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Veni, Vidi, Vici: Breaking the TAM Logjam

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Abstract:

Benbasat and Barki (2007) argue that TAM has been both a blessing and curse for the IS field and they detail reasons why this is the case. Our response to their critique is to highlight areas of agreement, disagree with one of their assertions, and extend their thinking along another, related line. Specifically, we agree that some TAM constructs, namely perceived usefulness and system usage, need to be more closely examined in order to break up the "black box" portrayal of these concepts. Our view of Benbasat and Barki's characterization of TAM as unassailable is that common methods bias has never been well tested and that TAM linkages may in fact be methodological artifacts. Finally, it is argued that the field desperately needs more parsimony in TAM models and that meta-analysis is one good way of achieving this goal.

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Introduction

Caesar said, with respect to Gaul, that he came, he saw, and he conquered ("Veni, vidi, vici"). To a large extent, TAM has achieved this same distinction. In fact, some would argue that TAM has "overconquered" in the sense that it has achieved this too well by creating a dominant but stifling paradigm. In their paper entitled "Quo Vadis TAM", Benbasat and Barki [B&B] (2007) ask where we are going with TAM, and, given their stature in the IS field and the persuasiveness of their points of view, the challenges they issue command respectful consideration. We would like to sketch out broad areas of agreement with their arguments and offer alternative perspectives on some aspects of their line of thinking.

B&B provide a straight-forward enumeration of their ideas at the beginning of their article, but we will rephrase them here to be assured that we are tracking them well. Their opening gambit is that a great deal of the field's attention has been consumed by TAM studies and that the basic TAM model has been established "almost to the point of certainty." This is ironic in many ways in that it gives the impression that TAM is the only way to study IT adoption and/or the impact of the IT artifact. This has led, again ironically, to "theoretical confusion" rather than to theoretical clarity for the field. Part of this theoretical confusion derives from B&B's view that a theory needs to provide "actionable advice." From their standpoint, TAM is highly deficient in this regard.

Another argument they advance is that overly simple conceptualizations and operationalizations of Perceived Usefulness (PU), Perceived Ease-of-Use (PEOU), and Use — which are, of course, the central constructs in TAM — have "blinded" researchers to the internal workings of these constructs. Moreover, researchers have multiplied TAM antecedents and reified the "frequency" measures of use without providing any new insights into how the black box of IT implementation really works. Finally, TAM++ models, including the UTAUT model (Venkatesh et al. 2003), have only added to the theoretical confusion because it is not clear which of these models should be the basis of future IS research. B&B offer solutions that include: (1) delving into belief sets associated with specific IT and particularize these to a phenomenon, especially if this means going beyond standard variables in the Theory of Planned Behavior (TPB); (2) opening up the black box of system use, and (3) even expanding this to longitudinal views of usage patterns; (4) developing antecedents to usefulness that embed truly practical advice; and (5) exploring objective measures of usefulness.

Broad Areas of Agreement

It is perhaps superfluous to elaborate on the broad areas in which we agree with B&B. A reason for doing so is to place our later reservations or alternative perspectives within this larger context. And we will not over-dwell on similarities, so it is likely worth doing.

We subscribe to the view of B&B that the focus of the field on TAM and the enriching effect this has had on the diversity of ideas is both its strength and its weakness. In their meta-analysis of TAM, Lee et al. (2003) also make this point abundantly clear. They estimate that about 10percent of the total journal capacity in the field has been occupied with TAM studies. Given the wide range of topics that are generally considered to be integral to the information systems field, this seems to be much too much.

B&B are insightful in arguing that we need a way forward with respect to the plethora of TAM variables that have been proposed and empirically supported. We carry this argument to what we see as a logical end in our discussion of parsimony (Section 3 below).

Another area of easy agreement is that the system use construct has been understudied and that many new insights are possible through opening up this black box. This is, in fact, the essential argument we advance in our 2006 *ISR* article on reconceptualizing system usage (Burton-Jones and Straub 2006). In this article, we offer a methodological approach for formulating valuable, new measures of system usage. We also present empirical evidence that the approach can be effectively applied to the use-performance nomology. As we note in this article, it would also be useful for TAM researchers to reconsider whether they really wish to explain system usage. Although TAM refers to "acceptance," the "acceptance" construct itself has never been clearly delineated.

