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ISSUES AND OPINIONS

COMPARING PLS TO REGRESSION AND LISREL: A RESPONSE TO MARCOULIDES, CHIN, AND SAUNDERS

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Appendix A

Comparison Across Approaches: Our Detailed Response to Specific Issues Raised by Marcoulides, Chin, and Saunders

In the "Comparison Across Methods" section of their Foreword, Marcoulides, Chin, and Saunders (2009, pp. 172-174) begin by seemingly acknowledging that for reflective models, PLS estimates are biased because they don't take into account measurement error.

> We note that in cases where the model errors are not explicitly taken into account for the estimation of endogenous latent variables, a new approach proposed by Vittadini et al. (2007) would need to be used to appropriately determine the PLS model estimates. This is because in PLS the reflective scheme assumed for the endogenous latent variables is inverted (i.e., the model errors are not taken into account). Savalei and Bentler (2007) have also indicated that with this type of model, PLS estimates (for example, those obtained by regression on factor scores) are generally biased as estimators of regression among latent variables (p. 172).

This is followed immediately by a discussion of issues that presumably could jeopardize a legitimate comparison between PLS and other statistical techniques. That discussion is framed, in effect, by the following "bookends."

> Opening Bookend:
>
> > Based on the above, it is quite clear that a variety of issues must be kept in mind when attempting these types of comparisons between approaches (p. 172).
>
> Closing Bookend:
>
> > In summary, it should be clear to the IS research community that comparison of PLS to other methods cannot and should not be applied indiscriminately. Ignoring any of the above issues could lead to incorrect conclusions or lead to overstating the importance of outcomes observed in a study (p. 173).

In between these bookends, we were able to identify five different issues that MCS raised as potential problems in comparisons. Rather than deal with them in the order in which they appear in the Foreword, we will start with the issue that is most clearly identified with the rejection decision for comparison papers: the claim that studies comparing PLS and CB-SEM or regression need to be "correctly parameterized." We will then deal with the other four issues in the order they are presented in the Foreword.

Issue 1: Incorrect Parameterization

The clearest charge in the Foreword against the rejected comparison articles is that those authors used incorrect parameterization when doing the comparison analysis. In discussing this issue, MCS supported their claims primarily by citing Dijkstra (1983).

> The instances for which researchers have reported the two modeling approaches as supposedly showing divergence of results generally have more to do with an incorrect comparison of selected mathematical functions and/or model parameterizations (Dijkstra 1983; Marcoulides 2003) (p. 173).
>
> Any observed differences are merely a function of the differentially parameterized models being analyzed. We note that the original term used by Dijkstra (1983, p. 71) was *correct parameterization* (p. 173).

These statements called into question the legitimacy of the rejected papers' comparisons between PLS and other statistical techniques, but nowhere in the Foreword is it made clear what correct parameterization would require. We believed that it was critical to learn what Dijkstra meant by the term correct parameterization and how that might apply to the validity of this line of research.

After careful review of Dijkstra, we realized that he uses the term correct parameterization in a completely different sense than we might have assumed, and quite differently from the sense we have employed (in the body of this paper) when we discussed "appropriate comparison." For Dijkstra, correct parameterization is a **characteristic of a statistical technique** (e.g., PLS or CB-SEM or regression). It is **not** a characterization of whether a statistical technique is correctly used by a researcher. Correct parameterization in Dijkstra's sense means that a statistical technique is "Fisher consistent."[1] That is, if data from the entire population (rather than a subset sample) is used with a statistical technique, that statistical technique will arrive at the true value of the parameters.

As an example, consider obtaining responses from a sample of 200 IT users to assess the strength of the relationships between three constructs: perceived usefulness, perceived ease of use, and intent to utilize a particular computer system. Assume there are three questions designed to measure each construct, with each of those questions having a certain measurement error. It is well recognized that any statistical technique, because of measurement error and a limited sample size, will not arrive at exactly the true value of the paths (relationships) for the population as a whole. This is true of PLS, CB-SEM techniques such as LISREL, or regression.

