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Post-positivist Review of Technology Acceptance Model.

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Abstract:

This paper reflects upon the technology acceptance model (TAM) from the perspective of the post positivistic philosophy of science. I explore what it is to know, what a theory is, and what it means to be scientific in the context of TAM. In particular, I review criteria for determining whether TAM is scientific or not in light of post-positivistic debates about the nature of science. For this purpose, I apply Popper's principle of demarcation, which determines whether a theory-like TAM-is falsifiable and the logical connection argument to show that connections between actions and intentions cannot be subjected to empirical testing similar to connections between chemical entities. I also draw on Kuhn's notion of scientific revolutions to observe the degree to which TAM has become normal science. Finally, I review TAM from the Lakatosian perspective of scientific research programs to evaluate whether the program is advancing or declining. My main objective is not to provide a conclusive evaluation of TAM as a research program or a paradigm, but to open the philosophical foundations of TAM for scrutiny so that it can be evaluated not only within the validation rules followed by its proponents, but by applying a set of well known criteria established in the post-positivistic views of science.

Key Words: technology acceptance model, TAM, theory of reasoned action, philosophy of science, critique, problems, information systems, information technology, research methods

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1. Introduction

The Technology Acceptance Model (TAM) has its roots in cognitive psychology, specifically in Fishbein and Ajzen's (1975) theory of reasoned action (TRA). TRA was formed to predict the acceptance of information or advice for inducing change in behaviors: for example to predict when someone would quit smoking if advised to do so (Grube et al. 1986). Fishbein and Ajzen proposed that the behavior of an individual could be predicted, if we were to know their beliefs,[1] attitudes and intentions. TRA is a deterministic and normative theory in that it assumes that chosen actions are rational. In Fishbein and Ajzen's (1975: 14) words: "The totality of a person's beliefs serves as the information base that ultimately determines his attitudes, intentions and behaviors. Our approach thus vies man as an essentially rational organism, who uses the information at his disposal to make judgments, form evaluations, and arrive at decisions."

In 1989, Davis and his colleagues (Davis et al. 1989) formulated a theoretical model based on TRA to predict computer use, called Technology Acceptance Model (TAM). The rationale of Davis et al. was that, given that TRA predicts any behavior, it could be used to predict computer use: "TRA is very general, "designed to explain virtually any human behavior" (Ajzen and Fishbein, 1980, p.4), and should therefore be appropriate for studying the determinants of computer usage behavior as a special case" (Davis et al. 1989: 983). In TAM, the acceptance of a technology by an individual, is accordingly predicted by two specific beliefs: that the technology is useful and that it is easy to use.

Since its introduction to the Information Systems (IS) field, TAM has been the basis for a substantial number of research pieces (see King and He 2006; Schepers and Wetzels 2007, and Davis et al. 2007 for a review in this volume) and adaptations (see Venkatesh and Davis 2000; Venkatesh et al. 2003, and Benbasat and Barki 2007 in this volume). Despite its widespread acceptance (no pun intended), IS researchers have not carefully scrutinized the philosophical and epistemological foundations of this model: to what extent it meets the criteria for scientific theories established for causal, positivistic explanations. This gap motivates this paper. My goal is to examine TAM from the perspective of tha post-positivistic philosophy of science. Philosophy of science, in general, is concerned with questions that are not addressed by either the natural or social sciences themselves (Rosenberg, 2000) and it deals with questions like "What is the nature of scientific knowledge?", "What is a scientific theory?" and "What distinguishes science from other human activities?" (see Chalmers, 1999). I will maintain focus on how these issues have been addressed by post-positivists: a stream of philosophers that established new rules for doing science after the breakdown of the Vienna circle. The most famous name in this circle is Popper (1972), but other well known names are Lakatos (1974) and to some extent Kuhn (1996), who introduced the first evolutionary model for scientific theories.

The paper is organized by a set of "trials" where I apply principles of the philosophy of science as proposed by Popper, Kuhn, and Lakatos to scrutinize the meta-theoretical and scientific foundations of TAM. Thus, in the next section, I will apply Popper's principle of demarcation to determine whether a TAM theory is falsifiable. In Section 2, I use Kuhn's idea of scientific revolutions to establish the degree to which TAM has become normal science. In Section 4, I will examine TAM using the Lakatosian idea of scientific progress based on his framework of scientific research programs. My main objective is not to provide a conclusive evaluation of TAM as a *research program* or a *paradigm* but to open the philosophical foundations of TAM for scrutiny so that it can be evaluated not only within the validation rules followed by its proponents, but by applying a set of well known criteria established in the post-positivistic views of science. The purpose is to offer readers a re-constructive criticism about the current state of TAM as a research program in our discipline.

2. TAM and Popper's Principle of Demarcation

Popper's principle of demarcation derives from his opposition to the earlier logical positivist view of science, where observations are seen to be enough to *confirm* a theory. His argument draws upon Hume's criticism on induction. Hume argues early on that there are no bases to assume that the future is going to resemble the past. Russell (1980: 35) offers a wonderful example of this problem:

1 The subjective norm, to which Fishbein and Ajzen define as the beliefs an individual has regarding how appropriate their action would be judged by institutional agents, is considered here as part of the individuals' beliefs.

