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Quo vadis, TAM?

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Abstract:

The Technology Acceptance model (TAM) is one of the most influential theories in Information Systems. However, despite the model's significant contributions, the intense focus on TAM has diverted researchers' attention away from other important research issues and has created an illusion of progress in knowledge accumulation. Furthermore, the independent attempts by several researchers to expand TAM in order to adapt it to the constantly changing IT environments has lead to a state of theoretical chaos and confusion in which it is not clear which version of the many iterations of TAM is the commonly accepted one. The present commentary discusses these concerns, speculates on the possible contributions to the current state of affairs, and makes several suggestions to alleviate the problems associated with TAM and to advance IT adoption research to the next stage.

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1. Introduction

The Technology Acceptance Model (TAM) is generally referred to as the most influential and commonly employed theory in information systems (Lee et al. 2003). Some also consider it to be the only well-recognized theory in IS (having dethroned Nolan's stage model that had this distinction earlier). The objective of the present commentary is to draw researchers' attention to the necessity of reorienting IT adoption and acceptance research toward potentially more fruitful avenues and away from "TAM++ research" that adds little knowledge to TAM or its many different versions.

The origins of TAM can be traced to the Theory of Reasoned Action (TRA) (Fishbein and Ajzen 1975). TRA requires that *salient beliefs* about one's attitude toward a particular behavior (e.g., buying on the web) be elicited in order to be relevant to the specific behavior being studied. As one approach to eliciting salient beliefs, Davis (1986) and, in one of the most frequently cited papers in IS, Davis et al. (1989) proposed two constructs, *perceived usefulness* (PU) and *perceived ease of use* (PEOU), that are included among the set of the perceived characteristics of innovations (Rogers 2003),through which we can capture all relevant beliefs in information technology (IT) usage contexts. Davis' (1986) approach largely simplified TRA, as well as making it more efficient to conduct IT adoption research and facilitating the aggregation of results across settings. In this regard TAM can be viewed as very successful. However, such success sometimes has unintended consequences. As an alternative approach to eliciting salient beliefs in each specific case associated with an IT use context, Moore (1987) and Moore and Benbasat (1996) proposed utilizing as a generic set of beliefs the full set of *perceived characteristics of innovations* (Moore and Benbasat 1991) identified in Rogers' influential work *Diffusion of Innovations* (Rogers 2003).

After 17 years of research and a large multitude of studies investigating TAM and its many variants, we now know almost to the point of certainty that perceived usefulness (PU) is a very influential belief1 and that perceived ease of use (PEOU) is an antecedent of PU and an important determinant of use in its own right. Unfortunately, we believe that, in spite of its significant contributions, the intense focus on TAM has led to several dysfunctional outcomes: 1) the diversion of researchers' attention away from important phenomena. First, TAM-based research has paid scant attention to the antecedents of its belief constructs: most importantly, IT artifact design and evaluation. Second, TAM-based research has provided a very limited investigation of the full range of the important consequences of IT adoption, 2) TAM-based research has led to the creation of an illusion of progress in knowledge accumulation, 3) The inability of TAM as a theory to provide a systematic means of expanding and adapting its core model has limited its usefulness in the constantly evolving IT adoption context, 4) The efforts to "patch-up" TAM in evolving IT contexts have not been based on solid and commonly accepted foundations, resulting in a state of theoretical confusion and chaos. We discuss each of these concerns in section 2. In Section 3 we speculate on the possible reasons underlying the current state of affairs. In the last section of the paper, we make several suggestions to alleviate the problems we see with TAM and to advance adoption research to the next stage.[2]

2. Theoretical Concerns with TAM

One important unintended consequence of our heavy reliance on TAM can be viewed as the "putting of blinders" on IS researchers, diverting their main focus from investigating and understanding both the design- and implementation-based antecedents, as well as the behavior- and performance-based consequences of IT adoption and acceptance.

In regard to *antecedents*, an undesirable, albeit unintended side effect of TAM has been its diversion of researchers' attention away from a key IS research objective or theme: that of artifact design (Benbasat and Zmud 2003; Orlikowski and Iacono 2001). While we do not doubt that Davis et al.'s (1989) original intention was that the influence of system and other characteristics be studied through TAM's constructs, study after study has reiterated the importance of PU, with very little research effort going into investigating what actually makes a system useful. In other words, PU and PEOU have largely been treated as black boxes that very few have tried to pry open. Contrast this with the period up to the 1990s when

[1] One of the authors of this paper was told by Fred Davis that one reason why he developed TAM was a question he was asked by a well known software developer about why a new system they implemented was not being used very much even though it had a very well-designed interface. The answer turned out to be that the system was not perceived to be useful!

