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MIS Quarterly

RESEARCH NOTE

THE CRITICAL IMPORTANCE OF CONSTRUCT MEASUREMENT SPECIFICATION: A RESPONSE TO AGUIRRE-URRETA AND MARAKAS[1]

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Aguirre-Urreta and Marakas (A&M) suggest in their simulation "Revisiting Bias Due to Construct Misspecification: Different Results from Considering Coefficients in Standardized Form," that, like Jarvis et al. (2003), MacKenzie et al. (2005), and Petter et al. (2007) before them, bias does occur when formative constructs are misspecified as reflective. But A&M argue that the level of bias in prior simulation studies has been exaggerated. They parameterize their simulation models using standardized coefficients in contrast to Jarvis et al., MacKenzie et al., and Petter et al., who parameterize their simulation models using unstandardized coefficients. Thus, across these four simulation studies, biases in parameter estimates are likely to result in misspecified measurement models (i.e., using either unstandardized or standardized coefficients); yet, the biases are greater in magnitude when unstandardized coefficients are used to parameterize the misspecified model. We believe that regardless of the extent of the bias, it is critically important for researchers to achieve correspondence between the measurement specification and the conceptual meaning of the construct so as to not alter the theoretical meaning of the construct at the operational layer of the model. Such alignment between theory and measurement will safeguard against threats to construct and statistical conclusion validity.

Keywords: Formative measurement, construct misspecification, standardized coefficients, unstandardized coefficients, simulation, construct validity, statistical conclusion validity

Introduction

In their research note in this issue, "Revisiting Bias Due to Construct Misspecification: Different Results from Considering Coefficients in Standardized Form," Aguirre-Urreta and Marakas (hereafter A&M) suggest that past simulation research overstates the biases in estimated parameters of structural paths and risks in inferences arising from misspecifying formative–reflective relationships between measures and constructs. A&M discuss the results of simulation studies by Jarvis et al. (2003), MacKenzie et al. (2005), and Petter et al. (2007) and provide evidence that, had standardized parameters been used instead of unstandardized parameters in these simulations, the biases in the estimated structural parameters would have been much smaller.

We believe that A&M misses the critical point of all three simulation studies and much other recent literature on the importance of achieving correspondence between a con-

[1]Joseph Valacich was the accepting senior editor for this paper.

struct's meaning and its measurement model. The implications of A&M's study could lead to a serious misdirection in the methodological literature. In our earlier paper, "Specifying Formative Constructs in Information Systems Research" (Petter et al. 2007), our concern was that researchers should not alter the meaning of a construct at the operational level by misspecifying the measurement model. This creates a disconnect between the theoretical level and the operational level of a construct, which, in turn, compromises both construct and statistical conclusion validity. We emphasized that the meaning of the construct changes at the operational level based on the measurement specification being reflective or formative. Reflective specification orients the meaning of the construct to capture the common variance among the measurement indicators, and formative specification orients the meaning of the construct to capture the unique variance of the measurement indicators.

Our key message, though, was that IS researchers should carefully evaluate the specifications of measurement models so they maintain correspondence between the theoretical and operational models and consequently safeguard against validity threats. What is troubling about A&M's paper is that this message could be lost if the impression that comes through from A&M is that model misspecification should not be of concern. It could distract the field from the importance of achieving correspondence between a construct's theoretical meaning and the type of measurement model that is specified for it. While A&M do state that "research on the proper specification of constructs is still of critical importance [in uncovering] the true relationships between variables and their structure" (p. 2), they conclude, based on their simulations results, that "the consequences of misspecification seem to be ***much less dire*** than previously thought" (p. 2, emphasis added). Regardless of the amount of bias present due to construct misspecification, we maintain that it is critical for researchers to still focus on whether a construct's measures should be specified as formative or reflective. Therefore, the main point of our response to A&M is on *why* researchers should be concerned about construct specification, both theoretically and empirically, irrespective of differences in the magnitude of statistical bias due to alternate parameterizations of the coefficients in the statistical models.