> The man who has fed the chicken every day throughout its life at last wrings its neck instead, showing that more refined views as to the uniformity of nature would have been useful to the chicken… But in spite of the misleadingness of such expectations [that the future is going to resemble the past], they nevertheless exist. The mere fact that something has happened a certain number of times causes animals and men to expect that it will happen again. Thus our instincts certainly cause us to believe that the sun will rise tomorrow, but we may be in no better a position than the chicken which unexpectedly has its neck wrung.

Popper elaborates his principle of demarcation to overcome these limitations. He states that there are no reasons to believe that a theory is scientific only because data–no matter how much of it there is–confirm it. To illustrate this idea, Popper offers the following example: a theory "all swans are white," is formed by an observer who repeatedly sees white swans. Popper indicates that no matter how many white swans an observer reports, these observations would not confirm the theory as being always true.

For Popper, then, what separates a scientific theory from pseudo-science is that a theory is falsifiable. This is a turning point in the post-positivist philosophy of science, as demarcation before Popper's principle depended mainly on the verification of meaning: That verification was conducted by scientists to ensure that their theories were confirmed by data (Quine, 1951). Popper states that the reason we find regularities in nature is because of a mental habit that makes us jump to conclusions. Therefore, he advises scientists to keep their guard up and be suspicious of continuous confirmations. Additionally, Popper suggests that scientists should design experiments that aim to falsify the theory to the maximum effects. If they are successful in falsifying all or parts of their theories, then they should go back to the drawing board and reformulate a new one. Thus, Popper calls his approach to science a process of conjectures and refutations (Popper 1972).

2.1 No scientific psychological theory can account for all types of behavior

Among the examples used by Popper to illustrate pseudo-science are astrology, Marxism, and psycho-analysis, which cannot be subjected to falsification and thus should be demarcated. Given that TAM is based on a psychological theory, the psycho-analysis example is pertinent for our purposes. Popper deems psycho-analysis to be pseudo-science because it can provide an account for every type of observed behavior, and thus is not amenable to refutation. In fact, Popper observes that the explanation of most types of behavior is what makes the theory attractive to others; this same characteristic is what makes it pseudo-science in Popper's eyes (Popper 1972:75)

> I may illustrate this by two very different examples of human behaviour: that of a man who pushes a child into the water with the intention of drowning it; and that of a man who sacrifices his life in an attempt to save the child. Each of these two cases can be explained with equal ease in Freudian and in Adlerian terms. According to Freud the first man suffered from repression (say, of some component of his Oedipus complex), while the second man had achieved sublimation. According to Adler the first man suffered from feelings of inferiority (producing perhaps the need to prove to himself that he dared to commit some crime), and so did the second man (whose need was to prove to himself that he dared to rescue the child). I could not think of any human behaviour which could not be interpreted in terms of either theory. It was precisely this fact that admirers constituted the strongest argument in favour of these theories.

The similarity, in terms of explanatory power, between psycho-analysis, as explained by Popper, and TRA/TAM is astonishing. For example, in the introduction I cited Davis et al. (1989: 983): "TRA is very general, 'designed to explain virtually any human behavior' (Ajzen and Fishbein, 1980, p.4) and should therefore be appropriate for studying the determinants of computer usage behavior as special case." We can see that the appeal of TRA and TAM is similar to what made psycho-analysis attractive. I can also imagine a couple of examples analogous to Popper's example of psychoanalysis for TAM. Let us imagine the adoption of an Enterprise Resource Planning system and the adoption of a Web board for communication between students and a professor. The latter will be explained in terms of students finding the system useful and easy to use. The former, despite being found by users to be difficult to use and disruptive to their work, will be explained in terms of the subjective norm; that is, the expectation and influence exercised by powerful institutions and authorities.[2]

Again, after Popper, it is very difficult to find a type of computer adoption behavior that cannot be explained by TAM. I once asked this question to a die-hard believer of TAM, and he told me that TAM could not explain emotional or irrational behavior. Thus, I would concede, then, that TAM could not explain how someone can in a moment of rage throw a laptop at somebody else with the purpose of causing harm or showing wrath. Yet I am sure that some TAM researchers could provide an explanation based on ease of use and usefulness; e.g., by saying that the person believed that the throwing of the laptop–because of its weight and its design as a tool of personal productivity–would achieve his objectives.

2 Although the original version of TAM did not include subjective norm, this construct was added to accommodate explanations in which adoption was elucidated in terms of users' beliefs regarding their duties.

To avoid falling in a trap similar to the one offered by psycho-analysis, Popper (1972: 34) provides scientists with the following guidelines:

> Every 'good' scientific theory is a prohibition: it forbids certain things to happen. The more a theory forbids, the better it is…
>
> Every genuine test of a theory is an attempt to falsify it, or to refute it. Testability is falsifiability; but there are degrees of testability: some theories are more testable, more exposed to refutation, than others; they take, as it were, greater risks.