[2] Note that the present commentary focuses on theoretical concerns related to TAM. Methodological concerns with TAM research, such as method bias (e.g., Lee at al., 2003), while important, are outside the scope of this commentary.

empirical behavioral researchers conducting laboratory experiments, e.g., in the area of DSS, investigated and evaluated a wide range of IT-based support functionalities using both objective (time, task performance) as well as perceptual measures of efficiency and effectiveness, roughly corresponding to PEOU and PU, respectively. This, we submit, is a more valuable endeavor, especially after we reached a saturation point in TAM work after which few surprises were evident. Thus, while TAM has had the potential to provide a vehicle for accumulating knowledge from such system functionality- (or characteristic-) based studies in a meaningful way, the knowledge that "usefulness is useful" has, in fact, provided little in terms of actionable research (Benbasat and Zmud 1999) and hence a paucity of recommendations to direct design and practice. While we are at a loss to explain why researchers have elected to so intensively focus on TAM rather than on using TAM to study other factors (e.g., Wixom and Todd 2005), a contributing reason may have been that while theorizing the relationship between the TAM constructs is easy, theorizing the effect of system characteristics on TAM factors is definitely harder.

A related concern associated with antecedents is the creation of an "illusion of cumulative tradition." The abundance of papers that more or less replicate TAM's original message can lead to the unwarranted belief that, because the same constructs and measures are being used, an accumulation of knowledge is occurring. Over the years, the additions that have been made to TAM include constructs such as trust, cognitive absorption, self-efficacy, job relevance, image, result demonstrability, disconfirmation, information satisfaction, top management commitment, personal innovativeness, information quality, system quality, computer anxiety, computer playfulness, and perceptions of external control. While researchers have made sound justifications for the addition of each of these *individual* constructs to TAM, in the final analysis this approach has basically provided explanations or antecedents for one set of belief perceptions (i.e., PU and PEOU) via another set of belief perceptions, without also increasing our knowledge of what makes an IT useful.[3] The extensive TAM research has reinforced our knowledge of the underlying TAM relationships without substantially extending that knowledge to a broader or more specific set of relationships, especially those about design. We believe that this is both creating an illusion of knowledge accumulation and acting as a barrier to fruitfully extending the model backward toward IT, implementation, and design factors, leading to research that is unable to provide actionable advice.

In regard to *consequences*, an undesirable outcome of TAM has been the intensive focus on the prediction or explanation of a single behavior conceptualized in a narrow manner, i.e., system use defined and operationalized as an amount or frequency. As several researchers have pointed out, such a simplistic view of system use has important shortcomings (DeLone and McLean 2003; Doll and Torkzadeh 1998; Straub et al. 1995). Moreover, an intense focus on this narrowly conceptualized behavior has also led to TAM researchers neglecting to study other important user behaviors (Agarwal 2000; Johnson and Rice 1987; Nambisan et al. 1999; Orlikowski 1992, 1996) such as reinvention (Rice and Rogers 1980) and learning (Papa and Papa 1992; Vandenbosch and Higgins 1996), which are not only interesting in their own right, but are also highly relevant to understanding IT implementation, adoption, and acceptance. It seems that the exclusive focus on the amount or extent of usage as the dependent variable has blinded researchers to other salient user behaviors. In effect, TAM appears to have provided a secure cocoon from which few have dared to venture outward. Again, this may have occurred because the internal strength of TAM's logic has dissuaded researchers from engaging in the hard work of theorizing about how TAM's constructs might differentially influence other behaviors. For example, an interesting question that needs to be answered is whether or not PU and PEOU, as currently conceptualized, are as powerful in explaining users' reinvention and learning behaviors as they are in explaining the amount of system usage.

