of these simulations, and perhaps Wold's own thinking, were flawed (Wold, 1982, p. 25). Some of these studies were flawed in multiple relevant ways. One flaw is common to all. A simulation must begin by specifying a population—a true state from which data are sampled. Even though these studies aimed to evaluate the performance of *composite-based* PLS path modeling, all of the simulations noted here began by defining a truth consistent with a *common factor* model. Thus, these simulations evaluated PLS models that were misspecified relative to the population. Statistical methods in general perform less well when the model is misspecified, and the same is true of PLS path modeling. Factor-based SEM itself is known to perform less well when the model being estimated is inconsistent with the population (e.g., Hu & Bentler, 1998), but scholars have repeatedly chosen to evaluate PLS path modeling using discrepant populations.

Simulations employing a correct population have shown that PLS path modeling estimates are consistent (Becker et al. 2013). Becker et al.'s (2013) simulations first defined composite-based populations, and then examined the performance of PLS path modeling in analyzing samples drawn from those populations. Becker et al. (2013) found the bias in PLS path modeling estimates approaching 0 as sample size increased. Even in these simulations, at lower sample sizes, PLS parameter estimates showed clear bias, with the nature of that bias varying based on simulation conditions. Thus, Becker et al.'s (2013) results provided further evidence against the use of PLS path modeling at low sample sizes. No simulations using composite-based populations have obtained results contradicting those of Becker et al. (2013).

Individual simulation studies critical of PLS path modeling have included additional design errors. Aguirre-Urreta and Marakas (2013) criticized the behavior of PLS path modeling in the context of "formative measurement." Aguirre-Urreta and Marakas (2013) created the population for their simulation using a common factor model. To minimize the problems that can be encountered when modeling a "formative" relationship with a factor-based model, Aguirre-Urreta and Marakas (2013) specified a population where a set of observed variables, along with another common factor, served as predictors of a focal common factor. Statistical identification for this model (a rather involved issue for factor-based SEM) was achieved by including other common factors that were exclusively dependent on the focal common factor, making it a second order factor. The PLS path model that the authors estimated, however, was different (Rigdon et al. 2014). In the PLS path model, the observed variables, modeled as *predictors* in the population, were now *components* of a composite—a composite which, in turn, was predicted by another composite. In the population, the observed variables and a common factor were predictors jointly, but in the PLS path model the predictor composite was the only predictor of a composite defined by the observed variables. These differences between models, compounded with the factor-based nature of the simulation population, invalidated Aguirre-Urreta and Marakas' (2013) conclusions regarding the behavior of PLS path modeling.

Rönkkö and Evermann's (2013; Rönkkö et al. 2015) simulations included a different design flaw. Rönkkö and Evermann's (2013) simulations appeared to demonstrate that PLS parameter estimates could be not only inconsistent but *bimodal*, a shocking deviation from the bell-shaped distribution that researchers would be inclined to expect. Rönkkö and Evermann's (2013) results suggested that PLS path modeling could hardly be trusted as a statistical method at all.

Rönkkö and Evermann's (2013) results were obtained by specifying a model that violated the known conditions under which the PLS path modeling estimation algorithm works. This algorithm requires that every composite proxy must be correlated with at least one other composite (e.g., Rigdon, 2013). The PLS path modeling algorithm alternates what are called "inner proxies" and "outer proxies." The inner proxy for a given composite is formed from other composites that have a direct relationship with the given composite, in the statistical model. If a composite is uncorrelated with all other composites in the model, the algorithm fails. Rönkkö and Evermann (2013) specified a population model with two common factors, with the population correlation between them set at 0. Then they attempted to estimate PLS path models using these data. The choice of population ensured that the PLS estimation algorithm would only function when random sampling error pushed the actual correlation between composites away from 0. Hence the simulation produced a bimodal distribution, with estimates on the positive side and estimates on the negative side, and little in between. PLS path modeling can certainly accommodate 0 paths between composites, but it cannot accommodate a composite that is orthogonal to all other composites in a model.