From my point of view, the above discussion has serious implications for TAM researchers if they wish to demonstrate the scientific nature of their work using Popper's criterion. They must not base their scientific claims on hundreds of confirmatory studies. In recalling Popper's earlier example, no matter how many white swans an observer reports, that would not grant the "all swans are white" theory as true. In the spirit of Popper's principle of demarcation, TAM researchers should instead demonstrate when they would be willing to give away their theory; that is, indicate when a technology would not be adopted as the result of beliefs and attitudes toward it. Yet, as will be discussed below, this is an almost impossible task given that beliefs and actions are not contingently linked but are, in fact, logically connected, as will be explained next.

2.2. The Logical Connection Argument

Actions form key concepts in the social and cognitive sciences and are deemed to be intended behaviors (Pavitt 2000). Accordingly, actions have two main components: (a) body movement (behavior), and most importantly, (b) an aim; that is to say that actions have meaning. Rosenberg (1995) holds that actions are composed of desires and beliefs and that both provide actions with their meaning. He illustrates this by the example of a person named Smith carrying an umbrella. Rosenberg says that the action of Smith carrying an umbrella—as a meaningful action—can be explained in terms of Smith *believing* that it is going to rain and having the *desire* of not getting wet. Thus, his reason or intention for carrying an umbrella can be stated in terms of his *beliefs* and *desires*. These give meaning to the action of carrying an umbrella.

It follows then that there cannot be actions without meaning or, equivalently, there cannot be actions without reasons. Smith might also carry an umbrella for different reasons than his belief in imminent rain and his desire to not get wet. He can carry an umbrella because it is part of his attire or because he wants to use it as a weapon. Yet, whatever the case, the carrying of an umbrella will always be linked to intentions or reasons. Identifying them will not predict the action. Instead, they render an action its meaning. Without providing a reason for Smith to carry an umbrella, Smith's behavior would be deemed irrational and, as such, it would not constitute an action (Rosenberg 1995). Hence, actions and reasons are linked by definition. This is what philosophers call *the logical connection argument* (cf. Anscombe 1957; Melden 1961; Wittgenstein 1953; Wright 1971). This, however, entails implausibility of falsifying TRA and TAM.

The *logical connection argument* suggests that *reasons or intentions* cannot predict actions as these are not linked contingently. In philosophy, contingent connections are opposed to necessary or analytic connections. The main difference is that a contingent entity depends on natural processes to occur, while a necessary (analytic) one does not depend on natural processes (Honderich, 1995). In this view, a chemical reaction is a contingent entity, as it requires the conjunction of different natural factors (substances and environmental conditions) to occur. On the other hand, examples of necessary and analytic entities are those that are found in mathematics. For example, stating that a rectangle has four sides is an analytical truth, as it does not depend on any natural phenomena to exist. Thus, it is clear that empirical science, whose main techniques are based on experimentation and observation, deals with contingent entities, while analytical relations are dealt with by subject matters such as mathematics and logic. Therefore, I argue that confirmatory studies of TAM (i.e. studies that apply confirmatory statistical techniques) have to be interpreted with extreme caution, as the relationship between actions, beliefs, and attitudes is analytical. In this sense, it would be hard to justify an empirical study confirming an analytical relation such as validating that rectangles in nature actually have four sides. This argument is developed further below.

The crux of the logical connection argument is that beliefs, attitudes, and intentions cannot be linked causally to actions since they are linked logically; i.e. they are analytical truths. Yet, something similar is done in confirmatory TAM studies that predict the intended use of technologies based on desires and beliefs. The problem here is that intentions, stated in terms of desires and beliefs, constitute only a re-description of the action they are thought to be predicting. The conjunction of actions and reasons, expressed in terms of desires and beliefs, constitutes an analytical truth. Rosenberg (1995: 43-44) expresses it in the following manner:

> ...every explanation of a human action is in fact tantamount to a redescription of the event to be explained. It does not identify other distinct and logically independent events, states, or conditions that determine the action...Thus, desires, beliefs, and actions are logically connected, not contingently connected...Therefore desires, beliefs, and actions are not causally connected by...any single causal law.

The analytic nature of TRA is something that has been observed in the psychology literature. For example Ogden (2003, p 426) provides poignant instances of the analytic nature of the Theory Planned Behavior (TPB), when reviewing items of previous studies:

> Rapaport and Orebell (2000)
> [Perceived behavioral control]: 'even if I wanted, I might not be able to provide practical assistance/emotional support for a parent of mine in need of care within the next twenty years' (p. 314)
> [Intentions]: 'If a parent of mine were in need of care within the next twenty years, I intend to personally provide practical assistance/emotional support' (p. 315)

Ogden observes that the researchers attempted to correlate perceived behavioral control with behavioral intentions, while both were defined almost in the same terms. Hence, as Ogden states, finding a high correlation between these two constructs is not surprising.

The fact is that actions, desires, and beliefs are logically connected, which implies that any causal relation among them cannot be tested empirically and subjected to falsification. No scientist will be able to design a test in which an action lacks meaning. In the case of TAM, I cannot imagine a test in which a technology can be adopted without the user having any beliefs toward it. Thus, TRA and TAM are not falsifiable. Stating a causal link among action, desires, and beliefs can hardly be regarded as scientific (Popper, 1972).