Therefore, we structure this response as follows. First, we identify several areas in which we specifically disagree with A&M and express our own views on these issues, such as the use of unstandardized estimates and other choices in our simulations. Next, we state reasons why researchers should be concerned about construct misspecification because of the potential impact on theory. Then, we discuss impacts of construct misspecification on empirical analysis and results and specifically address some of the concerns expressed by A&M with past work on simulating misspecified constructs. Finally, we conclude with key points that we hope researchers take away from this discussion about the serious impacts of construct misspecification.

Differences in Parametizing Simulation Models

In our paper (Petter et al. 2007), our intent was to inform readers of the importance of considering the relationship between constructs and measures and achieving correspondence in a construct's meaning across the theoretical and operational layers of a model. The simulations we performed provided evidence of the potential for theoretical and empirical misinterpretation when constructs are misspecified. In A&M's paper, the authors specifically focus on the approach used in our (and others') simulations and suggest that the bias that occurs when constructs are misspecified has been overstated. The crux of A&M's argument against prior studies using simulation data to examine the ramifications of misspecifying formative constructs is that unstandardized coefficients inflate the bias of misspecified constructs.

We disagree with A&M's comparison of standardized and unstandardized estimates in simulation results and, by implication, their advocacy for the use of standardized estimates. We believe that their comparisons fail to consider important distinctions between standardized and unstandardized coefficients and promotes the misconception that standardized estimates are *always* superior in simulations or, more generally, in statistical tests of research models. There are reasons for using either standardized or unstandardized estimates, and we believe the use of unstandardized measurement for our simulation was the appropriate choice. A&M also discuss constraining the variance of a formatively measured construct, which may or may not be the best choice for a researcher depending on their research question. We address this issue as well as their criticism of our use of 0.03 as a value for a nonsignificant structural path in our simulation. We conclude this section with a strong response to A&M's statement about their concern that our study labels IS research as "largely" invalid.

Standardized Coefficients and Unstandardized Coefficients in Simulations

When evaluating standardized and unstandardized coefficients, it becomes important for the researcher to fully consider which values are most relevant to their needs. When comparing A&M's unstandardized results to the results obtained in Petter et al., MacKenzie et al., and Jarvis et al., it is clear that all obtained similar findings and degrees of bias. (See Figures 2, 3, and Appendix D in A&M.) The discrepancy about magnitude of bias due to misspecified constructs arises when comparing the unstandardized coefficients to the bias of the standardized coefficients.

When using standardized coefficients, additional information is introduced into parameter coefficients. The most obvious difference between this approach and the unstandardized approach is that in the former case the variance of the construct is now embedded in the coefficient. This information needs to be acknowledged and understood by both researcher and reader. After transformation, each variable may now have a different standard deviation, making it difficult to interpret statistical results because the relationship between variables is no longer patently straightforward. Standardized coefficients not only capture the relationship between the independent variable(s) (IV) and dependent variable (DV), but also the variance of the IV relative to the variance of the DV. When interpreting an unstandardized coefficient, therefore, one knows that a unit change in the IV has a given effect on the DV. However, with a standardized coefficient, a unit change in the IV is no longer associated with a one unit change in the DV but with a *one standard deviation* change in the DV. This changes one's interpretation of the data since we are now working with transformed coefficients and not coefficients based on the raw data.

In a simulation context, this issue may or may not be moot based on the choice of variances for the constructs. If the variances are the same for all of the constructs, then the interpretation of the constructs for a standardized value is rather straightforward as no interpretational confounds are introduced by scaling the magnitude of the effect by the *same* variance for the constructs. With empirical data, however, the variance of constructs cannot be assumed to be homogeneous and scaling the magnitude of the unstandardized effect by a common variance may not be possible. If the variances are heterogeneous, standardized estimates will have to be derived by scaling the magnitude of the unstandardized effect of each construct by its variance. Thus, not only do standardized and unstandardized coefficients have different meanings, but they also need to be computed and interpreted differently based on whether construct variances are homogeneous or heterogeneous.