It need hardly be said that we would also find ourselves in agreement with B&B that objective measures of usefulness should be explored. Straub et al. (2004) argue that theories are more robust when variant forms of measurement result in similar

conclusions. In fact, the very philosophical and epistemological basis of statistical meta-analysis is based on this assumption (Hunter and Schmidt 1990). Constructs are abstractions, and there is not a one-to-one mapping with measurement scales. There is, indeed, a one-to-many relationship.

In spite of these broad areas of agreement with B&B, we would respectfully disagree with their positions in one area: that TAM is indisputable.

The Unresolved Issue of Common Methods Variance in TAM

B&B argue that: "After 17 years of research and a ...multitude of studies investigating TAM and its variants, we now know almost to the point of certainty that perceived usefulness (PU) is a very influential belief and that perceived ease of use (PEOU) is an antecedent of PU and an important determinant of use in its own right."

We feel that an important caveat needs to be raised about the unassailability of TAM. Nearly all of the studies in this domain have measured the self-reported beliefs independent variables (IVs) of PU and PEOU using the same method as the dependent variables (DVs) of attitude toward use, intention to use, or user behavior (Lee et al. 2003, Sharma et al. 2004). Respondents are asked their perceptions of how useful and easy to use is the system, and then what they perceive to be their own level of usage. With this utilization of the same method (i.e., the same or common rater), the risk of common methods variance (CMV) is extremely high (Podsakoff et al. 2003).

Why would TAM studies have a so-called common rater problem? Consider the typical circumstances under which TAM data on the IVs and DVs are gathered. You as a subject or respondent are asked if you believe a particular information technology is useful in your work through a set of questions. Shortly thereafter you are asked if you use the technology (alternatively whether you have a positive attitude toward it or intend to use it). The cognitive dissonance is high for respondents unless their answers to these two sets of questions align. Otherwise, why would a reasonable person use a technology that s/he did not find to be useful? The problem, thus, is that respondents are not giving researchers data that is "independent" of the method that was used to collect it. The IVs and the DVs are related by the method that was used to query respondents, and this common self-reporting method can affect the bivariate correlations as much or more than any underlying theoretical linkage.

What this suggests is that independent sources of data relevant to usage behaviors (computer logs, for example) are virtually the only way to avoid significant common methods bias in TAM studies. But this approach runs counter to the cumulative tradition which, to date, has largely measured usage through self-reports.

This specific problem has been recognized prior to the current paper. In their review of TAM research, Lee et al. (2003) conclude that self-reported usage is the most critical limitation of TAM studies. Indeed, both Szajna (1996) and Straub et al. (1995) find that self-reports are not good surrogates of actual usage behaviors in TAM. These findings are consonant with other studies in the attitudes-behavior literature, where researchers conclude that self-reports may be reasonably accurate renderings of beliefs about behavior, but they produced results different from actual behavior in attitude-behavior tests (Manfredo and Shelby 1988).

We hasten to add that some studies have used actual measures of usage and found support for the Technology Acceptance Model (e.g., Venkatesh and Morris 2000; Venkatesh et al. 2003). Yet this does not abrogate other compelling evidence that, overall, there is a large CMV effect. In a meta-analysis of both published and unpublished works and 47 separate tests of the TAM relationships, Sharma et al. (2004) find that there is a marked difference between studies that use actual usage versus those that use self-reports. The large difference between these groups in terms of the strength of the TAM linkages strongly supports the possibility that CMV is a serious issue.

Without necessarily concluding that CMV is an albatross for TAM studies, we simply are raising the possibility that, without the field realizing it, methodological bias may have been a long standing problem. What should also give IS researchers pause in the face of these conflicting results is that there is documented evidence of common methods bias depressing explained variance across the business and education disciplines. In Cote and Buckley's analysis of 70 studies in marketing, management and education (1987), common methods variance added 24 percent, on average, to explained variance in these literatures. Marketing was the lowest among the three at 15.8 percent. Therefore, if the levels in TAM studies ranged from the low marketing figure (15.8 percent) to the disciplinary sample mean (24 percent), the loss in explained variance would still be substantial, and some of the touted explanatory power of TAM would disappear. This is, indeed, the conclusion of Sharma et al.'s meta-analysis (2004) where they find that CMV accounts for 27 percent of the variance in TAM. They find that the relationships between PU and Use and between Intention to Use and Use are significant and strong *only* when the variables are measured with the same method over all the studies examined; these relationships

were *insignificant* when the variables were measured with different methods. This finding is clearly at odds with B&B's view that the TAM relationships are known "almost to the point of certainty."