Thus, examining TAM through the lens of Popper's principle of demarcation reveals two challenges. First, TRA–the foundation of TAM and its further versions–attempts to account for every type of behavior, and second, actions, beliefs and desires are linked analytically. As it has been discussed here, these two challenges render TAM to be virtually unfalsifiable. I am not certain whether TAM is pseudo-science, but I can imagine that TAM's claim of being scientific would have raised Popper's eyebrows.

3. TAM as Normal science

Thomas Kuhn's work on the structure of scientific revolutions (Kuhn 1996) is so well known that I will focus only on the concepts that I consider relevant for my argument. Kuhn's main contribution to the philosophy of science was his novel empirically grounded analysis regarding the progress of science (Kuhn 1996). While other post-positivists see the progress of science *in abstracto* in reductionism, by integrating abstract theories into other theories through higher levels of abstractions Kuhn showed that scientific knowledge was socially constructed, negotiated and evolving. Kuhn argued that scientists-through peer influence, learning research skills, and socialization into values and cognitive frames and institutional power-would establish what he called normal science.

Normal Science

Normal science dominates when what Kuhn calls a paradigm or a disciplinary matrix is adopted and followed like a puzzle-solving activity. A paradigm defines and characterizes the subject matter of a discipline through the institutionalization of a fundamental model of the domain and associated knowledge-producing practices (disciplinary matrix). Bechtel (1988, p 52) illustrates Kuhn's conception of a paradigm by drawing on physics and cognitive science: "Kuhn has in mind items like Copernicus' model of the planets revolving around the sun or the theory of physical bodies attracting one another in accord with Newton's laws. In cognitive science, the idea of the mind as an information-processing system could constitute the paradigm." In this sense, TAM constitutes a sort of paradigm given that it provides IS researchers with a model and theory (based on TRA) for studying all types of IS usage and acceptance situations. It also offers ways to formulate research problems, to approach those research problems, and to solve them.

Thus, a disciplinary matrix consists of two parts: (1) a set of exemplars that are deemed to be crucial in showing problem solving and in demonstrating the achievements in the discipline and (2) exemplary theory development. Pavitt (Pavitt 2004, p. 37) observes that both the exemplars and theory development: "...are viewed as models for students to emulate, and a disciplinary matrix, or set of beliefs shared by a community of scientists, numbering as low as in the tens but more likely in the low hundreds, who have been trained through examination of the same set of exemplars and as a consequence tend to perform their work in roughly similar ways."

Let us consider the possibility of TAM as a paradigm. First, we can say that Davis et al (1989) and Davis (1989) are exemplary and critical pieces for establishing TAM as a paradigm in IS research. Not only have these articles been cited widely,[3] but the proposed theory has also become the center of further modifications and adaptations (see Taylor and Todd 1995a; Taylor and Todd 1995b; Venkatesh and Davis 2000, Davis et al. 2007, this volume). Given the number of research articles that apply TAM, we can infer that there is a critical mass of IS researchers being trained on the basics of the TAM and its methods.

A paradigm is also constituted by instruments, methods, and techniques that illustrate how to conduct research within the boundaries of the theory and how to emulate the success of the exemplary pieces of research. TAM exhibits this last condition in the multiple instruments available for collecting data and in the ways researchers use confirmatory statistical techniques. An IS researcher equipped with the theoretical model, the data collection instruments, and the right statistical package (e.g. PLS) is ready to explore the beliefs related to the adoption and usage of almost every technology. It is not difficult to imagine that most Ph.D. programs, especially in North America, in which TAM seems to be one of the dominant research streams[4], will socialize aspiring students to the essence of the model, the data collection instruments, and the "proper" confirmatory statistical techniques.

In times of normal science, research concentrates on confirming the paradigm, and every time there is an anomaly in the theory, the paradigm remains unquestioned. Instead, the responsibility is attributed to other factors, such as the instruments or sample size, or simply, it is argued that the researchers did not apply the methodology appropriately. This is well epitomized in Hu et al.'s (1999, p 106) discussion of the limitations of their TAM study:

> ...operationalization of the constructs included in the research model was basically drawn from prior TAM studies. Despite the reported validity and reliability, the measurements used in the study exhibited relatively low reliability values, particularly those for attitude and perceived ease of use. The observed limited reliability may suggest potential "nonapplicability" of the same instrument to a very different context or group of target users. This responds and reinforces the instance of instrument re-evaluation, as suggested by Straub [63]

Is there a paradigmatic crisis?

In Kuhn's view of science, normal science is the result of the evolution of an incipient immature science. Kuhn observes that eventually normal science will reach a level of crisis in which it will be unable to solve particular anomalies in puzzles within which it operates. As a result, a new group of scientists, most often young scientists, will start working on solving those anomalies and thus move to other space in "puzzle-solving." They will confront the dominant paradigm. Often their results will be dismissed as spurious or equivocal in the start. Ultimately, the pressure of not solving the anomalies becomes so severe that the old paradigm evaporates. In this case, the discipline reaches a crossroad: either it can adopt a new paradigm or it can simply remain in the chaotic state.