A&M note that

> *the apparently large bias that results from the misspecification of the relationship between latent variables and indicators is more a function of comparing coefficients expressed in different metrics rather than the bias in the underlying effects themselves* (p. 8).

While many, including A&M, suggest that standardization allows for better interpretation of coefficients expressed in different metrics, others suggest this is not the case. If the researcher wishes to compare IVs to determine which one has the strongest effect on a DV, some argue that the comparison is no longer straightforward when one uses standardized coefficients because the standard deviation of a variable can vary from group to group (Richards 1982). Pedhazur and Schmelkin (1991) suggest that using standardized coefficients in this manner is "deceptively simple" and "evades rather than solves the problem" of interpreting the true relationship between the independent variable and dependent variable (p. 375).

Constraining Variance of Formatively Specified Constructs to 1.0

In developing their argument, A&M state that

> relationships in unstandardized form are expressed in a metric that is dependent on the particular estimated variances for the factors involved, whereas those expressed in standardized form have been rescaled to make the variances of all latent and manifest variables equal to one (p. 5).

Therefore, consistent with other researchers (Franke et al. 2008), A&M suggest that there is value in achieving identification for a formative construct by setting the construct variance to 1.0 (rather than constraining the weight for one of the formative measurement items to 1.0) (see A&M, pp. 3-4). When setting the construct's variance to 1.0 to achieve identification, the construct is now standardized. A&M state that

> researchers can opt to set the variance of a latent variable at any given number, again most commonly one, but any non-zero number would be acceptable. If the variance of both the exogenous and endogenous factors were fixed at one, the resulting regression coefficient would be in standardized form (p. 3).

Making this choice to set the variance of a construct to 1.0 to achieve identification is not a trivial decision. As Edwards (2011) notes, the variance of the formative construct is determined not just by the paths from the measures to the construct but also by (1) the variances and covariances of the measures and (2) the variance of the disturbance (error) term of the construct.[2] The decision to set the variance of a formative construct to 1.0, therefore, should be made by a researcher after evaluating the nature of the variances and covariances and the formative construct's indicators and variance of the construct's error term in a given research context.

A&M state that

> a lack of attention to the metric of latent variables is responsible for the posited bias [in prior simulations about misspecified constructs], and when considering the relationship in their standardized form neither their direction nor magnitude are biased to the degree previously discussed (p. 2).

But if researchers using SEM set the metric of the latent variable by fixing the first factor loading to 1.0, the metric of the latent variable inherits exactly the same unit change value (but not necessarily the same range) as this first indicator. Accordingly, going from 2 to 3 on the item scores and on the factor scores has the same meaning and is straightforward in its interpretation. This simplicity notwithstanding, it is true that there can be problems with metric scaling when a formative measurement weight is constrained to 1.0 rather when than the variance of the construct is constrained to 1.0 (Franke et al. 2008); thus the implications of this choice should be carefully considered by the researcher.

Diamantopolous (2011) recommends that researchers carefully consider the form of scaling for a formatively measured construct, and we reiterate this advice. To determine if there are scaling problems, the researcher could examine the differences in the measurement and structural model when using each of the formative indicators as the reference variable (Diamantopoulos 2011; Franke et al. 2008). Another alternative to the metric scaling problem is setting the formatively measured construct variance to 1.0. Yet another option is to constrain the relationship between the formatively measured construct and another latent construct in the model to 1.0 (Bollen and Davis 2009).[3]

What is of note here is that when measurement models are incorrectly specified in the four simulations by Jarvis et al., MacKenzie et al., Petter et al., and A&M, all of the constructs are specified as reflective. Therefore, the challenges with identification and metric scaling that exist for formatively measured constructs are not pertinent for these misspecified models because every construct is modeled as reflective. When constructs are reflective, researchers usually achieve identification by constraining one measurement item to 1.0 rather than constraining the variance of the construct. Therefore, the reason argued by A&M that formative constructs should be standardized to address identification concerns (pp. 3-4) is not relevant in this particular scenario because in the misspecified models, all constructs were modeled as reflective.