In addition, the research community has invested significant resources and research effort in conducting numerous TAM studies. Ironically, a synthesis of these efforts has resulted in a model (i.e., UTAUT in Venkatesh et al. 2003) that essentially brings us back full circle to TAM's origins, as described in the Introduction section. In essence, TAM started out as a simplified adaptation of TRA to IT contexts (Davis et al. 1989). And now, after years of investigation, social influences and facilitating conditions are being added to the two main constructs of TAM, i.e. PU and PEOU. Adding social influences and facilitating conditions to TAM results in a model that is not very different from the Theory of Planned Behavior (TPB), since these two constructs overlap considerably with TPB's subjective norms and perceived behavioral control. TAM's PU and PEOU can be viewed as antecedents of TPB's Attitude,

Related to the above point is the fact that TAM initially not only excluded subjective norms and perceived behavioral control, but due to its dominance, it also made the consideration of potentially *salient beliefs* other than PEOU or PU (e.g., cognitive absorption, Agarwal and Karahanna 2000; trust, Gefen et al. 2003; enjoyment, van der Heijden 2004) and their inclusion in research models more *difficult*. Because TAM does not provide a mechanism for the inclusion of other salient beliefs, and given its dominance as a model, researchers have to justify the addition of any beliefs other than PU and PEOU, as an

[3]Note that, while antecedents, such as trialability, visibility, image, relevance, etc., are characteristics of an IT, they still reflect individuals' beliefs.

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extension of TAM. However, this is contrary to the spirit of TRA, the source of TAM, where a variety of salient beliefs may be generated depending on the specific context of IT use.

Some readers may wonder why we are criticizing TAM even though, as we describe above, several researchers have already published work that has gone beyond the original confines of TAM. There are several reasons. First, such efforts have been few and far between, and a very small proportion of all published TAM research. Second, as noted earlier, each researcher who has done so has had to go through a lengthy exercise to justify an extended model. Third, and *most importantly*, we are now left in a state of methodological vacuum and theoretical confusion since there is no commonly accepted adoption model in IS, and the original TAM has outlived its usefulness. Currently, we have a number of versions of TAM and TAM-like models, any one of which might be "preferred" by the reviewer who happens to evaluate one's work. As a consequence, IS researchers are at a loss to decide on which adoption model to base their new work. Is it the last version published in a top IS journal, which may appear long after one has completed her study based on an earlier published version, or will any one of the current models do equally well? This state of affairs is not tenable for the reasons we have explained, and clearly is detrimental to developing a cumulative research tradition on adoption research in an orderly fashion.

3. Why did we end up here?

One plausible explanation for the reason TAM has evolved the way it has can be found in the changing context of IT, or the restricted context in which TAM was tested. Our concerns about the evolution of TAM take on greater salience when the changing nature of IT applications is considered. These changes have led to the evolution of IT applications from a single-user system in an organizational context to multiple users communicating via technologies in inter-organizational and more global settings, to users interacting with multiple entities consisting of both technology and online merchants as well as other customers in on-line settings. This evolution has created conditions under which PU and PEOU have largely ceased to be the sole salient beliefs. For example, *trust* in online shopping contexts, *cognitive absorption* in Internet usage, and the *social presence* of others with whom one is communicating in collaborative online work contexts represent salient beliefs that have become increasingly more important. It is also the case that when the application of IT is considered in other important and emerging non-business contexts, such as health care, other salient beliefs such as *image* (Moore and Benbasat 1991) become important (Hebert and Benbasat 1994).[4] Thus, researchers have sought to add constructs to TAM as these became relevant to the changing technology, leading to the present situation.

Perhaps another explanation for the current state of affairs with TAM is that the security and lower risk provided by staying within the confines of a dominant paradigm have made us complacent concerning the *status quo*. Making minor adjustments and not daring to venture too far away from home, i.e., the strong theoretical anchor provided by TAM, provide an "intellectual" protection that allows the *justification* part of an academic investigation to be more easily achieved. Unfortunately, this does little for the *discovery* aspect of the work conducted, hence shifting the emphasis toward "true" to the detriment of "new." In many ways, the present situation reminds us of Kuhn's (1970) structure of scientific revolutions. While TAM initially helped tie together the inconsistent and scattered knowledge that existed regarding IT adoption and use, it also turned into a dominant paradigm that has led to the creation of lots of consistent knowledge about a narrow slice of the IT domain.