The origins of TAM can be tracked back to Davis' Ph.D. thesis that was published in 1986 (Davis 1986). It is not difficult to imagine that TAM's genesis was in the computerization problems of early 1980s. This is an era of transition, from mini computers to personal computers, marked by unfriendly user interfaces and characterized by a lack of useful personal applications. In this context, computer usage and adoption were major concerns for managers and vendors. This is exactly what Davis et al. (1989: 982) state for motivating their classic article: "Computer systems cannot improve organizational performance if they aren't used. Unfortunately, resistance to end-user systems by managers and professionals is a widespread problem. To better predict, explain, and increase user acceptance, we need to better understand why people accept or reject computers." What is noticeable here is not only their positivistic "social-engineering" stance, but their concern with end users not using computer systems.

I do not think it is an exaggeration to credit early TAM studies for influencing system designers to deliver friendlier and more productive systems. However, in light of Kuhn's model, the question is whether TAM currently resolves a puzzling phenomenon such as the one it addressed initially (i.e. workers not using productivity tools). Is the question of wide user acceptance still a challenging one? I think this is a hard question given the widespread and institutionalized use of technology in organizations and households. What are the real issues and challenges now to explain? Is it what happens after adoption? Is it how to convert information into knowledge? Is it the relationship among productivity, strategic advantage, and the use of IT? If TAM is not addressing or answering any puzzling questions, should we be talking about a crisis?

3 A Social Sciences Citation Index search in March 2007 yielded 805 cites for Davis (1989) and 672 cites for Davis et al. (1989).
4 A recent meta-analysis by King and He (2006) showed that European journals are not among the major publication outlets for TAM. Of the European journals, only two papers were published in EJIS and two in ISJ, which is indicative of TAM research not being as predominant in that region.

The readers are most likely aware of the debate regarding a crisis in the IS discipline. Some suggest there is an identity crisis because researchers are not concentrating close enough to the IT artifact (Benbasat and Zmud 2003). Others contend that there is not much attention to the technology (Orlikowski and Iacono 2001),[5] or even that the results of our research are meaningless to managers and professionals (Bennis and O'Toole 2005). I think TAM can be examined through these three debates. It could be that the IT artifact related crisis and the need to theorize on it are a reflection of a dominant paradigm such as TAM that is not answering the current puzzles faced by researchers and practitioners. One of the challenges of TAM is its focus on users' beliefs and statements about their beliefs without a theorization of the object of beliefs-technology. The ontological aspects of technology are completely absent in the model. We have a need to incorporate the ontological components of the technology into the social and psychological dimensions of IS research. This has motivated the proliferation, especially in Europe, of the application of theories such as Actor-Network Theory (see for example Latour, 2004).[6] Regarding the criticism of relevance epitomized in the Bennis and O'Toole article, I can only add that perhaps the commonsensical nature of TAM combined with its statistical data analysis is what those authors called "methodological wizardry," and this is what may render the concerns with this type of research. Perhaps the publication of this special issue on TAM is symptomatic of a crisis.

The Kuhnian lens has made us reflect on the fact that most normal science experiences crisis when its anomalies cannot be resolved. The question here is: Is TAM and related research experiencing a crisis? This is a question that I cannot categorically answer in an essay like this. The historical approach to the philosophy of science assigns the fate of paradigms to the actions of actors and not to the propositional content of their theories. This is one of the key differences between Kuhn and Popper. The latter sees the philosophy of science as eminently rational, while the former sees it as socially constructed. Although these positions seem disparate, it has been argued that, in fact, a historical view of the philosophy of science can be conciliated with a rational perspective. This argument was made by Lakatos (1970, 1978), and it is the perspective that I adopt to analyze TAM in the next section.

4. TAM and Lakatos' Concept of Scientific Research Programs

Lakatos started working with Popper at the London School of Economics in 1960 where he eventually became Popper's successor, though he died at an early stage of his career and before Popper. His work in the philosophy of science constitutes a synthesis of Popper's principle of demarcation based on falsification and the historical perspective established by Kuhn. Lakatos called this endeavor the "Methodology of Scientific Research Programs."

Lakatos' Methodology of Research Programs

According to Lakatos, a research program consists of a set of theoretical assumptions to which a community of scientists is committed. Such theoretical assumptions form what Lakatos calls the core. Committed scientists will defend the credibility of the core against any threats posed by discrepant facts and research propositions. In other words, scientists committed to the research program will not question the assumptions postulated in the core. Also, Lakatos says that scientists within a research program will not attempt to falsify the core. The defense of the core is conducted by the elaboration of auxiliary hypothesis. Lakatos proposed specific terms for the core and auxiliary hypotheses. The latter is called the *protective belt* while the former is the *hard core*. These are also referred to as heuristics: positive heuristic and negative heuristic. The negative heuristic rules are tacit rules that scientists within the research program accept so the hard core is never questioned, while the positive ones allow the modification or addition of auxiliary hypotheses to protect the hard core and modify the protective belt.