Now consider that the entire world population of potential or actual users of that system (perhaps several billion people) is included in the sample. If a statistical technique is correctly parameterized in Dijksra's sense (i.e., it has Fisher consistency), that technique would arrive at the correct value for the paths when the data includes the entire population. On the other hand, a statistical technique that is not correctly parameterized will not arrive at the correct values for the paths, even though the entire population is used.

Dijkstra shows that CB-SEM[2] is correctly parameterized (pp. 72-74.) He also shows (pp. 76-88) that PLS is not correctly parameterized, regardless of whether mode A, mode B or mode C is used.[3] It is not clear how important this difference *in the limit* is; we almost never have the full population as a sample. However, Dijksta concludes,

> Clearly (15) says that PLS will tend to *underestimate* the correlations between latent variables; the discrepancy between true values and probability limits depends in a simple way on the quality of the proxies as measured (p. 81).

[1]Dijkstra defines Fisher consistency for readers on page 70 of his article. For more accessible definitions of Fisher consistency, see Cox and Hinkley (1974) and Jureckova and Picek (2006).

[2]Dijkstra uses LISREL as an example of a covariance-based SEM (CB-SEM) model. In other work he makes quite clear that comments relating to LISREL are also true of other CB-SEM models. Schneeweiss (1993) also uses the term LISREL, while McDonald (1996) tends to use the term SEM.

[3]Mode A is the use of all reflective constructs. Mode B is the use of all formative constructs. Mode C is the use of a combination of reflective and formative constructs.

It is not a new idea to say that PLS underestimates path values when there is measurement error.[4] MCS seem to be saying exactly that in the first quote in this Appendix. However, two aspects of this are important in the context of the question of whether PLS and other statistical techniques can be compared. The first is that researchers might be very interested in *how much* PLS underestimates path values in comparison to CB-SEM techniques such as LISREL under typical research conditions. This is certainly a relevant and empirically answerable question, assuming the two techniques can be legitimately compared.

The second aspect that is important is that MCS say statistical techniques can only be compared if they are correctly parameterized (citing Dijkstra). The logical conclusion of MCS's assertion and Dijkstra's definition of parameterization, however, would be that PLS can *never* legitimately be compared with LISREL or other CB-SEM techniques.

Would Dijksta agree with this conclusion? In personal correspondence with us, Dijkstra expressed his belief that not only **can** results from PLS analysis be compared to those from other statistical techniques, but in the context of estimating the parameters of a latent variable model, they should be.

> This does not protect PLS from scrutiny of course: IF it purports to estimate the parameters of a latent variable model, its estimation accuracy should be judged in comparison with contenders, its handicap (the incorrect parameterization) being its own doing (Dijkstra 2010a).[5]

One might read the "IF" in Dijkstra's above quote as a suggestion that behavioral researchers using PLS might not always be interested in estimating the parameters of a "latent variable" model. We don't believe he intended it that way, but if we entertain that possibility, the real question raised by Dijkstra's "IF" is, what type of underlying reality are researchers studying? Is it best approximated by a latent variable model, or by a composite variable model? The key difference is in how measurement error is assumed to operate as we move from the underlying reality to our measures of it, and our estimates of the values for constructs of interest.

A latent variable model assumes that all measures contain measurement error, so the value of a construct of interest must be inferred from imperfect measures. A composite model assumes that measures contain no measurement error, and that the value of a construct of interest is an exact weighted average of its measures, plus possibly a single error term to capture the impact of unmeasured components of the construct of interest.

If the underlying reality behavioral researchers are interested in studying is truly best approximated by a composite model with no indicator measurement error, then CB-SEM, regression, and PLS are all Fisher consistent and none has a handicap. If it is best approximated by a latent variable model that includes measurement error, then regression and PLS have the handicap Dijkstra referred to, and CB-SEM does not. We will go out on a limb and suggest that it is untenable to assert that there is no error in the measures used in most behavioral research. How much of a handicap regression and PLS therefore face, and under what circumstances, is a question that is empirically answerable with Monte Carlo simulation.

With this understanding of what Dijkstra means by correct parameterization, the assertion that any differences between PLS and SEM are due to incorrect parameterization may be literally true, but not in the sense the Foreword seems to imply. It is not because the comparisons were done incorrectly. Rather it is because PLS has a handicap when there is measurement error: it is intrinsically incorrectly parameterized.