Moreover, another reason for adhering to the global and generalized perceptions measured in TAM, which has resulted in our lack of understanding of its antecedents, is that opening the black box of usefulness is neither straightforward nor trivial. When TAM is applied to a new technology, it is not clear which component or components of the particular technology are perceived to be useful and which ones are not, even when a user labels it as useful, thus leading to a lack of practical lessons for design. A deeper understanding of usefulness would require detailed studies, possibly via experimental designs, which are costly and time consuming to conduct. Moreover, undertaking this additional work to answer such questions requires a currently unavailable theory of usefulness to explain the relationships that can exist between users' perceptions and IT characteristics, as well as to identify the possible moderators of those relationships.

4. Proposed courses of action

Based on the concerns and their causes discussed earlier, we will make five recommendations to take the IT adoption literature beyond TAM, to the next generation of adoption and acceptance research. First, we suggest going back to the original theory, TRA, or preferably its more comprehensive version TPB (Ajzen 1991). Since extensions of the belief set are a natural application of TRA and TPB, this course of action would open up the left hand side of the model (antecedents), where the salient beliefs are identified, and help provide an adequate theoretical grounding that researchers can use to

[4] In the context of the implementation of bedside terminals, it was observed that nurses viewed that technology utilization was contrary to their image as human care givers.

incorporate into their models various constructs relevant to the changing nature of IT applications. This will provide the theoretical green light for going beyond TAM and allow for novelty and discovery. Some researchers are already beginning to move in this direction by using TPB instead of TAM as their research model (e.g., Pavlou et al., 2006). Note that, using TPB instead of TAM-based models will not eliminate the original TAMs contributions so long as PU and PEOU are modeled as, or among, the key belief antecedents of TPB's Attitude construct.

However, it should be noted that going back to TPB is not a complete solution, since TPB has its own problems, such as its exclusion of emotions (e.g., enjoyment) and habits (Triandis 1980), the moderate correlations between global and belief-based measures of its constructs, and the lack of knowledge regarding the precise nature of the relationships between attitudes, subjective norms, and perceived behavioral control (Ajzen 1991). Moreover, our investigations of TPB's system and design antecedents are not likely to be very fruitful unless we can first develop sound theories about the IT artifact, an area of weakness in the IS field that has already been underscored (Orlikowski and Iacono 2001). By theorizing about the IT artifact, it may also be possible for IS researchers to extend TPB in ways that address its shortcomings, and make significant contributions not only to the IT literature, but to the organizational behavior literature as well.

Second, as TPB is silent regarding the dependent variable, we also need to better conceptualize system usage so as to include a broader perspective of what users actually do in and around the notion of system use. While several researchers have noted the limitations inherent in our current conceptualizations and operationalizations of system use in terms of frequency, duration, or variety of system functions used (DeLone and McLean 2003; Doll and Torkzadeh 1998; Straub et al. 1995), to date very little research has been done to address this issue (for an exception see Burton-Jones and Straub 2006). As a solution, we propose that researchers broaden their perspective of system use from one that exclusively focuses on a narrow "amount" view of users' direct interaction with systems to one that also includes users' adaptation, learning, and reinvention behaviors around a system (Barki et al., forthcoming). The literature provides many examples showing the important influence that such behaviors have on IT implementation outcomes (e.g., Beaudry and Pinsonneault 2005; Majchrzak et al. 2000; Orlikowski 1996), with some researchers recommending the development of a broader conceptualization of IS use (e.g., Agarwal 2000; Saga and Zmud 1994). The advantages of an expanded behavioral view of IS use include a more faithful representation of usage activities that users engage in, stronger links with salient outcome variables such as individual performance, and its applicability to both voluntary and mandatory usage contexts (since users' reinvention and learning behaviors can take many forms and are mostly freely chosen, a system use construct incorporating them would be applicable in mandatory contexts as well), thereby largely eliminating a nagging limitation that has cramped the "amount" views of IS use.