Lakatos (1970) provides a vivid example of how this methodology of scientific research programs operates. This example describes a Newtonian scientist who is puzzled by the alteration of a known planet's orbit:

> The story is about an imaginary case of planetary misbehaviour. A physicist of the pre-Einsteinian era takes Newton's mechanics and his law of gravitation, (N), the accepted initial conditions, I, and calculates, with their help, the path of a newly discovered small planet, p. But the planet deviates from the calculated path. Does our Newtonian physicist consider that the deviation was forbidden by Newton's theory and therefore that, once established, it refutes the theory N? No. He suggests that there must be a hitherto unknown planet p' which perturbs the path of p. He calculates the mass, orbit, etc., of this hypothetical planet and then asks an experimental astronomer to test his hypothesis. The planet p' is so small that even the biggest available telescopes cannot possibly observe it: the experimental astronomer applies for a research grant to build yet bigger one. In three years' time the new telescope is ready. Were the unknown planet p' to be discovered, it would be hailed as a new victory of Newtonian science. But it is not. Does our scientist abandon Newton's theory and his idea of the perturbing planet? No. He suggests that a cloud of cosmic dust hides the planet from us. He calculates the location and properties of this cloud and asks for a research

5 Some researchers have argued that it is not proper to speak of a crisis in a multiparadigmatic discipline such as IS. (Ives et al. 2004)
6 ANT is not free of criticism. Its epistemology has been questioned (see for example Collins, 1992) and Latour himself (Latour 1999) has declared that ANT is not a theory, that is open to falsification, but it is a method to describe the relationship between technology and society.

> grant to send up a satellite to test his calculations. Were the satellite's instruments (possibly new ones, based on a little-tested theory) to record the existence of the conjectural cloud, the result would be hailed as an outstanding victory for Newtonian science. But the cloud is not found. Does our scientist abandon Newton's theory, together with the idea of the perturbing planet and the idea of the cloud which hides it No...Either yet another ingenious auxiliary hypothesis is proposed or...the whole story is buried in the dusty volumes of periodicals and the story never mentioned again (pp. 100-101).

In this example we can see that the hard core of the theory is represented by Newton's gravitational law, and the protective belt is the culmination of the auxiliary hypotheses (i.e. the presence of a little planet and the presence of a cosmic cloud that produce the perturbation). The tacit negative heuristic forbids the scientist to question Newton's general law of gravitation, and the positive heuristic motivates him to continue formulating auxiliary hypotheses and modify the protective belt.

In the case of TAM, the hard core would be TRA (i.e. the theoretical assumption that postulates that all actions are caused by beliefs). The auxiliary hypotheses of TAM, its protective belt, would be constituted by the different additions made by researchers committed to this research program to protect the hard core from anomalies. Accordingly, TAM researchers have added new constructs to the model with the purpose of explaining unexpected results. Some of those added constructs are *prior experience* (Taylor and Todd 1995), *gender* (Gefen and Straub 1997; Venkatesh and Morris 2000), and *voluntariness* (Venkatesh and Davis 2000) (for a critical review see Benbasat and Barki 2007, this volume). For example, Ma and Liu (2004, pp 61-62) noticed how, in measuring TAM constructs, the reported results vary from researcher to researcher. This has triggered the addition of more constructs: "In succeeding studies, the measurement items for these constructs varied from researcher to researcher (Adams et al. 1992). As a result, the cumulative number of items for measuring PU [Perceived Usefulness] has increased from the original six to currently about 50, and that for PEOU [Perceived Ease of Use] has increased from six to 38....upon closer scrutiny of the list, we found that the differences in measurement items between studies tend to be the result of adapting TAM to different technologies."

Likewise, several TAM researchers (Gefen and Straub, 2000; Ma and Liu, 2004; Taylor and Todd, 1995b; Venkatesh and Davis, 2000) have noticed that PEOU has not been found to be consistently linked to adoption. To illustrate these anomalies, Gefen and Straub (2000) formulated a summary of such inconsistencies. These authors explain the anomalies (via auxiliary hypotheses) by suggesting that the role of PEOU depends on the task.

In addition, Lakatos (1970: 176) discourages researchers from accommodating empirical content in their research program through statistical techniques; he calls this practice a:

> ...patched-up, unimaginative series of pedestrian 'empirical' adjustments which are so frequent, for instance, in modern social psychology. Such adjustments may, with the help of so-called statistical techniques', make some 'novel' predictions and may even conjure up some irrelevant grains of truth in them. But this theorizing has no unifying idea, no heuristic power, no continuity. They do not add up to a genuine research programme and are, on the whole, worthless.

Lakatos continues warning researchers to not concentrate on statistical significance to claim scientific contributions; he does this by citing Lykken (1968):

> 'Statistical significance [in psychology] is perhaps the least important attribute of a good experiment; it is never a sufficient condition for claiming that a theory has been usefully corroborated, that a meaningful empirical fact has been established, or that an experimental report ought to be published'...

Lakatos finishes this observation by indicating that the methodology of research programmes may help researchers to avoid ad-hoc theorizing.