Differences between PLS and correctly parameterized estimation techniques (such as CB-SEM) are to be expected due to that underlying fact. If one uses Dijkstra's definition of incorrectly parameterized, the logical implication of MCS's assertion (that comparisons must be equivalently

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[4]See Chin et al. (2003), Supplemental Material (//www.informs.org/Pubs/Supplements/ISR/1526-5536-2003-02-SupplA.pdf, p. 13):

> Nevertheless, being a limited information method, PLS parameter estimates are less than optimal regarding bias and consistency. The estimates will be asymptotically correct under the joint conditions of consistency (large sample size) and consistency at large (the number of indicators per latent variable becomes large). Otherwise, estimates on paths from construct to loadings tend to be overestimated and structural paths among constructs underestimated.

See also Chin (1998, p. 329): "This bias tends to manifest itself in higher estimates for component loadings (outer model relations) and lower estimates at the structural level (inner model relations)."

[5]After reading a draft of this Issues and Opinions paper, Dijkstra commented in a follow on e-mail (2010b), "To me the central quote from my [prior] email to you is" and he restated verbatim the sentence of the quoted material from above.

parameterized) would be that comparisons between PLS and CB-SEM can never be legitimate, since PLS is inherently incorrectly parameterized, and CB-SEM is correctly parameterized.

Since we doubt MCS are asserting the above, we admit to some confusion about what they mean by their term correctly parameterized. As noted in the body of our paper, this leads us to the supposition that MCS may be using the term in the way we defined appropriate parameterization.

As we have suggested previously, appropriate parameterization depends importantly on the question being studied in a given comparison. Therefore, categorical assertions of inappropriate parameterization should not be made without specific arguments about why the parameterization is inappropriate, and suggesting a more appropriate parameterization for the intended research question.

Selected passages of Dijkstra's article and our personal correspondence with him that lead us to these conclusions are included in Appendix B. We note that earlier in his 1983 article (p. 71, see Appendix A) Dijkstra stated that, in general, at least one of three conditions for ideal estimation (correct parameterization, correct distribution, and no data mining—i.e., capitalization on chance) will be violated at least to some extent by any estimation technique. We point this out to clarify that Dijksra's assertion that PLS is incorrectly parameterized is not a damning indictment of PLS, merely his conclusions about this particular characteristic of PLS. In other words, Dijkstra does not believe that different techniques must exhibit a correct parameterization (by his definition) before they can be legitimately compared.

Issue 2: Comparisons Are Trivial

A second issue raised in the Foreword was the apparent assertion that any comparison of the performance of regression and PLS or CB-SEM techniques based on a single regression equation is trivial, in that the techniques will produce the same results:

> any comparison of the performance of multiple regression relative to either PLS or SEM is trivial. Specifically, it is well known that an analysis of the same data and model based on a single regression equation using multiple regression, PLS, or SEM approaches will always result in identical estimates (irrespective of the estimation method used, be it maximum likelihood, unweighted least squares, generalized least squares, etc.) (p. 172).

It certainly is true that if there are single indicators for each construct, or if identical weightings are used to create composite values for each construct, the different statistical techniques would all give the same results. In other words, when measurement error is non-existent or no information about it is available, the three techniques will give identical path estimates.[6]

But by far the more common situation in behavioral research is where researchers are dealing with measurement error and multiple indicators for each construct. For all three techniques (regression, CB-SEM, and PLS), the researcher specifies the hypothesized relationships among the constructs (the structural model). For regression, the researcher typically computes a composite score (e.g., using a simple average) for each construct. For PLS and CB-SEM, the researcher specifies items intended to measure each construct (the measurement model), and the software package implementing the statistical technique will estimate measurement indicator loadings.

This multiple indicator situation is the context we are interested in, rather than the situation of a single indicator for each construct. In the former case, the three statistical techniques will not produce identical results[7] and the comparison is not trivial. We note that the three authors most prominently cited by MCS in connection to the comparison issue (Dijkstra, McDonald, and Schneeweiss) all explicitly discuss differences between PLS and CB-SEM results when legitimate comparisons are made in this context.