Nonsignificant Paths in Simulation Studies

A&M also expressed concerns about the use of 0.03 as a parameter estimate for the study of Type I error by Petter et al. In their research note, A&M state that this relationship should have been examined with a relationship of 0.0 between the two constructs; they go on to suggest that this is actually a test of Type II error rather than Type I error as described in Petter et al. We refer to Type I error in the evaluation of a hypothesized relationship (i.e., a relationship does exist between X and Y) as opposed to a Type I error in the evaluation of a null hypothesis (i.e., no relationship exists between X and Y). Our view corresponds to the form of specification of hypotheses that are commonly theorized and tested in IS research and the corresponding reporting of p-values on the risk of a false positive or Type I error.[4]

To be thorough, we performed the additional analysis recommended by A&M and did find that if one simulates absolutely no relationship between two constructs (i.e., parameter of 0.0), the estimates from the simulation for both properly specified and misspecified constructs are statistically non-

[2]When setting a formative construct's error term to 0, the implicit assumption is that the indicators explain all the variance in the construct. Depending on the construct, this assumption may or may not be valid. While this choice allows the researcher to obtain model identification, it does affect the researcher's interpretation of the construct and resulting structural model (Diamontopoulos and Winklhofer 2001; MacCallum and Browne 1993). "In cases where not all possible causes are explicitly incorporated as indicators, the error term must be included as a model parameter and estimated along with other parameters in the model" (Diamantopoulos 2006, p. 11). If the error of the construct is unknown, then a researcher should consider allowing the construct to accommodate any possible error that may exist because the construct is not thoroughly defined.

[3]Diamantopolous (2011) provides details on each of these approaches to scale the measurement of a formatively measured construct.

[4]IS researchers, like researchers in many other fields, are most frequently testing theoretically based relationships and, therefore, are seldom testing a null hypothesis. That is, in the vast majority of cases, we are testing a directional hypothesis of a posited effect. This is why avoiding Type I errors is so important. Our conceptualization of Type I and II errors follows IS practice.

significant with effectively no bias. Therefore, when there is absolutely and positively no relationship between two constructs (i.e., 0.0 parameter), construct misspecification does not create the upward bias for the parameter estimates that could lead to Type I error using the A&M view and Type II error using our view.

However, we believe that a small correlation, such as at the insignificant 0.03 level, is a much more realistic scenario to be simulating than an idealized world where the correlation is a perfect zero. Some form of correlation will occur by chance alone even if the variables are truly unrelated.[5] Therefore, Petter et al. simulated a situation that arises frequently in the collection of data from a population in that some relationships may have a very small, but still statistically nonsignificant, relationship. The 0.03 parameter estimate used in these simulations was a nonsignificant path when constructs were properly specified (with the noted exceptions of properly specified endogenous formative constructs with higher levels of intercorrelations and higher sample sizes). With empirical data, it is unlikely that the population would, in reality, demonstrate a relationship of 0.0 between two constructs; the relationship may not differ practically or in terms of statistical significance from 0.0, but it would vary and is likely to have a small relationship in terms of magnitude of the parameter estimate; in brief, it is statistically equivalent to zero. Essentially, the 0.03 value is at the upper bound of the nonsignificant range, which is a realistic circumstance in which the researcher had a barely insignificant finding, but it was still nonzero.

Both the approach used by A&M (i.e., relationship of 0.0) and our approach (i.e., relationship of 0.03) provide insight to readers. Each approach examines a different question about the potential danger of structural bias when a formative construct is misspecified when the actual relationship is exactly zero versus a practically and statistically nonsignificant value.

Theoretical Implications of Petter et al.