An important paper that challenges the possibility of detrimental effects of CMV on TAM has recently been published by Malhotra et al. (2006). They find that the average amount of CMV in TAM is about 16 percent, similar to that in marketing. The authors characterize this level of CMV as "low" (p. 1871), a judgment that might in itself be questioned. But their analysis goes into greater depth in suggesting that CMV lowers correlations by only 6 percent, which, admittedly, does not seem to be substantial.

Whereas their analysis would seem to exonerate TAM, there are several interesting caveats that must be raised about Malhotra et al.'s (2006) marker variable analytical approach. In order to follow our counterarguments, it is first necessary to discuss the marker variable approach of estimating CMV. The technique was introduced by Lindell and Brandt (2000) and Lindell and Whitney (2001), who assert that variables that are theoretically unrelated to at least one other variable in the study can be markers for CMV. The average of its correlations with these other variables in the nomology being studied must be due to CMV and, therefore, serve as a surrogate for CMV. In defense of this technique, it needs to be said that the marker variable technique follows an empiricist approach in which the researcher is simply postulating what s/he believes is a likely or possible amount of CM bias. It makes no claim of invincibility.

But, as Podsakoff et al. (2003) argue, this technique suffers from serious conceptual and empirical problems. One of these is that: "...because a marker variable is one that most people believe should not be related to the predictor or criterion variable, there is no reason to expect that it provides an estimate of the effect of a person's implicit theory about why the predictor and criterion variables should be related, and therefore partialling out the effects of the marker variable will not control for these sources of common method variance" (Podsakoff et al. 2003, p. 893). Podsakoff et al. (2003) present the rather dull example of the marker variable shoe size in a study of supervisors' leadership behavior leading to employee performance. Likely as not shoe size has little to do with either of the other variables, so it qualifies as a marker variable. But on what intellectual grounds can one partial out average shoe size correlations from the theoretical correlation between supervisory leadership and employee performance and argue that this fully and well accounts for CMV? By the same token, one could examine the correlations of marker variables like age of respondent and the traditional TAM variables, but it is an intellectual leap of faith to assert that respondent age can readily serve as the best adjustment for the true relationship between PU and Usage, and, therefore, as a good estimate of CMV.

One could argue even more stridently that marker variables will simply not control for common rater problems, which, we would submit, are most likely to be the key underlying issue for TAM studies, a limitation acknowledged, in fact, by Malhotra et al. (2006). Why not? When we have common rater problems, we are concerned that choosing this method of measuring variables like PU and Usage virtually prescribes respondent evaluations, as described above. The extent of CMV, though, is not assessable by a marker like age of respondent because it is not a matter of similar instrument format in gathering the markers and the other variables that is in question. What is in question is a matter of substance. The strength of the true relationship between PU and Usage is the theoretical substance that we as a field should be focusing on, and so gathering data from the same source is by definition problematic. This is the classic mono-method threat that Cook and Campbell (1979) and Campbell and Stanley (1963) have spoken eloquently about.

Finally, mis-specification of formative constructs as reflective can also complicate estimation of CMV (Podsakoff et al. 2003). It is our belief that a fair number of TAM researchers validate their measures of usage as if these DVs were reflective when, in fact, they are formative. Subsequent analysis of CMV (such as marker variables or any of the other standard techniques) would be based on this faulty assumption and, therefore, subject to erroneous conclusions. Formative measures should not necessarily correlate with one another (Diamantopoulos and Winklhofer 2001), and so this represents a specific undermining of the marker variable approach for estimating CMV.