The quote from the Foreword above (that correct comparisons result in identical estimates) is true only in the very unusual circumstance that there is no measurement error or only single indicators for each construct. Therefore, we don't see it as having any direct bearing on the validity of comparisons across different statistical techniques that are being used to analyze constructs measured with error, using multiple indicators for each construct.

[6]We note that the identical results assertion is not true for the t-statistics of path estimates, as they are generally determined in at least two quite different ways (normal theory testing and bootstrapping).

[7]The reader can verify the differences with his or her own data and analysis if desired.

Issue 3: Distinguish Between Latent Constructs and Composite Variables

MCS state that they concur with McDonald (1996) that variables in PLS are not true latent variables but are "composites" (pp. 172-173). That is, PLS variables are estimated by exact linear combinations of their indicators. Therefore, different terminology should be used to distinguish between the composite variables used in PLS on the one hand, and the latent variables used in LISREL and other CB-SEM techniques on the other hand. We agree with both MCS and McDonald on this point.

The reason McDonald insists on the distinction is that he believes that because any representation of data by a weighted composite (as in PLS) omits an error term, estimates of path coefficients cannot be consistent (for a finite number of observed variables), so weighted composites can give only crude approximations to the generating model (see Appendix C and McDonald 2010b). Although in a different form, this is roughly equivalent to Dijkstra's assertion that PLS is inherently incorrectly parameterized.

McDonald is quite clear in his assertion that this will be a disadvantage for PLS. More specifically, as his closing remarks, McDonald asserts the following:

> Let us, then, as the final topic for discussion, consider the dilemma we face in the choice between SEM /path analysis with latent variables (in the sense of common factors) and path analysis with composites. (It is strongly recommended that we avoid a common but confusing convention in the rather informal literature on PLS of referring both to common factors and to composites as latent variables.) Indeed, the choice for the researcher is at least threefold, between (a) latent variables [such as LISREL], (b) composites with prior weights (equal weights and so on) [such as regression with equally weighted indicators], and (c) composites with data-dependent, possibly optimizing weights [such as PLS]....An obvious disadvantage of options (b) and (c), illustrated by the chain example, is that they do not provide a clear test of the path hypothesis, the effect of "measurement error" being to introduce an apparent departure from the known causal model. This can be expected to occur quite generally.
>
> The advantage claimed for option (a), indeed its central motivation (which is well illustrated by the causal chain example) is that if the attributes are measured imperfectly by their indicators, the relationships between composites (whether by (b) or (c)) will be attenuated by the "errors of measurement," and the use of latent variables is in effect a correction for attenuation (pp. 266-267).

In other words, McDonald believes that we should expect differences between results obtained with PLS (or other statistical techniques using composites such as regression) and results obtained using CB-SEM techniques, when the techniques are correctly compared. Further, the CB-SEM techniques should result in more accurate path estimates, since they provide "in effect a correction for attenuation" due to errors of measurement, while PLS does not. Excerpts of our correspondence with McDonald addressing this and related issues are included in Appendix C.

Once again, we find nothing in the comments from the Foreword on the importance of distinguishing between latent and composite variables that would lead one to doubt the legitimacy of a particular comparison between PLS and any other statistical technique. While the quote above is factually true, it does not seem to be relevant to the question of whether a given comparison between statistical techniques is legitimate.

Issue 4: The Ratio of the Largest Eigenvalue to the Sum of the Squared Loadings

A fourth issue raised in the Foreword is the suggestion that PLS will provide similar results to CB-SEM techniques under certain conditions:

> Mathematically, the key to governing the closeness of PLS to SEM latent variables for a particular block is the ratio of the largest eigenvalue of the error covariance matrix to the sum of the squared loadings. In situations where this ratio, or by the model specified, is made to be small (e.g., path coefficients and loadings,) estimates obtained from PLS and SEM will be very close to each other or approximately equal (Schneewieiss 1993) (p. 173).

Schneewiess (1993) says that one would expect differences between PLS and LISREL results in general (because they are attempting to do two different things). He later says that if the "ratio" is small, it might be advantageous to use PLS rather than LISREL, because under these circumstances the results will be about the same, and PLS is computationally less demanding (pp. 315-316).