A&M state that, due to the bias found in Monte Carlo simulations in studies such as those conducted by Jarvis et al., MacKenzie et al., and Petter et al., it would logically follow that "a large portion of the empirical research in these areas, including the discipline of information systems, is ***largely*** invalid" (p. 2, emphasis added). First of all, Petter et al. found that 30 percent of studies in their three-year sample contained misspecifications and Jarvis et al. found that 29 percent were misspecified. If prior empirical research were "largely invalid," then we would expect that misspecifications were occurring in the vast majority of cases, not in only 29 or 30 percent of the cases. Moreover, Petter et al.'s analysis concluded that misspecification of only endogenous variables led to Type I and II errors. For many of the studies captured in the 29 percent figure, misspecification is unlikely to have threatened statistical conclusion validity. Finally, the simulations in Petter et al. do not suggest that some portion of IS research is "invalid," but rather argue that there is a potential for misinterpretation of theory where measurement misspecification could impact the interpretation of the structural model.

While A&M state that researchers should have concerns about the consequences of misspecification, they argue, based on their use of standardized coefficients in their simulations that, "the consequences of misspecification seem to be much less dire than previously thought" (p. 2). In that A&M draw empirical conclusions about the magnitude of biases from measurement misspecification when standardized parameters are used in their simulations, we consider this to be ancillary, and even potentially distracting, to the core issue: researchers must achieve correspondence between a construct's meaning and the formative or reflective specification of its measurement model, and they must understand the theoretical implications of measurement misspecification.

Theoretical Reasons for Proper Construct Specification

Petter et al. explain why construct misspecification has not only empirical consequences, but also theoretical consequences in IS research. They agree with Law and Wong (1999) who assert that "conceptualization of…constructs should be theory-driven" (p. 156). Researchers should consider the goals, objectives, and nature of the research as they determine whether or not the construct should be measured formatively or reflectively (Fornell and Bookstein 1982; Hardin et al. 2008; MacKenzie et al. 2005). When a researcher is developing measures for a reflective construct, the goal is to identify measures that are intercorrelated, have unidimensionality, and have strong internal consistency; when developing measures for a formative construct, the ideal is to explain unobservable variance, reduce multicollinearity, and consider the indicators as predictors of the construct (Diaman-

[5] This simulation uses a value (0.03) in which the sample sizes (i.e., 250 and 500) would not be statistically significant if the construct was correctly specified.

topoulos and Sigauw 2006). *Thus, if the measures used by a researcher are causal, rather than measures that reflect the latent construct, it changes our understanding of the construct and the nomology in which it is embedded.*

In a reflective construct, it is the change in the construct that causes a change in the measures. In a formative construct, it is the change in the measures that causes the change in the construct. By overlooking this important distinction between reflective and formative constructs, many researchers may not recognize that there are profound changes taking place in how we conceptualize the construct, how we consequently measure the construct, how we validate the construct, how we analyze the construct, and how we interpret the construct within a nomology. Researchers should take time to develop an auxiliary theory in which the relationships among the constructs are defined, content domains are specified, antecedents and consequence of the construct are determined, and the relationship between the construct and measures are identified (Kim et al. 2010). A construct with the same name, but varying in terms of being measured reflectively versus formatively, will *not necessarily* be the same construct (Diamantopoulos 2006, 2010). Once a researcher identifies the approach for measuring the construct based on theoretical or research objectives, then the researcher should develop "a contract with him- or herself to stick to the tenets and consequences of those decisions" in terms of subsequent measurement development and validity (Marakas et al. 2008, p. 538).

The nature of measurement purification also changes when the researcher determines that a construct can be measured formatively as opposed to reflectively. Unfortunately, construct misspecification could occur earlier in the research process than simply when analyzing the construct and specifying the relationship between the measures and the constructs. When the construct is being validated, the measures selected for inclusion will vary if the construct is measured formatively versus reflectively because the researcher has different goals when s/he begins the measurement purification of a formative construct as opposed to a reflective construct (Diamantopoulos and Sigauw 2006). For example, content validity (as opposed to reliability) becomes all important in the case of formative measures (Borsboom et al. 2004; Petter et al. 2007).