Likely the best way to avoid CMV and to estimate the true relationship between IVs and DVs, thus, is independent measurement methods (Podsakoff et al. 2003). In this, we merely repeat what has become a canard in many scientific discussions. That being said, there is a cautionary note to this sacred cow. Campbell and O'Connell (1982) argue that when researchers choose methods that are very different for measuring the IV and DV, their results may systematically understate the true relationship between the variables. This is because each method suffers from its own problems, and these problems can sometimes compound (rather than counterbalance each other), resulting in dilution of the true effects.

Nonetheless, we believe that more IS researchers should undertake the strenuous effort required to gather usage data from sources that are independent of the assessment of the perceptions of usefulness and ease-of-use, and if TAM relationships still prove to be strong, then we will concur wholeheartedly with Malhotra et al. (2006) that CMV is not a problem. Sharma et al.'s meta-analysis (2004), already provides direct evidence from the TAM literature that CMV is a problem, but, given

the contrary findings of Malhotra et al. (2006), in our opinion, the jury is still out on whether TAM is a good theory or merely a methodological artifact.

The Issue of Parsimony

B&B make a series of arguments about improving future studies in implementation success (and TAM). Their general view is that it has been a serviceable “middle-range” theory and that it is time to move beyond it. They are keen on understanding the roots of PU, for example. This is particularly important because it will shed light on the contingency of the IT artifact and how it relates to use and success. They have other suggestions such as more longitudinal, multi-stage modeling and improving our understanding of system usage. The current authors could hardly contend that latter point, as we have raised this issue ourselves in a recent issue of *ISR* (Burton-Jones and Straub 2006).

So while we do not disagree with any of the remedies proffered by B&B, there is an issue that lies at the heart of theory-building that they did not address: parsimony. We believe that moving TAM research toward parsimony should be a high priority of the IS field.

Where do we stand with respect to a parsimonious view of TAM? The final development in the TAM evolutionary stream is the UTAUT model (Venkatesh et al. 2003). This model is deceptively parsimonious. It sets forth 10 constructs that are said to summarize the stream. In actuality, these constructs are shadowed by other concepts that have been studied in the tradition. For performance expectancy, one of the UTAUT constructs, the authors list these as: perceived usefulness, extrinsic motivation, job-fit, relative advantage, and outcome expectations.

In fact, the number of constructs that has been considered under the TAM umbrella is huge (Lee et al. 2003). Lee et al. (2003) enumerate 21 external variables that affect the four central variables in the model. This is anything but parsimonious.

Is there a way forward? An effective means of teasing out the critical variables in nomologies is statistical meta-analysis. Originally conceived of as a way of reviewing an entire stream of literature and removing measurement error in order to determine the true amount of correlation between independent and dependent variables (Glass et al. 1981), it was later refined to assist in theory development and refinement (Hunter and Schmidt 1990, Schmidt 1992).

It has been used in this way in numerous studies. In Nair (2006), for example, new directions for quality management theory are proposed as a result of the study findings. Indeed, King et al. (2004) conclude that changes in theories of mergers and acquisitions are needed, given the discovery of important moderators in their meta-analysis.

Although many researchers have conducted quantitative meta-analyses of certain TAM relationships (King and He 2006 Lee, et al. 2003, Ma and Liu 2004, Sabherwal et al. 2006, Schepers and Wetzels 2007, Sharma et al. 2004), no one to our knowledge has yet conducted a comprehensive meta-analysis that accounts for all of its relationships, associated external variables, and moderating factors. Thus, meta-analysis has not been used to refine TAM into its essential, parsimonious set of variables, including critical antecedents. What would be discovered, one can only surmise. But the goal, we believe, should be a more parsimonious model that focuses the attention of the field on the strongest linkages explored to date.

Conclusion

TAM has served the field reasonably well in that it has given IS researchers a theoretical model that speaks to the unique nature of information systems. But its strength has also been its weakness in that it rapidly drew too much empirical attention, partly as a result of its deceptively straight-forward constructs and measures. We believe that TAM should be revisited to ensure that usage is being measured in the best possible way, both from the standpoint of developing more sophisticated conceptualizations of what systems usage means in specific research contexts as well as from the standpoint of avoiding/estimating common methods bias. In the long run, a dispassionate assessment of the real contribution of espoused TAM variables will also allow the field to move forward with a parsimonious model of technology acceptance.



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