We communicated with Professor Schneeweiss to verify this interpretation of his work; excerpts of our correspondence and our interpretation are provided in Appendix D. When we asked whether PLS and LISREL could be compared using Monte Carlo simulation with known true path values, his response was the following:

> Although you estimate different things, you can of course compare them as far as comparisons make sense. It should however always be clear that your estimates refer to different, though related, objects (Schneeweiss 2010b).

It is clear from the Schneeeweiss article, the above quote, and the material in Appendix D that the "ratio" referred to in MCS is not viewed by Schneeweiss as indicative of whether a comparison between PLS and LISREL results is valid. The above quote does refer to two ideas that are clearly important to Schneeweiss: "as far as comparisons make sense" and "[it should be] clear that your estimates refer to different, though related, objects." We have indirectly discussed these ideas as they relate to comparisons between statistical techniques in a Monte Carlo simulation, and we believe that comparisons of the same performance metrics from different statistical techniques do "make sense," even though path values determined by PLS and CB-SEM are "different, though related, objects."

We conclude that MCS's comments about Schneeweiss's "ratio" are factually correct. But we do not see any reason why the value of that ratio is relevant to the question of whether a comparison between PLS and CB-SEM is legitimate.

Issue 5: The Number of Indicators

MCS again refer to McDonald's 1996 paper to support their claim that

> With a sufficiently large number of indicators in an examined model, however, the choice of composite weights actually ceases to have an influence on the parameters of the path model. Research has shown that an actual value with regard to the effect of the number of indicators on the approximation can be calculated using a so-called biasing factor formula (Lohmoller 1989; McDonald 1996). Ultimately it would seem that without adding an inordinate number of indicators to make the weighting issue irrelevant, it boils down to what a researcher is interested in examining (pp. 173-174).

The reason McDonald insists on not confusing composite variables in PLS with latent variables in LISREL is that, as is made clear in his quote under Issue 3 above, estimates from any technique using a weighting scheme for composite variables "will be attenuated by 'errors of measurement'" (McDonald 1996, p. 267). McDonald does suggest that with a sufficient number of indicators (which we note will reduce measurement error), different weighting schemes for composite variables will converge:

> It is reasonable to regard a path model with weighted composites as approximating the path model with latent variables. Theory, and the numerical evidence with respect to the biasing factor, suggests that increasing the number of indicators will, certainly, improve the approximation, and the rate of improvement will be affected very little by the choice of weighting schemes, unless a scheme is deliberately chosen to slow that rate. We note, however, that the number of indicators needed to secure negligible disagreement seems considerably greater than is common in applications of path analysis in the social sciences (p. 264).

What McDonald is saying in the quote immediately above is that, with a sufficient numbers of indicators, the results from techniques with different weighting schemes for **composite variables** will converge with the results from a CB-SEM technique. However, McDonald expresses concern that the number of indicators needed for that convergence may exceed what is typically available in behavioral research. In the specific example McDonald analyzes in his paper, 12 indicators for each construct would be necessary to get agreement within about 10 percent. Twelve indicators per construct is probably out of the realm of possibility for most behavioral researchers, and certainly more than are used in most published IS studies.

To make sure that we were correctly interpreting Professor McDonald's work, we sent him our interpretation of his paper and a copy of the MCS Foreword. His response is given below. Appendix C provides some of McDonald's additional comments in personal e-mail correspondence with us.

> I have skimmed the material you sent. It is easy for me to see that you have correctly read my 1996 paper on every point. So that, at least, is fine! I found your paper clear and easy to read (McDonald 2010a).

Relative to MCS's above quoted comments about the number of indicators, we argue that the issue of the closeness of the convergence between results of different statistical techniques is not relevant to the *legitimacy* of the comparison, as long as both analyses are utilizing the same data and the same number of indicators.

Appendix B

Evidence on "Correct Parameterization" from Dijkstra (1983), and Personal Correspondence with Professor Dijkstra (2010)

To understand the crux of the argument made in the MCS Foreword, it is necessary to understand what Dijkstra (1983) meant by "correct parameterization" and how that would relate to the comparisons of PLS and CB-SEM (and, by extension, regression). Because this is a critical point, please bear with us as we explain it.