Furthermore, when a researcher chooses to use formative measures for a construct, s/he is able to gain insight on what affects change in the formative construct by considering the weights of the formative indicators in the measurement model (Cenfetelli and Basselier 2009). This insight can be important for theoretical or practical understanding of the factors that form the construct. Using empirical data, Diamantopolous and Sigauw (2006) evaluated the differences that could arise if a researcher developed a scale conceptualizing the construct as reflective versus formative. Then they evaluated the different scales for the same construct using the same dependent variable. In their study, while the variance explained in the dependent variable was similar across the two scales, the substantive conclusions drawn from the study were different. There were several additional relationships that could offer insight to researchers when the construct was properly specified and measured as a formative construct based on underlying theory. This empirical study is a useful demonstration of the need to achieve correspondence between a construct's measurement model specification and its theoretical meaning before one begins data collection and analysis.

Furthermore, by discounting the theoretical relationship between the measures and the construct, it creates problems in validating the construct. If a formatively measured construct is misspecified as reflective, the construct may be unable to satisfactorily meet the validity requirements for reflective constructs. Additionally, a researcher believing his/her construct to be reflective, when the construct is actually measured formatively, may remove indicators from the construct, which changes the conceptual domain of the resulting construct (MacKenzie et al. 2005).

In summary, a researcher may choose to measure a given construct either reflectively or formatively, but regardless of the choice, the choice made in how the construct will be measured has implications on the definition, the measurement, and the interpretation of the construct. There should be conceptual congruence between the researcher's theoretical definition of the construct and the measures used. Inconsistencies between the theoretical definition of the construct, the measures, and the relationship between the construct and measures introduces spurious error that can negatively impact the researcher's understanding and interpretation of the results (Bagozzi 2011).

Empirical Reasons for Proper Construct Specification

A misspecified formative construct analyzed as a reflective construct has problems with unidimensionality, which definitely impacts the structural model as demonstrated in study after study (e.g., Aguirre-Urreta et al. 2012; Anderson and Gerbing 1982; Jarvis et al. 2003; Law and Wong 1999; MacCallum and Browne 1993; MacKenzie et al. 2005; Petter et al. 2007). Anderson and Gerbing (1982) state that "a prerequisite to the causal analysis of constructs is satisfactory measurement of the constructs themselves" (p. 458). Satisfactory measurement of constructs requires that a researcher properly specify the relationship between the measures and the construct.

Misspecified constructs can impact the structural model, both in simulations (Aguirre-Urreta and Marakas 2008, 2012; Jarvis et al. 2003; MacKenzie et al. 2005; Petter et al. 2007) and in studies with empirical data (Law and Wong 1999). These impacts to the structural model affect understanding of the theory being tested or developed. Even A&M found in their simulation procedures that the structural model has negative bias for structural paths leading to a misspecified endogenous formative construct. Whether one believes in the use of unstandardized coefficients in simulation models (e.g., Jarvis et al. 2003; MacKenzie et al. 2005; Petter et al. 2007) or the use of standardized coefficients in simulation models as in A&M, we can all likely agree that *any* statistical bias is a cause for concern. Bias over- or under-states the actual relationship between variables and can lead to incorrect researcher conclusions about the relative strength of ties between constructs and within the nomology. After standardizing the coefficients in their simulation model and addressing their concerns related to scale metrics within the original simulations (see A&M Appendices A and B), A&M acknowledge bias in the magnitude of some structural parameter estimates can occur when formative constructs are misspecified as reflective. When A&M examine the simulations for statistical power, a variation not performed in Petter et al.'s simulations, their results supported Petter et al.'s original position that there is potential for Type II error when an endogenous formative construct is misspecifed as reflective.