I find that, although a bit dramatic in its tone, Lakatos' advice is relevant given the prevalence of statistical techniques to conduct confirmatory studies of TAM. Often in TAM studies, the claim to a scientific contribution is made by reference to a high explained variance. More recently, in referring to this practice, Bennis and O'Toole (2005: 99) suggests,: "When applied to business--essentially a human activity in which judgments are made with messy, incomplete, and incoherent data--statistical and methodological wizardry can blind rather than illuminate." Of course, I would not like to chastise these techniques, but I consider Lakatos' observation as pertinent not only for TAM researchers, but also for IS researchers, in general, who are interested in reflecting on the scientific nature of their endeavors.

In light of Lakatos' methodology of scientific research programs, I argue that the complementary constructs and additional theoretical explanations were added by TAM researchers to protect the hard core. These additions can be considered auxiliary hypotheses that have been incorporated in the protective belt. In this sense, it is also worth mentioning that in my reading of the TAM literature, I could not find papers that challenged the hard core. I argue that this is the result of the application of the negative heuristic.

Lakatos' Principle of Demarcation

In addition to the already discussed concepts, Lakatos' methodology provides a principle of demarcation. Unlike Popper, Lakatos did not differentiate between science and pseudo science; instead, he differentiated between degenerative research programs and progressive research programs. For example, Lakatos considered Newton's theory of gravitation, Einstein's relativity theory, quantum mechanics, Marxism, and psychoanalysis as instances of research programmes. They are composed of a hard core of assumptions protected from "anomalies" by a protective belt of auxiliary hypotheses. In this sense, both Marxists and Newtonians would be able to explain phenomena and accommodate anomalies that seem to threaten the stability of the hard core. So the question here is: are all research programmes of the same scientific quality? Lakatos' answer would be a categorical no. The difference between a degenerative and a progressive research program, Lakatos (1978: 5) states, is that: "…in a progressive research programme, theory leads to the discovery of hitherto unknown novel facts. In degenerating programmes, however, theories are fabricated only in order to accommodate known facts."

Lakatos (1978) argues that Newtonians have been more successful than Marxists on at least one dimension: the ability to predict novel facts. Using Newton's theories, Halley predicted that a comet would re-appear in seventy-two years. He made this prediction after observing only a brief stretch of the comet's path. Seventy-two years later, the comet reappeared at the exact time that Halley had predicted. Similarly, Newtonian scientists predicted the existence of small planets that were unobservable at the time. The existence of these planets was empirically confirmed later. Marxism, on the other hand, has never predicted a novel fact successfully. Marxism predicted the absolute impoverishment of the working class; it predicted that the first socialist revolution would take place in the most industrially developed country; it predicted that socialist societies would be free of revolutions; and it also predicted that socialist countries would not have conflicts.

Lakatos notes that none of those predictions were fulfilled. Yet, with the help of auxiliary hypotheses, Marxism was able to accommodate these abnormalities. For example, Marxists explained the rising living standards of the working class in capitalist societies by devising an auxiliary hypothesis of imperialism. Marxists also explained why the first revolution occurred in industrially under-developed Russia. Marxists explained the Russian-Chinese, the Budapest (1956) and the Prague (1968) conflicts. However, all these accommodations were made after the fact. According to Lakatos (1978), retrospective accommodation of known facts does not qualify a research programme as progressive. "What really counts are dramatic, unexpected, stunning predictions: a few of them are enough to tilt the balance; where theory lags behind facts, we are dealing with miserably degenerating research programmes" (Lakatos 1978, p. 6).

If we were to apply this principle of demarcation to TAM, we will see how its researchers have proceeded when facing anomalies. In the previous section I presented TAM researchers who, when confronted with anomalies, proposed a revision of the instruments (i.e. a revision of the auxiliary hypotheses). I also discussed researchers who, when faced with unexpected results, opted for adding new constructs to the model; this is yet another example of adding auxiliary hypotheses to protect the hard core. Thus, we can conclude that TAM constitutes a research program. Whether this research programme is progressive or degenerative would depend on if TAM can predict novel, bold facts. For this purpose, I will leave it to the reader's judgment whether predicting that a user will adopt a technology that they find easy to use and useful constitutes a bold prediction or merely represents the accommodation of a known fact.

Conclusions

It is fair to assume that TAM researchers may not be amused when someone questions the scientific legitimacy of their theory using the post-positivist criteria. Most questions these philosophers ask are not aimed to create comfort among scientists-quite the opposite. I believe that the questions a philosophy of science leads us to ask about a model like TAM are fruitful, as they help us to reflect critically our endeavors. I believe that being uncritical about one's work as a researcher is a sure step to stagnation.

Accordingly, I have examined TAM through the lens of the post positivist philosophy of science with the purpose of providing a constructive critique. Specifically, I have discussed TAM from the perspective of the works of three prominent post-positivist philosophers of science: Karl Popper, Thomas Kuhn, and Imre Lakatos. These should be appropriate yardsticks for evaluation, as all of them have focused on sciences that aim for causal explanation and assume rational behaviors around explanation-a belief that TAM scholars also share. Given the significance of TAM in our field, a discussion such as that presented here should be beneficial because it helps us to critically analyze TAM from three critical perspectives: Is it falsifiable?, Is it normal science? Is it moving toward crisis? And, how does it fit with the methodology of scientific research programs and is it a progressive or degenerative program? In this section, I will review the main points that have emerged from our discussion. It is pertinent to say that I believe that the lessons learned can also be of benefit to IS researchers working on other research programs.