Dijksta's article focuses on the estimation properties of maximum likelihood estimation as embodied by LISREL, and as compared with the estimation properties of PLS. As his abstract states,

> The paper discusses some general aspects of two estimation methods, which are designed for analysis of interrelationships between indirectly and directly observable variables. The paper's main object is to summarize in broad terms what appears to be known about the asymptotic properties of maximum likelihood and partial least squares. The author would be pleased if, as a side-effect, interest is stirred up in the analysis of estimators under non-textbook assumptions (p. 67).

After discussing estimation in general for several pages, Dijkstra states three conditions critical for proper use and interpretation of an estimation technique:

> Now for some fitting functions *F* there exist computer programs which produce, apart from an estimate θ^(s), an estimate of Ω. A little reflection upon the way in which this estimate as a rule is obtained (see also section 4.4) will make clear that in general its proper use and interpretation hinges upon the joint validity of the following three conditions:
> C_1: The "distance" between s and $\sigma(\Theta)$ may be ignored, or more strongly, plim *s* [is an element of] $\sigma(\Theta)$.
> C_2: The value of a consistent estimator of V is available.
> C_3: The choice of Θ, σ, or s did not depend on d (p. 71).

Dijkstra goes on to help us understand these three conditions by saying the following (we note that he clearly states that in general it is expected that at least one of the conditions will be violated):

> The three conditions can perhaps be paraphrased as: "correct parameterization," "correct distribution," and "no data mining" (or no capitalization on chance). It does not seem to be much of an exaggeration if we state that in practice at least one of the conditions is violated. In the next sections we will try to summarize what is known about the behavior of LISREL and PLS estimators for such "non-ideal" cases (p. 171).

Therefore, we see that Dijksta, by the phrase "correct parameterization," means that condition C_1 is met. Condition C_1 means that in the limit, the estimated parameters are not far from (or are identical to) the true situation. (This can be deciphered from the first three paragraphs in Dijkstra's section 2, page 68.) As Dijkstra says on pages 69 and 70, this is equivalent to the property of Fisher consistency,[8] or the property that if the estimator were calculated using the entire population rather than a sample, the true value of the estimated parameter would be obtained.

Understanding what Dijkstra means by "correct parameterization," we can now look at what he concludes about LISREL and PLS in this regard. Dijkstra notes on page 74 that **"The LISREL criterion is Fisher-consistent."** Later in evaluating PLS, Dijkstra notes, referring to Wold's rules about which of the three modes (A, B, or C) to choose for PLS:

> These rules are hard to evaluate because the requisite knowledge of small-sample distributions is lacking. However, for all modes one can prove that estimates of the parameters, obtained by standard methods using proxies for latent variables, induce in general an estimate of Σ which is not close to its true value, even when S is (we will elaborate on this in the subsections below). Or in other words, PLS is not Fisher-consistent (p. 80).

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[8]For definitions of Fisher consistency, see Cox and Hinkley (1974) or Jureckova and Picek (2006).

We now note that with the exception of distinguishing between PLS's mode A, B, and C, nowhere in the article is Dijksta concerned with differences in the parameterization of PLS, in the sense of differences in how users set up the analysis. Instead, the entire article is interested in differences between CB-SEM in general and PLS in general or PLS with the three different modes. Dijkstra concludes that while CB-SEM is "correctly parameterized," PLS is not, regardless of the mode used. We remind readers that Dijkstra has already stated that in general at least one condition will be violated at least to some extent. So this is not a damning indictment of PLS, merely his conclusions about this particular condition.

However, we also contacted Professor Dijkstra by e-mail (Dijkstra 2010a) to ask him about this, and a portion of his response is included below, verbatim:

> A LISREL type model[9] says essentially that the true value of the covariance matrix is located on a smooth manifold, the graph of a smooth function of a parameter vector from a suitable space. The parameter whose image coincides with the true covariance matrix is the true parameter. The sample covariance matrix will not be on this manifold, but ML or GLS project it onto it, with the inverse of this point as the parameter estimate. Fisher argued that a natural requirement for estimation or projection methods is that they produce the true parameter when applied to the true covariance matrix.
>
> Since PLS replaces latent variables by proxies who can "never" represent them exactly, the proxies are not related to each other as the latent variables are, as in a simultaneous system of equations for instance. Whereas the true reduced form regression matrix of the latent variables is located on a smooth manifold as well, the true regression matrix of the proxies will not be on it. So the parameterization for the proxies is incorrect.
>
> This does not protect PLS from scrutiny of course: IF it purports to estimate the parameters of a latent variable model, its estimation accuracy should be judged in comparison with contenders, its handicap (the incorrect parameterization) being its own doing.