Far from demonstrating the untrustworthy nature of PLS path modeling, Rönkkö and Evermann's simulations showed what happens when you "break" a statistical method, asking it to work outside of its boundary conditions. Rönkkö and Evermann (2013) could easily have demonstrated a similar limitation in factor-based SEM, if that had been their purpose. Their simulation model included three observed variables loading on each of the two common factors. If the model had instead specified that four observed variables loaded on one factor while two loaded on the other, then the zero population correlation would have caused factor-based SEM to fail, just as PLS path modeling failed, because the factor model would have been under-identified (Bollen, 1989). When a statistical model is under-identified, parameter estimates are inconsistent and other negative consequences follow. The "three indicator rule" for identification of factor models specifies that a model will be identified if each common factor has at least three "congeneric" indicators (loading on that factor alone, with no residual correlations). The "two indicator rule" specifies that the model can be identified with only two congeneric indicators per factor as long as each factor is correlated with some other variable in the model. Rönkkö and Evermann included *just enough* indicators for each factor in the population model so that estimation of the factor model would not fail when the factor correlation was 0, and so that it would seem that PLS path modeling, by contrast, was unreliable. If either common factor had had only two indicators instead of three, factor-based SEM would have also failed, perhaps even more spectacularly. In sum, the undesirable behavior attributed to PLS path modeling in each simulation study should actually be attributed to design flaws in the studies themselves, not to a fundamental weakness in PLS path modeling.

4.2. Not a latent variable method

Factor-based SEM is said to be distinguished from other multivariate statistical methods because it is a technique that models "latent variables." By contrast, it is said that PLS path modeling "is not a latent variable method," and on this basis that it is not structural equation modeling at all (Rönkkö et al. 2015).

"Latent variable," like the noun "construct" (Michell, 2013), is a term with multiple potential meanings. In different usages, the term appears to mean either (a) a conceptual variable in a theoretical model which is believed to affect the behavior of other variables, or (b) a common factor. If by "latent variable," someone intends the second meaning, then of course it is correct to say that PLS path modeling, a composite-based method, is not a factor-based method, but this on its face appears to be mere methodological tribalism. It would be no less pointless to assert that composite-based PLS path modeling is better than the factor-based approach because the factor-based approach is not composite-based.

On the other hand, if "latent variable" refers to an unobserved conceptual variable, then the argument is incorrect. From a scientific realist perspective, conceptual variables are as crucially important to PLS path modeling as they are to the factor-based approach to SEM. Without conceptual variables, a proxy is no more than some function of the data. There can be no question of validity, because the proxy, formed from the dataset, represents nothing beyond the dataset. The composite-based approach and the factor-based approach alike cannot be anything more than exercises in data description. It may well be that, in practice, many applications of both approaches are no more than this, but the two approaches are equally stripped of their potential to build knowledge if their proxies stand only for themselves.

It has also been argued that the patterns of correlation captured by common factor models imply a common cause, so that common factor models provide evidence for the existence of unobserved conceptual variables in a way that composite-based models do not. However, identical patterns can be induced by processes that lack this common cause (van der Maas et al. 2006, 2014), though the statistical models implied by these complex processes may be beyond the power of existing software to estimate. Moreover, given that factor models are generally no more than approximations to data from real situations (Jöreskog, 1969; MacCallum, Browne, & Cai, 2007), common factor proxies cannot be generally assumed to carry greater significance than composite proxies in regard to the existence or nature of conceptual variables. Moreover, the multiple meanings associated with the terms "latent variable" and "construct" make these terms no more useful than "formative" and "reflective."

4.3. No overall fit test

Jöreskog's (e.g., 1969) maximum likelihood factor analysis became an inferential statistical method thanks to an overall test statistic that, under assumptions, follows a central χ^2 distribution. This statistic can be used to formally test the null hypothesis that the data-based covariance matrix of the observed variables is equal to the model-implied covariance matrix, within sampling error. PLS path modeling, being a composite-based method more closely tied to regression, offers no such test. Therefore it is argued that PLS path modeling is not in position to falsify and reject models in the same way as the factor-based approach to SEM. Critics of PLS path modeling have suggested that this is a fatal flaw in PLS path modeling, making it fundamentally less suitable for serious research.

Of course, the factor-based SEM community itself does not think much of the χ^2 statistic. Fit evaluation in factor-based SEM tends to focus on alternative fit indices, not on the χ^2 itself. The null hypothesis is assumed to be false (MacCallum et al. 2007), so that, "If a sufficiently large sample were obtained this χ^2 statistic would, no doubt, indicate that any such non-trivial hypothesis was statistically untenable." (Jöreskog, 1969, p. 200).