Applying Popper's principle of demarcation draws our attention to the falsifiable aspects of TAM. The major challenge I found is the claim made by early TAM researchers regarding the explanatory power of their model: TRA is powerful enough to explain all types of conduct. Popper strongly opposed such statements and even associated them with what he called pseudo-science. I also noted that TAM faces difficulties in falsifying its main theoretical postulates, since beliefs are logically connected to intended actions. The relationship between beliefs and actions is not contingent, but is instead an analytic one. Thus, TAM researchers should stipulate the falsification provisos of their main theoretical assumptions and consider alternative research designs that can falsify the core theory rather than continually confirm it.

I believe that TAM has provided us with valuable insights, such as the relevance of designing user friendly interfaces and emphasizing the value of systems in terms of their productivity and applicability. In discussing TAM from the perspective of Kuhn's work, I reflected that Davis' observation relating the constructs *ease of use* and *usefulness* to that of *usage behavior* was an early valuable observation. It was relevant given the technological progress and diffusion at the time the model was formulated in the mid 1980s. Yet, from the same perspective of normal science, we learned that the major challenges faced by IS researchers and practitioners today may no longer deal with behaviors such as initial acceptance.

Lakatos' methodology of scientific research programs provided us a poignant analysis of TAM. The main components of the TAM "research program" were identified-hard core, protective belt, and the negative/positive heuristics. By drawing on the literature, I showed that anomalies in TAM have been resolved by resorting to the instruments or by adding new constructs, i.e., modifying the protective belt with positive heuristics. I did not find any inquires to the suitability of the hard core of the theory. The resemblance between the type of research conducted by TAM researchers and what Lakatos' proposes in his methodology is startling. More importantly, I applied Lakatos principle of demarcation: that progressive research programs should predict bold facts. In this sense, Lakatos also mentions that contributions to the protective belt–in terms of new facts or auxiliary hypotheses-can be progressive only as long as those contributions represent additions to the research program. The latter, i.e., contributions to the auxiliary belt, was illustrated in Lakatos' example of the Newtonian scientist. Yet, given the subjective nature of the terms "bold" and "incremental" I leave the final judgment on whether TAM has provided these predictions to the reader.

There are some concrete lessons for IS researchers that we can derive from the reflection presented in this essay. From Popper's principle of falsifiability, we learned that a good theory should "prohibit" the occurrence of specific phenomena. In this sense, we should always be worried when a theory claims to explain almost all types of behavior. Moreover, we should not consider confirmatory studies as conclusive evidence for corroborating a theory. In this sense, Popper recommends, rather paradoxically, that the surest way to strengthen a theory is through attempts to falsify it through bold experiments. In addition, the Kuhnian lens helped to see TAM as a typical example of normal science, as it offers a complete puzzle-solving apparatus that is easily transferable and verifiable, so it gradually became a legitimate way of conducting research in IS.

I believe the discussion from the point of view of normal science offers an interesting angle to the discussion regarding the legitimacy of IS as a discipline (Weber 2006; Lyytinen and King 2004 and 2006). In the context of this debate, I understand legitimacy as a manifestation of progress in our field. Accordingly, it is my view that our field is going to advance by the solving of puzzles deemed to be relevant not only by IS researchers but also by practitioners. Hence, based on Lakatos' principle of demarcation, what is going to establish our discipline is not the application of rigorous statistical techniques but the proposition of theories that explain intriguing phenomena in bold ways. Moreover, reflecting on TAM from the point of view of the post positivist philosophy of science has pointed out that progress in our field will be achieved neither by attempting to confirm theories nor by accommodating already known facts. Progress will be achieved when we are able to propose theories that solve phenomena that neither practitioners nor researchers have been able to fully explain; in other words when we establish progressive research programs in the Lakatosian sense.

In conclusion, I would like to highlight the value of critical discussions in our field. Quine (1951), in his classic paper "The Two Dogmas of Empiricism," discusses how we learn about the world. He suggests that almost any sensorial experience or language proposition can cause us to question our most certain beliefs. He gives as an example a simple statement such as: "This is a brick house." If we were to have the time and energy, we could go on and conclude that such a statement, even though it seems a plain fact, would question fundamental theories about ontology (what is a brick?) and semantics (what does the word house mean?). The reason we do not proceed to reflect on those matters, says Quine, is pragmatic; social life would be paralyzed by such perennial questions. However, the IS community has not only the right, but also the duty to question the scientific and philosophical foundations of our field including the epistemology of TAM. A reflection on TAM from the point of view of the philosophy of science can provide us with a broader perspective on how to evaluate our endeavors in any historical context. Such an exercise certainly will show some things that will bother and irritate us. However, although most self-examinations in our life initially scare us, they are beneficial for our own intellectual growth and maturity. Hence the spirit of a paper like this-to engage in self reflection and self critique-is warranted so that we can advance our field.

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