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Theorizing in information systems research: A reflexive analysis of the adaptation of theory in information systems research ¹

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Abstract.

In this paper we consider what it means to be an informed IS researcher by focusing attention on theory adaptation in IS research. The basic question we seek to address is: "When one borrows theory from another discipline, what are the issues that one must consider?" After examining the role of theory in IS research, we focus on escalation theory applied to IS projects as an example. In doing so, we seek to generate increased awareness of the issues that one might consider when adapting theories from other domains to research in our field. This increased awareness may then translate to a more informed use of theories in IS. Following a self-reflexive tale of how and why escalation theory was adopted to IS research, we offer four recommendations for theory adaptation: (1) consider the fit between selected theory and phenomenon of interest, (2) consider the theory's historical context, (3) consider how the theory impacts the choice of research method, and (4) consider the contribution of theorizing to cumulative theory.

Keywords: Theorizing, Escalation Theory, Theory Adaptation

¹ Detmar Straub was the accepting senior editor. Shirley Gregor was the invited senior editor. This paper was submitted on 29th January 2005 and went through two rounds of revision.

Introduction

Among the more controversial issues in the information systems (IS) field are whether we, as researchers, *should* continue to borrow theories originating in our reference disciplines, how we should appropriately use those theories once they are borrowed, and, once used, if we have an obligation to contribute something additive back to the reference disciplines from which we borrow. Some would argue that IS researchers should be free to borrow 'as-is' whatever theories they can from other areas, so long as they help to inform our own understanding. Those who subscribe to this view would not feel under any obligation to make a contribution back to the reference discipline in terms of theory development. At the other end of the debate, there are those who feel that it can be dangerous to apply theories from outside IS as-is and that IS researchers have an obligation not only to carefully select theories that can be adapted to the context at hand, but also to contribute back to the reference discipline in terms of theory development. This controversial issue raises the broader questions of: (1) When is it appropriate to draw upon theories from other areas? (2) How might IS researchers best accomplish this? and (3) How can IS researchers begin to "give back" rather than simply "take" in their efforts to theorize? The aim for this issues and opinions paper is to bring fresh insights on the important topic of how IS researchers should appropriate theories from other disciplines.

The IS discipline has evolved with relatively permeable research boundaries. The diversity in theoretical underpinnings has not only been healthy, but also essential to our disciplinary evolution. However, a discourse has recently emerged in our field in which it is argued that intellectual shortcomings are haunting our field more than other fields (Lyytinen and King, 2004). The relative lack of core theories is frequently discussed as a key problem. We agree with Lyytinen and King that better theory is likely to contribute to stronger results (Lyytinen and King, 2004). But how do we accomplish better theory? To move the discussion forward from the debate around whether or not theory should be borrowed, this paper's focus is not on if or why theory should be borrowed, but rather on how borrowing might best be done. This paper offers a set of recommendations to IS researchers on the issues that one must consider when one borrows theory from another discipline. Adapting theory is a much trickier endeavor than one might first assume. A poorly informed adaptation risks: repeating mistakes made, debated, and dealt within the original disciplines' discourse; misinterpreting the underlying notions about the nature of reality and how knowledge is acquired that are implicit in the theory and the methodological implications those assumptions imply; and wasting time and effort by not adding value to the cumulative tradition in our own field. It is therefore our position that we need to be more reflexive about the ways in which we adapt theories to our field and to deepen our understanding about how and why any theory is adapted.

In two MISQ editorials, Weber called for improved theory-building skills by reflexive researchers (Weber, 2003a; Weber, 2003b). In arguing that our field relies too heavily on theories borrowed and adapted from other disciplines, Weber called for an increased awareness of the role of theory in our research. We agree that researchers need to be more aware of the role of theory, but disagree with the notion that importing theories is an indication of weakness in our discipline. Rather, we see the problem as being related to *the manner in which theories are borrowed* by IS researchers. Theories will continue to be borrowed by IS researchers. We believe that the solution Weber seeks lies not in ceasing to borrow theories from other disciplines, but in borrowing them in a more