In summary, PLS will never be correctly parameterized (in the sense used by Dijkstra), regardless of which mode (A, B, or C) is used. CB-SEM is correctly parameterized, and hence we should anticipate differences in results from the two. Therefore, we can (and we should) continue to test the efficacy of PLS relative to other statistical techniques.

Appendix C

Correspondence with Professor McDonald

We sent Professor McDonald our interpretation of his work with an early draft of this Issues and Opinions paper, and exchanged several e-mails with him. Two excepts from this correspondence (with some personal comments removed from the first at his request) are presented here.

> I have skimmed the material you sent. It is easy for me to see that you have correctly read my 1996 paper on every point. So that, at least, is fine! I found your paper clear and easy to read.
>
> …
>
> It is a source of regret to me that my 1996 paper does not seem to have been read as I intended it. My position is that PLS is a collection of algorithms that were casually and very foolishly conceived, and cannot be recommended, although I was able to prove they had some optimal properties. I hoped that people would see that the alternative method I recommended-- a "SEM model" without unique terms and with LS/GLS estimation would be superior, and that this was only a crude (usually bad) approximation to the desired model (McDonald 2010a).

[9] Professor Dijkstra uses different terminology than we have in this paper. Generally, when he uses "model," he is referring to what we have called a "technique" (i.e., CB-SEM, regression, or PLS).

> I believe that because any representation of data by a weighted composite of them omits an error term, estimates of path coefficients cannot be consistent (for a finite number of observed variables), so it can give only crude approximations to the generating model (McDonald 2010b).

Obviously, Professor McDonald is not a big fan of PLS. From our perspective, his last statement that we list is key: PLS can not explicitly incorporate error terms, so its weighted composite scores for underlying constructs will always represent approximations of the truth. How good these approximations are should be an empirical question which we can test.

Appendix D

Correspondence with Professor Schneeweiss

We sent Professor Schneeweiss the following quote from MCS.

> Mathematically, the key to governing the closeness of PLS to SEM latent variables for a particular block is the ratio of the largest eigenvalue of the error covariance matrix to the sum of the squared loadings. In situations where this ratio, or by the model specified, is made to be small (e.g., path coefficients and loadings,) estimates obtained from PLS and SEM will be very close to each other or approximately equal (Schneeweiss 1993) (p. 173).

The question we posed to Professor Schneeweiss was:

> *Marcoulides et al. seem to imply that only when the above condition is met can PLS and LISREL be legitimately compared. However, we believe that you decidedly do not suggest that* (Authors 2010a).

His response was the following:

> I do not think I was misinterpreted by Marcoulides et al. It all depends on what you mean by "comparing."
>
> PLS and SEM (or LISREL in my terminology) latent variables are different entities. But since they are random variables defined on the same sample space they can be compared in the sense of studying their closeness to each other measured, e.g., in terms of their correlation. On the other hand, if methods to compute or to estimate them (or to estimate their loadings) give rise to different results, this is simply due to the fact that they are different anyway. In that sense, it does not make much sense to compare results that refer to different entities (Schneeweiss 2010a).

This response made us worry that perhaps we had misinterpreted his paper. By e-mail we acquainted Professor Schneewiess with our Monte Carlo simulation experiments and asked him the following:

> *In your opinion, would it be appropriate to compare the methods on how close their estimates came to the known, true underlying values [in a Monte Carlo simulation]? It is true that any differences would be a function of the way the estimates were made, but in this case there is an underlying true value. If there were systematic differences, wouldn't it tell us something about the relative efficacy of the techniques for determining path values and variance explained?* (Authors 2010b).

His response was the following:

> I suppose you are right. Although you estimate different things, you can of course compare them as far as comparisons make sense. It should however always be clear that your estimates refer to different, though related, objects (Schneeweiss 2010b).

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