One could then argue that construct specification is only relevant in some circumstances, such as endogenous, formatively measured constructs, based on the results of the simulations by A&M. However, the IS discipline strongly relies on past studies to develop measures for constructs per prior recommendations in the field (Straub 1989). When authors are developing and validating constructs and subsequently publishing their work, many researchers will be relying on these measures for years to come. Measurement misspecification may not only impact the initial study, but could also have a downstream effect on future research. A misspecified construct that was exogenous in one study could later become an endogenous construct in another study as research builds upon past research. If researchers unthinkingly assume that a construct can be misspecified without consequences, then they may find themselves changing the position of a construct in a nomology without realizing that their new model is now subject to a heightened chance of a Type I or II error. Therefore, the location of the formatively specified construct within the research model ultimately does not matter; researchers should properly specify the relationship between measures and constructs, regardless of the construct's position within the model as an exogenous or endogenous construct.

Conclusion

Irrespective of whether one agrees or disagrees with A&M's interpretation of the simulations performed by Jarvis et al., MacKenzie et al., and Petter et al., it appears that we all agree that measurement specification is still relevant and important for both theoretical and empirical reasons. In addition to those scenarios examined by A&M, studies have also found bias in the structural model using both simulated and survey data when constructs are misspecified (Diamantopoulos et al. 2008, Table 2). Assuming one agrees with A&M that improper specification of constructs is not as much a cause for concern as previously thought in terms of the magnitude of bias when standardized coefficients are used, there are still solid theoretical reasons to focus on measurement specification. Proper specification is critical, irrespective of whether measures are formative or reflective and whether standardized or unstandardized parameter estimates are used.

If, as researchers, we do not fully understand how our constructs are being measured, then how can we understand and define theory? If the measurement model is incorrect, then can we have any confidence in our research findings and the evidence they provide in support of, or against theories?[6] Problems that can arise due to problems with model misspecification, such as those also found by A&M for misspecified endogenous formative constructs, could lead to valuable research not being published. If a researcher foregoes proper measurement model specification, then the paper may have been rejected due to the appearance of poor measurement or due to results contrary to theory (Jarvis et al. 2003) or the researcher may interpret the results improperly and offer new or modified theories based on invalid results. Moreover, even if one agrees with the viewpoint espoused by A&M, the discussion about formative measurement is not rendered null and void in either IS research or any other discipline. While some researchers may disagree with the use of formative measurement or may believe that the magnitude of the empirical effect of construct misspecification is open for debate, the core requirement for good research is still to properly specify measures of constructs.

[6]We thank an anonymous reviewer for raising this point during the review process.

In their limitations and suggestions for future research, A&M recommend that we seek out better ways to detect model misspecification. We agree, but regardless of whether simulated models use standardized coefficients, fit statistics can suffer due to underlying problems with construct misspecification (Bollen 2011; Jarvis et al. 2003; MacKenzie et al. 2005). A&M focus on the magnitude of the parameter estimates, but ignore the fact that model fit declines when constructs are misspecified.

We note that the interpretational and statistical issues associated with formative measurement has received recent attention in the information systems field as well as other disciplines (e.g., Bagozzi 2011; Edwards 2011; Hardin et al. 2011; Kim et al. 2010), While a detailed review of this discussion is outside the scope of this research note, we observe that some constructs, such as firm performance, are better suited to formative measurement, while other constructs are better suited to reflective measurement (e.g., Diamantopoulos and Winklhofer 2001; Petter et al. 2007; Podsakoff et al. 2003). As a result, when grounded theoretically and analyzed properly, formatively specified constructs can play a valuable role in IS research.

In our original paper and in this discussion, we would like to reiterate the critical importance of ensuring that the theoretical definitions of constructs are appropriately aligned with the measurement of the construct. Regardless of one's opinion about unstandardized or standardized coefficients in simulations, no conscientious scholar should conclude that it is acceptable to misspecify a formatively measured construct as reflective or vice versa. We hope everyone can agree that all scholars should become highly sensitized to the importance of proper measurement specification of a construct.

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