*reflexive manner.*²

As Weber (2003a, p. vii) notes:

“An important skill we need to develop as researchers is an ability to reflect on, to understand, to evaluate and to see the interrelationships among the deep assumptions that underlie our work . . . [But, this] is a skill that does not come easily. We first need to acquire knowledge that is both broad and deep—knowledge that allows us to understand paradoxically what we know and what we don’t know. We then need to have the discipline and courage to stare at the underbelly of our research—to scrutinize it ruthlessly so we can learn more about our subject matter, the strengths and limitations of our research, and more broadly ourselves as researchers and our place within a community of scholars. Being able to reflect deeply on our research is a skill that is difficult to master and sustain. We must hone it assiduously throughout our careers.”

In keeping with Weber’s call, we advocate greater critical awareness of the underlying assumptions implied by the use of particular theories in the IS field. The relative immaturity of the IS field has led to the “borrowing” of a number of theoretical approaches from other subject areas, often with little regard for the associated baggage of underlying assumptions.

We acknowledge that there have been many instances in which a theory developed in one discipline has been successfully adapted to IS research. Theories borrowed from other disciplines have favorably impacted IS research and, in one sense, this paper is inspired by such successful examples. But this paper is also motivated by our concern over the negative impact that uninformed borrowing of external theories has on our field. By uninformed borrowing, we mean the temptation to adapt and use the bits of a theory that seem applicable to the task at hand without having understood and considered the limits and problems that may also be associated with that theory. This paper is also motivated more than anything else by our own struggles with borrowing theory.

This paper proceeds as follows: In section two, we discuss the role of theories and the importance of informed theorizing in IS research. In section three we apply a self-reflexive analysis of borrowing theory from the domain of escalation theory. In so doing, we refine and further develop the principles for borrowing theory successfully.

² By reflexive, we mean the researcher’s awareness of his/her own presence in the research process and in the shaping of the research. As researchers, our choice of questions, our use of methods, and so forth, influence what we ultimately claim to ‘find’. Reflexive research has two basic characteristics: careful interpretation and reflection. Reflexivity itself has two aspects: The first is the fact that the researcher is part of the setting, context, and social phenomenon being studied; and the second is “[a] process of self reflection of one’s biases, theoretical predispositions, preferences and so forth . . .”, Barry, C. A., *et al.* [1999], pp. 26-44.

The role of theory in IS research

IS research has long been characterized as a 'fragmented adhocracy' (Banville and Landry, 1989) in that it is not dominated by a single methodological or theoretical paradigm but rather by pluralism in both method and theory. While this characteristic may be perceived as the most pervasive weakness or as the key strength in the discipline depending on one's perspective (for a discussion, see Robey (1996), it is clear that our field is still young and relatively inter-disciplinary. As such, IS researchers still reach out to other disciplines for theory (Lee et al.; 1996).

Numerous characteristics come to mind when we consider what it means to be an informed IS researcher. But among those we consider most important is the manner in which informed IS researchers are able to answer the question: Why did you favor a certain theory versus another? In our own experience we have witnessed far too many instances in which the researcher has been unable to formulate a reasonable reply to that question, either when choosing a theory native to IS or, even more frequently, when borrowing a theory from another discipline. So we have focused on the question: When one borrows theory from another discipline, what are the issues that one must consider, and what does the theory mean for the IS discipline?

In her examination of the structural nature of theory in information systems, Gregor (2006) deals with the issue of what theory means in information system research. She distinguishes between five interrelated types of theory: (i) theory for analyzing; (ii) theory for explaining, (iii) theory for predicting; (iv) theory for explaining and predicting; and (v) theory for design and action. These types are all very different but share this in common: Research projects always begin with a problem or question of interest. Whether the questions themselves are worth asking can only be considered against the state of knowledge in the field at the time. In other words, all five types of theory have in common that they are used as a means of advancing the state of knowledge in a given field – to add to cumulative theory. We accept any of Gregor's distinctions and for the purposes of this paper include them in our generic use of the term 'theory'.