Third, longitudinal, multi-stage models are needed to better capture the influence of salient belief variables on system use at different stages of an implementation, and the subsequent influence of this usage on users' beliefs at later periods. With a few exceptions (e.g., Bhattacherjee 2001; Bhattacherjee and Premkumar 2004; Karahanna et al. 1999; Kim and Malhotra 2005; Venkatesh and Davis 2000), many TAM studies typically focus on static models and measure all model constructs concurrently. As such, they do not adequately capture or describe the dynamic interplay that usually occurs between various user behaviors that revolve around system use from go-live to the relatively more stable and steady states of an implementation. It should be noted that our recommendation is not only to develop and test multi-stage models that focus on a broad and comprehensive range of behaviors as consequences instead of the *single, narrowly conceptualized usage behavior* of past research, but to observe these behaviors more than once over time as well. Longitudinal studies that view and assess system use over time are likely to be particularly revealing, as they can help us better understand the fluid relationships that exist between an adoption model's constructs and a variety of mutually influential set of behaviors users typically engage in, such as their adaptation, learning, and hands-on usage behaviors, as well as the subsequent influence of these behaviors on users' future beliefs.

Fourth, we need to identify the antecedents of the beliefs contained in adoption models in order to benefit practice. Focusing on the mediators of the impact of IT design on adoption is beneficial to the extent that this identifies which ones are important. However, repeatedly demonstrating that certain mediators (beliefs) are influential without understanding how to influence such beliefs through IT design is ultimately of limited value. For example, it is clear from extensive work on TAM that usefulness is a key, if not *the* key, influential belief influencing use. Therefore, it would be fruitful to investigate the antecedents of usefulness in order to provide design-oriented advice. However, to be able do so in a systematic fashion, we first have to develop a taxonomy, or preferably a theory, of usefulness. A good conceptual starting point for such an endeavor may be an augmented task-technology fit model (Goodhue 1995) that would provide finer and more focused design advice in specific task contexts. We already have evidence from the literature on transaction processing systems - DSS, GSS, and knowledge-based systems--about what makes a system useful. For example, Todd and Benbasat (1999) have demonstrated how decomposing the *normative* strategies for handling preferential choice problems to their lower level processing steps can be helpful in identifying those IT-based decision aid functionalities that are useful in reducing the cognitive cost of implementing such strategies and hence lead to their implementation. Ye and Johnson (1995) have shown

that in the special context of expert (knowledge-based) systems, where the decision maker will not use the advice provided without understanding its reasoning, explanation facilities are useful for increasing system adoption. Similarly, a consideration of the relatively new and important B2C e-commerce context provides a good illustration that the antecedents of usefulness vary greatly. For example, better product understanding in online shopping usefully improves the quality of buying decisions. In this context, functional control and visual control technologies are useful because the enhanced vividness and virtual interactivity they provide lead to improved product understanding (Jiang and Benbasat 2004). However, if the focus is on online collaborative shopping, then a technology that allows two physically separated friends to be able to see the same products on web pages (*what I see is what you see*) would be useful.

Fifth, from a practical point of view, there is the need to consider the solely perceptual belief-based focus approach the IT adoption models have followed to date. We need to make sure usefulness is measured beyond perceptions where possible, in that we would like to identify IT artifacts that are not only perceived to be useful but that can also be objectively shown to be useful (Davis and Kotteman 1994).

In conclusion, the main thesis of this commentary is that TAM has fulfilled its original purpose and that it is time researchers moved outside its limited confines. The key problem with TAM could be described as a focus on a middle range theory that provides a potentially useful bridge to antecedents and consequences of adoption, but the bridge seems to have become an end in itself.[5] We do not dispute the fact that several adoption researchers have ventured beyond TAM. However, our big concern is that the dominance TAM has attained has caused a high degree of enforcement, conformity, and lack of innovation that have not served the IT adoption research community well. We have recommended that researchers revisit the core theory of TPB and redirect their focus toward examining different antecedents (e.g., IT artifact and design) and different consequences (e.g., adaptation and learning behaviors) in order to reach a more comprehensive understanding of what influences adoption and acceptance in different IT use contexts and to provide more useful recommendations for practice. The challenge to the IS research community is to provide a systematic way to fully identify and explore such factors. As an example, a helpful step toward identifying antecedents has been provided by Wixom and Todd (2005), who have proposed the information and system quality perspectives to explain and understand what variables (e.g., accuracy and accessibility) drive usefulness, leading the way to the next step and challenge of identifying the IT artifacts and their characteristics that influence and enhance these variables. We hope that the suggestions made in this commentary will lead to a richer and more comprehensive theory of IT adoption that takes into account the constantly changing context of IT as well as one that provides IS researchers more degrees of freedom to be creative and adapt the theory to novel IT applications that cannot be foreseen at this time.

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[5] We thank Peter Todd for bringing up this point.

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