Understanding structural equation modeling as involving conceptual variables, observed variables and proxies, moreover, leads to an even more fundamental question about the advantage, if any, that accrues to factor-based SEM on the basis of the χ^2 test statistic. Statistical analysis is not an end in itself. The realist aims to reach conclusions about the conceptual variables in a theoretical model. Structural equation modeling facilitates this process to the extent that the proxies (common factors or composites) in statistical models, built from observed variables, are valid representations of the conceptual variables. If they are, then researchers can learn about the behavior of the conceptual variables through the behavior of the proxies. If the proxies are not valid, then the statistical model is uninformative regarding the conceptual variables.

The χ^2 test statistic tells the researcher essentially nothing about this most fundamental validity question. What *does* it indicate? According to Jöreskog (1969, p. 201), “If a value of χ^2 is obtained, which is large compared to the number of degrees of freedom, this is an indication that more information can be extracted from the data.” So the χ^2 might indicate whether or not the common factors in a model account for all systematic variance among the observed variables. But that is not the same thing as indicating whether the proxies are valid representations of particular conceptual variables. It might well be that valid proxies could be formed while leaving a substantial amount of the observed variables' systematic variance un-accounted for. Similarly, a good χ^2 value, indicating that the common factors do account for all systematic variation in the observed variables, offers no reason to believe that those factors are valid proxies for particular conceptual variables—because the same χ^2 value applies to the statistical model, no matter what conceptual variables are hypothetically being studied. In terms of the validity of the factor proxies employed in a statistical model, the χ^2 statistic is no more than a very sophisticated and powerful answer to the wrong question.

4.4. Measurement error

One of the seemingly most compelling criticisms of PLS path modeling is that the method fails to address measurement error. The presence of measurement error can be shown to induce bias in parameter estimates (e.g., Rigdon, 1994), not just in parameters directly linked to a single error-tainted observed variable but across an entire model. Factor-based SEM, it is argued, has a built-in mechanism accounting for measurement error—a mechanism which composite-based PLS path modeling lacks, leaving PLS path models vulnerable to the negative consequences of measurement error in a way that factor-based structural equation models are not.

Being composite-based, PLS path modeling partially reduces the impact of any random variance within individual components (Rigdon, 2012). The variance of a sum of components is equal to (a) the sum of the components' individual variances plus (b) twice the sum of the covariances among the components. Random variance is universally orthogonal. The correlated parts of the components thus are counted three times (once in the original variance and twice in the covariances), while random variance is represented only once. If the components are weighted, as in PLS path modeling, then the variance of the sum is similarly weighted. The weighting in PLS path modeling is aimed at maximizing correlations. Again, with random variance being orthogonal, components that are high on random variance will tend to be under-weighted, and this will tend to further reduce the influence of random variance. However, random variance will not be completely eliminated in this way, so that the PLS path modeling user will still be exposed to some share of any negative consequences.

The “measurement error argument” figures prominently in the literature critical of PLS path modeling (e.g., Antonakis et al. 2010; Rönkkö & Evermann, 2013). Nevertheless, the measurement error argument is not a valid objection to the use of PLS path modeling, because *neither* the factor-based approach nor the composite-based approach to structural equation modeling protects users from measurement error.

This position runs counter to the vast bulk of factor-based SEM literature, but the roots of the argument go as far back as Wilson (1928). A researcher might represent a factor model linking p observed variables y_i ($i = 1$ to p) to a single common factor F as:

$$y_i = \lambda_i F + \varepsilon_i$$

The p terms ε_i ($i = 1$ to p) are specific factors or residuals, the part of each y_i not accounted for by the common factor, F. These terms have often been described as “measurement errors” (e.g., Jöreskog, 1983). No such terms figure in PLS path modeling, because PLS path modeling is a method of composites and not factors. With “measurement error” thus explicitly represented in the factor model and not in PLS path modeling, researchers could easily believe that the factor-based approach addresses measurement error while the composite-based approach does not.

Yet the failure of the factor-based approach to address or account for genuine measurement error is apparent on both conceptual and analytical grounds. Consider again the conceptual framework featuring observed variables, conceptual variables and proxies. Defining the ε_i terms in the factor model as “measurement errors” implies that the common factor F is that which is to be measured. Yet the original purpose of the research effort was to learn about the behavior of a conceptual variable, not to learn about a common factor. These two different goals could be pursued simultaneously *if* the conceptual variable and its common factor proxy were identical or equivalent. For that matter, the factor model residuals would indeed be “measurement errors” if it could be assumed that the common factor proxy was identical to the conceptual variable that it was intended to represent.