What is the point of having or using a theory? Simply stated, theory guides the process of making sense of complicated and often contradictory real-world phenomena. Theory acts as a lens through which we focus and magnify certain things, while filtering out others things presumed to be "noise." In our view a researcher always approaches a topic from some theoretical point of view; however, this may or may not be explicit. If it remains an implicit and unstated set of assumptions it may actually inhibit the study of the phenomena at hand. As Weick (1985) observes:

“...implicit theories impede understanding; they act as blind spots... Because believing is often seeing, implicit theories become undeliberated assumptions, which are imposed and appear to be self-confirming. People see what they expect to see” (Weick, 1985, p. 113).

Theories thus affect what we see and what we don't see. But as a research field it seems that our focus of attention varies over time as theories come in and go out of fashion (Jones, 2000). In the field of management research, Barley and Kunda (1992) illustrate how there are cyclical ebbs and flows of theory use. A key point of their work is that as the field incorporates each theoretical 'surge' into the mainstream, it also folds many of the assumptions of all previous theories into the theoretical *doxa* (commonly

held beliefs and understandings) of the discipline. Thus, one challenge a new theory presents to a research community is that of assessing the theory's contribution to the cumulative tradition and then folding this new contribution into that historical context. New theories also challenge the researcher who must invest time and effort to understand the theory in its native environment; to learn the vocabulary and underlying assumptions of the theory; to understand its weaknesses as well as its strengths, and to acknowledge its previous use, applicability, and challenges to its veracity. Hence, because of the substantial learning curve required to become well-versed in a theoretical tradition, it becomes impractical for a researcher to conduct work in multiple theoretical traditions with equal assurance and competence.

What is the problem with an IS researcher flitting from theoretical flower to theoretical flower like an itinerate butterfly? We see two problems. First, to faithfully 'borrow' and use a theory, a researcher must become inculcated into the internal logic and intellectual tradition associated with the theory. Any nonreflexive and partially informed use of a theory can lead to the inheritance of problems already identified in the originating discipline. Thus, as Walsham reminds us, "Any new theory which receives significant attention tends also to attract criticism... It is important that IS researchers who are thinking of using the theory should be aware of these criticisms, and should thus be able to generate an informed view of the usefulness and limitations of the theory in their own work" (Walsham, 1997).

The second problem is that adopting a theory is a costly exercise when it is taken seriously. Thus it is our contention that it is not sufficient to simply 'use' a theory. Rather, the process of moving a theory from one research domain to another is a process of deep reflection, before possible adaptation and development of that theory.

While it is important for IS researchers to make use of theoretical advances in other disciplines (Banville and Landry, 1989; Baskerville and Myers, 2002; Hirschheim and Klein, 1989; Robey and Zmud, 1992) this should be done considering the cumulative theory development in IS research too. (Hirschheim and Klein, 1989; Klein and Myers, 1999; Klein and Lyytinen, 1985). Or, as Weick puts it, to know if what one is putting forth is a theory, you have to put it in the "context of what came before and what comes next" (Weick, 1995, 389). To illustrate the notion of 'cumulative theory', we use the analogy of an ongoing conversation within members of any community, wherein the thread might be interrupted and picked up again some time later (see, for example, Damsgaard and Truex (2000) and Whetten and Godfrey (1998)).

Robey and Zmud (1992) suggest that: "Theory should be a tool, and the only requirement for its successful use is the ability to see the parallels between theoretical constructs and real problems" (Robey and Zmud, 1992, 25). Our position differs. For us, theories are not just "tools" that are used without being changed; in fact, theory development (both through theory building or theory challenging and refinement through theory testing) is a primary research outcome or goal. Such development can depend on the generalization that Yin labels as an "analytical generalization," where the researcher "is striving to generalize a particular set of results to some broader theory" (Yin, 1994, 36).

We believe that it is both appropriate for our field's development as well as inevitable that theories from other fields will be borrowed and applied in the IS research arena. In this paper we posit, based on our own experience, four general guidelines or

recommendations that researchers might consider in borrowing theory from another domain. These same guidelines should also be beneficial to reviewers in evaluating the quality of the work they are reviewing. The guidelines we developed were then tested against work that has been judged successful in making such a transition. In a self-critical manner, we tested the process of that transition *post hoc* against the principles and in the process, refined and extended the guidelines.

