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Comment on Benbasat and Barki's "*Quo Vadis* TAM" article.

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Abstract:

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Commentary

I had two immediate reactions on reading Benbasat and Barki's "*Quo Vadis* TAM" article. The first was that Benbasat and Barki have made a very good point–it is time for the IS field to look in other directions; perhaps we have overworked TAM. The second was that I cannot stress enough how powerful and valuable the TAM model has been in our field.

TAM was valuable because (1) it drew on a body of research (Fishbein and Ajzen's TRA) that had been refined over time to a quite elegant and compelling formulation and (2) because Fred Davis took that formulation and reshaped it to apply to a particularly difficult problem in the IS field: why don't people make more use of information systems? In a field without a lot of conceptually compelling theories that had clear practical implications, it is not surprising that TAM generated a lot of interest. While it is possible to argue that we have now so over-studied the adoption/utilization phenomenon that additional studies make very little contribution, it is also true that the IS field badly needed a compelling answer to the question of why individuals use systems, and now we have one. That is not a small contribution.

But I applaud Benbasat and Barki in their insight that there have been some negative consequences from TAM. TAM is like any good theory in that it is a lens that lets us focus on one view of reality and see important relationships. But like any lens, it brings some things into focus and blurs others. This can leave an academic field with some blind spots and, I would argue, TAM has left us with some significant blind spots. In large part this is because TAM only asks a limited question ("What causes users to utilize a technology?") when the overall question ("By what means do technologies affect performance?") is ultimately more important. I will focus here on two blind spots that I think are especially important, one that comes after TAM in the technology-to-performance chain, and one that comes before TAM.

In my mind the most significant of these blind spots is due to the fact that, stated or unstated, virtually all TAM research makes the implicit assumption that *more use is better*. In other words, more utilization of a technology increases performance. In fact this assumption is often false in practice.

This is well illustrated by Pentland (1989) in his study of revenue agents' use of a new Automated Examination System (AES) at the USA's Internal Revenue Service. The AES was intended to support field audits of completed tax returns. It had functionality that included database, word processing, spreadsheets, and a tax calculator. Although the system was well received by the revenue agents and many of them used it extensively, Pentland's analysis showed that there was no empirical link between the amount of overall AES use and the performance of the agent (objective measures of use and of the average agent time per case or the average $ per agent hour.) With more detailed analysis it became clear that more use of the word processing and tax calculation functionality did improve performance, but more use of the database and spreadsheet functionality reduced performance. Pentland concluded that the spreadsheet and database aspects of the AES technology were a "poor fit" for most of the tasks the agents were performing. Whenever a technology is a poor fit for the intended tasks, more use will not improve performance, and will probably reduce it. Clearly more use is not always better.

How often are information systems a poor fit for the tasks to which they are applied? Sadly, the answer is too often. Pentland's article is a clear example that designers' perceptions about what users need may not be accurate. Both Pentland's article and work by Davis and Kotteman (1994) demonstrate that users may make choices about technology use that are inconsistent with the actual fit of the technology to the task. Therefore, decision makers may choose not to use a system even though it is demonstrably a good fit to their tasks (in Davis and Kotteman' study, a quantitative decision rule capability), or choose to use a system even though it is demonstrably a poor fit to their task (in Davis and Kotteman's study, a what-if analysis capability).

This reasoning and these empirical studies should encourage IS researchers to use extreme caution before they assume that more use of an information system will lead to higher performance. That being the case, the TAM model (which only goes as far as utilization) is quite incomplete if our goal is to understand how technologies can affect performance. A simple addition to the model might be: for a technology to positively affect performance, it must be utilized and it must be a good fit to the task. Given that despite the hype, many technologies are not a good fit to the task, this could make a huge difference in the way we think about TAM and the utilization of IT.

The second blind spot I would point out mirrors Benbasat and Barki's lament that there is "very little research effort going into investigating what actually makes a system useful." They argue that more attention needs to be focused on the design of the IT artifact. I would argue that although the IT artifact is almost always a part of our models (if not as a box on the diagram, at least prominently in our minds), a key missing construct that is too often not part of our thinking is the task of the user, and the fit of the IT artifact to that task. I might go so far as to say the fit of the IT artifact with the task requirements is the number one determinant of usefulness. To state this another way, it would be foolish to focus too intensely on how the design of the hammer affects its usefulness, without first asking whether the task is to drive in a nail or to saw off a piece of wood. A consequence of this primacy of task would be that no one should ever design or recommend an information system without first conducting a thorough study of the actual tasks in which users are engaged. We all give lip service to this (i.e. we talk about requirements analysis), but so often the requirements analysis is perfunctory and the designed system does not truly meet the users' needs (as in Pentland's study).

Anyone who knows my work will not be surprised that I have suggested that task- technology fit is a critical construct both before the decision about technology use and after the decision about technology use, i.e., as an antecedent to TAM and as a key construct between TAM and performance impacts. Perceived task-technology fit is a key predictor of perceived usefulness, and thus affects utilization (in the TAM model). Actual task-technology fit is a key moderating variable between utilization and performance impacts. Although Benbasat and Barki point to one of my papers (Goodhue 1995), I would suggest that a different paper (Goodhue and Thompson 1995) is a much more helpful (and cited twice as often!) conceptual starting point for filling in the larger technology-to-performance chain.

However, most task-technology fit models are static. In the modern world, tasks are often changing, making changes to the designed IT artifact necessary. Perhaps the proper larger question for the IS academic field now is how to design (and redesign) tasks at the same time as designing (and redesigning) information systems, or how to design the entire work system (Alter 2006), including task, technology, participants, work practices, etc. to meet the changing needs of the organization. TAM and the issue of whether or not and why individuals use a given technology will always be important concerns to the design of work systems. But the TAM model in itself does not encourage us to think about these larger contexts.

I will add one final comment, again mirroring an insight of Benbasat and Barki, but with a twist. In answer to their question, "How did we get here?" Benbasat and Barki offer several possible explanations, including that for a researcher to go beyond the powerful conceptualization provided by TAM is both risky and difficult. I would suggest that the problem is *not* that IS researchers are inherently averse to engaging in either difficult or risky research. Instead, I would lay the blame on our doctoral education programs, in which we emphasize so heavily that research must be "theory–based," without helping doctoral students understand what that means. Taken to an extreme, the idea that research must be theory-based suggests that all research must start with an existing theory and make a small addition to it. That is exactly what Benbasat and Barki lament has happened with TAM.

But what about important real world problems for which there isn't a clear established theory that can be used to illuminate the issue. Who will be the first to borrow a distant theory base and craft it to the new domain, or to use clear conceptual thinking and compelling logic to develop a prototype theory that will be the basis of other researchers' refinements? It is truly difficult and risky to be the first to argue for a new way of thinking about an issue (as a doctoral student named Fred Davis did with TAM). But these are the real contributions to the field. If our doctoral students (and our journal reviewers!) would better understand this, the IS field would be much more vibrant, and contribute more to society.

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