For common factors, such an identity is essentially impossible due to the phenomenon of factor indeterminacy (Guttman, 1955; Mulaik, 2010; Schönemann & Steiger, 1976). With a composite, given the values of the composite's observed variable components and a set of weights, it is easy to determine the one correct value for the composite. With a common factor, however, this is almost always impossible. There is *no one best value* for a common factor, except at a hypothetical limit, even if data and model parameters are fully known. A given common factor can be represented as a combination of two parts (Guttman, 1955; Schönemann & Steiger, 1976). One part is, indeed, a function of model parameters and the model's observed variables. However, the other part is arbitrary, capable of taking on an infinity of different values even when the values of observed variables and model parameters are all fixed. The scope of variation permitted depends on the number of indicators in the model and the strength of their relationship with the common factor. In general, however, the correlation between two different realizations of the same common factor, with the same observed variables and same parameters, need not be high, nor even positive (Guttman, 1955).

The correlation between different realizations of the same indeterminate common factor, in turn, governs the relationship between that common factor and any variable not explicitly included in the factor model (Steiger, 1979). The correlation between common factor and external variable can only be defined in terms of a range. The greater the degree of indeterminacy, the wider the range of possible correlations between common factor and external variable. The conceptual variable, for which the common factor proxies, is one such external variable. The conceptual variable is clearly not a part of the factor model itself—if it were, then there would be no need for a proxy. Thus, the extent to which a researcher can establish the correlation between common factor proxy and conceptual variable is a function of the common factor's indeterminacy. The researcher cannot claim evidence for a perfect correlation, implying equality between proxy and conceptual variable, unless indeterminacy is 0. Zero indeterminacy can only be approached when a common factor is associated with a very large number of strong indicators, something that is never observed in practice.

If the common factor and the conceptual variable are *not* identical, then the observed variable residuals are not "measurement errors," because it is the conceptual variable that is being measured, not the common factor proxy. A composite proxy will also fail to be identical to the conceptual variable it represents, because, at a minimum, the composite will be tainted by random variance. Of course, any kind of proxy may be affected by systematic error, as well. Whether a particular proxy is affected by error to a greater or a lesser degree must be an empirical question, but there seems no basis for arguing that factor proxies will have any overall advantage over composite proxies. This is why a concern about "measurement error" does not amount to a valid argument for favoring a factor-based approach over a composite-based approach like PLS path modeling.

5. Conclusion

As a family of statistical methods, structural equation modeling is still young. The factor-based approach to SEM spread rapidly across the social sciences (Bentler, 1986), while PLS path modeling emerged later and developed more slowly, perhaps due to the absence of strong software. Regardless, much of the "received wisdom" on SEM has a limited evidentiary base for support. More than that, the emergence of SEM invited researchers to think in new ways, at new levels of abstraction. Analogies and heuristics are powerful tools in helping people to come to grips with things that are new and difficult to understand. While useful, however, heuristics can also be misleading. The more fundamental technology of null hypothesis significance testing is itself widely misunderstood, with textbooks enshrining mistakes as basic principles and infecting whole generations of researchers with falsehoods and confusion (Gigerenzer, 2004; Ziliak & McCloskey, 2008). Regarding factor analysis, Stewart (1981, p. 51) noted a similar state: "So widespread are current misconceptions about factor analysis in the marketing community that even its defenders and some prominent reviewers perpetuate misinformation." The same might be said of both factor-based and composite-based approaches to SEM today. Saunders and Bezzina's (2015) "methodological tribalism" appears to separate quantitative methods communities just as much as it separates quantitative from qualitative.

It should be clear to European management researchers that PLS path modeling is not a panacea for flaws in research design or execution. It does not multiply a small sample size into a large one. It does not transform a poorly conceived approach into a piercing, insightful analysis. At the same time, PLS path modeling is not a flawed analytical method. It may be misunderstood, but probably is no more so than the factor-based approach to SEM, or any other sophisticated data analysis technique.

So, if PLS path modeling is a valid tool for structural equation modeling, when should researchers choose this composite-based approach, and when should researchers choose the factor-based approach? Answers to this question in the literature tend to retreat to the same false beliefs and invalid arguments rejected here. There may, indeed, be some set of specific circumstances where one approach or the other has a predictable advantage, but that will require future research. Comparisons of different approaches should be based not on the particulars of the statistical models, which are different, but on their common outcome, proxies for conceptual variables. When researchers have the ability to compare different empirical representations with the conceptual variables being represented, then they will be able to determine whether one set of representations is better than another.

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