The proposed recommendations follow. First, a researcher should *consider the fit between the theory and the phenomenon of interest*; close attention should be paid to how well suited the theory is to explaining and interpreting the object of study. Second, a researcher must *consider the theory's historical context*, and be informed about previous use of the theory. All theoretical constructs are bearers of some key assumptions, making them relevant for some given contexts, but not necessarily for all contexts. Third, one should *consider how theory impacts the choice of method*. Fourth, one should *consider the theorizing process' contribution to cumulative theory*.³ Because theory impacts research from its inception, to its conduct (method) and finally to the interpretations of research outcomes, we further argue that appropriation of theory from another domain should be done reflexively.

In the following section, we will apply these criteria by exploring the ways in which escalation theory was adapted to IS research. Echoing Weber's [2003a] recent call for a deepened reflexivity among IS researchers, we will explore in some detail how and why escalation theory came to be an accepted part of IS research.

Escalation theory applied to IS projects as example

In this section we discuss Mark Keil's experience of borrowing constructs from escalation theory to better understand and explain software project failures and reflect on his approach. We focus on Keil's work not so much as a test of our recommendations, but rather to illustrate how one researcher grappled with the issues associated with appropriating theory from one domain and applying it to another domain. Through this process of engaged analysis, we have had the opportunity to refine our recommendations. Before launching into how Keil dealt with these issues, we provide a brief description of the origins of the theory in question, escalation theory. We describe how it came to be appropriated by information systems (IS) researchers, and the various ways in which Keil and colleagues have contributed to the discourse.⁴

Escalation of commitment refers to the human tendency to continue a previously chosen course of action in spite of negative feedback concerning the viability of that course of action. What is commonly called "escalation theory" can be traced back to several different streams of experimentation exploring how individuals, groups, and

³ We do not claim that this is an exhaustive set of criteria. We have however, attempted to put forward a parsimonious set of criteria that at a minimum should be considered when one considers borrowing theory.

⁴ The approach we have taken, while unusual is neither wholly unique nor unknown. It is akin to both the anthropological approach of the confessional narrative and to forms of action research in which the subjects are active members of the research team. In both instances the accounts are highly personal, self-reflexive, and focus on learning. In the present case we have chosen to provide our account of Keil's work using three rhetorical vehicles: 1) a description of his work and the evolution of the published ideas using borrowed theories, 2) an interview addressing specific issues and questions about the nature of that process, and finally, 3) a reflexive account of the process by Keil himself.

organizations become entrapped in what appear to be failing courses of action. Staw's seminal work "Knee Deep in the Big Muddy" [Staw, 1976] examined the extent to which personal responsibility for a previously chosen course affects escalation. Seeking a theoretical explanation, Staw developed "self-justification theory," which suggests that decision makers responsible for initiating a failing course of action will escalate their commitment in order to avoid cognitive dissonance. About the same time, Brockner and his colleagues sought to examine a similar phenomenon that they labeled "entrapment." Teger investigated the phenomenon as well, observing escalation in the context of dollar bill auctions in which people demonstrated a willingness to continue raising their bids after it was clear that they would lose money even if they "won" the auction [Teger, 1980]. From this beginning, further studies on escalation followed. Not until the mid-1990s, however, did IS researchers begin to appropriate escalation theory.

For many years, both IS researchers and practitioners had bemoaned the high failure rates associated with information technology (IT) projects, and the popular press continues to turn out stories of so-called "runaway projects" with great regularity. While prior research had been conducted on IT project failures and the factors that can threaten successful implementation of information systems, there had been little or no attention placed on developing or applying theory-based explanations of how troubled projects could escalate to become "runaway systems" or, for that matter, how troubled projects could be successfully turned around if possible, or abandoned if necessary. This situation changed in the mid-1990s, when IS researchers began turning to escalation theory to help explain the phenomenon of runaway systems projects. Several researchers have applied escalation theory to the study of IT projects, including Keil and his colleagues (Keil, 1995; Keil and Flatto, 1999; Keil et al., 2000a; Keil et al., 1995a; Keil and Montealegre, 2000; Keil et al., 2003; Keil and Robey, 1999; Keil et al., 2000b; Keil et al., 1995b), Sabherwal and his colleagues (Newman and Sabherwal, 1996), (Sabherwal et al., 2003), Drummond (1996; 1998), and Heng et al. (2003). In this paper, we focus on the work of Keil and his colleagues, as it represents the earliest and largest application of escalation theory within the context of information systems. We provide a brief overview of how he came to consider the theory, the intellectual journey required to understand and adapt the theory to his own work, and his efforts to further develop the theory.

In his doctoral dissertation research, Keil witnessed a software project that seemed to "take on a life of its own," absorbing valuable resources without ever reaching its objective. His first article on IT project escalation (Keil, 1995) provided field-based evidence of escalation in an IT project context, while illuminating the factors that led to both escalation and de-escalation. This article showed that some of the factors previously identified in the escalation literature were indeed present in this case. By mapping the factors that promoted escalation back to those discussed in the escalation literature, the paper also identified new factors that may promote escalation, thus contributing to the reference discipline.

Later, Keil and his colleagues conducted a large-scale survey to: (1) understand the extent to which IS projects are prone to escalate, (2) compare the outcomes of projects that escalate with those that do not, and (3) test whether constructs associated with different theories of escalation could be used to discriminate between projects that escalate and those that do not. (Keil et al., 2000a). More recently, Keil and his colleagues have examined the same dataset using a set of project management

constructs (Keil et al., 2003). The results suggest that project management constructs offer an even more promising avenue for identifying projects that are likely to escalate.

In parallel with the case-based and the survey research described above, Keil and colleagues have conducted a series of laboratory experiments with human subjects aimed at gaining a better understanding of the factors that promote software project escalation. Much of this work was based on replicating and then extending the work of Howard Garland and others who have examined the so-called sunk cost effect and how this can lead to escalation behavior. Several articles grew out of this work, including Keil et al., (1995a) and Keil et al., (1995b).

The sunk cost line of experimentation was subsequently extended both theoretically and culturally [Keil et al., 2000a]. By incorporating concepts from risk-taking theory (e.g., risk propensity and risk perception) and carrying out matching laboratory experiments in three cultures (Finland, The Netherlands, and Singapore), the authors were able to explain a greater amount of the variance in decision-makers' willingness to continue a project than in previous studies. Their results also suggested that some factors behind decision-makers' willingness to continue a project were consistent across cultures, while others might be culture-sensitive.

After conducting these studies, Keil shifted his attention from escalation to de-escalation. In a study involving interviews with 42 IS auditors (Keil and Robey, 1999) both quantitative and qualitative data were collected concerning the de-escalation of commitment to troubled software projects. The interviews sought judgments about the importance of 12 specific factors derived from a review of the literature on de-escalation. The researchers found that seven of these factors exhibited statistically significant differences (in paired t-tests) as projects moved from escalation to de-escalation. Their interviews also generated insights about specific actors and actions taken to turn troubled projects around.

Montealegre and Keil (2000) developed a process model of de-escalation was developed from a longitudinal case study conducted of the IT-based baggage handling system at Denver International Airport (DIA). The model revealed de-escalation to be a four-phase process: (1) problem recognition, (2) re-examination of prior course of action, (3) search for alternative course of action, and (4) implementing an exit strategy. For each phase of the model, the researchers identified key activities that seemed to enable de-escalation to move forward. A companion paper that applied the model to another well-known case of escalation—the Taurus system at the London Stock Exchange (Keil and Montealegre, 2000) demonstrated the generalizability of the model.

Along with the studies described above, Keil and his colleagues began conceptual and empirical work relating the theory of real options to the escalation phenomenon (Keil and Flatto, 1999; Tiwana, Keil, and Fichman, 2006). The real options perspective offers new theoretical insights that challenge the traditional assumptions and yet complement existing theories regarding escalation behavior. With colleagues, Keil has also explored the application of actor-network theory (ANT) to the problem of escalation. Exploring the escalation of commitment surrounding the computerized baggage-handling system at the Denver International Airport from an ANT perspective provided new and different insights relative to a comparative analysis drawing on escalation theory (Mähring et al., 2004).

Finally, Keil and colleagues examined the reluctance to transmit bad news (the so-called “mum effect”) and how this relates to the escalation and de-escalation of troubled software projects (Keil and Robey, 2001; Smith et al., 2001; Tan et al., 2003). Taken as a whole, Keil’s experiences in applying escalation theory to the problem of IT project failure provide a rich context in which to illustrate and discuss the four general guidelines that researchers might consider in appropriating and adapting theory from outside the IS domain.

Consider fit between selected theory and phenomenon of interest

The question of theory ‘fit’ as it relates to a given study is often pragmatic and closely associated with discussion of research methodology. Some would argue that the nature of the research problem should determine the choice of research methodology (Trauth, 2001). Karl Weick related theory, problem, and method in a keynote address to an IS research gathering at Harvard in 1985 (Weick, 1985). Trauth (2001, pg. 6) considers the question of ‘fit’ among a research problem, theoretical lens, and research method choices. Her recommendations for choice of research method explicitly consider the ‘skill set’ of the researcher, where ‘skill set’ includes a researcher’s methodological and theoretical experience and competencies. If we take the position that the goal of research is to develop ‘good theory,’ then we should consider whether any proposed research is likely to develop, extend or test existing theory significantly, or if it may develop new and novel theory. Thus, one aspect of the notion of ‘fit’ is how well the theory appears to fit the facts. If there is already well-developed theory that appears to provide a good explanation (i.e., a very good fit) for the phenomena, then the significance of any proposed work might be in doubt unless it can be shown to provide a superior fit.

We draw upon Keil’s experience to illustrate the need for goodness of fit between the theory and the phenomenon of interest. In his doctoral dissertation research, Keil witnessed a software project that seemed to “take on a life of its own,” continuing to absorb valuable resources without ever reaching its objective, but he only later discovered escalation theory. He recalls how serendipity played a role in his search for a theory to explain what he saw:

Throughout my dissertation, I wondered how a company could continue to devote millions of dollars to a project that was clearly off-track and not likely to be successful. Of course, at the time of my dissertation, I did not have the theoretical tools to address this question. I wasn’t familiar with the escalation literature until one day in January 1992 when I was browsing the shelves of the periodicals at Georgia State’s library. I distinctly remember picking up an issue of the Academy of Management Review (AMR) and scanning the table of contents and seeing Joel Brockner’s 1992 article entitled: “Escalation of Commitment to a Failing Course of Action: Towards Theoretical Progress”. At that very moment, it was like a light bulb went off in my head, because I immediately saw a linkage between escalation theory and the phenomenon I had observed in my dissertation research.

By drawing upon the escalation of commitment literature, he was able to borrow theory from an established reference discipline and use it as a means of examining runaway software projects, providing both researchers and practitioners with a deeper understanding of this phenomenon.

In his dissertation research, Keil was prepared to study the re-implementation of a new and improved expert support system (CONFIG). With this context in mind, he was guided by concepts and theory from diffusion of innovation and the technology acceptance model, equipped to measure such constructs as usefulness and ease-of-use. When he got to the field, he was struck by the fact that the company seemed to be overly committed to a system that had failed to gain acceptance in the past and that was not likely to succeed under the present course of action. Yet, the theoretical tools he brought to the field did not allow him to explain this interesting phenomenon that presented itself. As Keil recalls:

I could not understand why this project was allowed to continue. The constructs and theories I brought with me to the field did not shed any real light on this, so the question continued to nag at me. However, in picking up the escalation angle in 1992, I was able to go back to my dissertation data, gather some additional data, and reinterpret the case through the lens of escalation theory.

With escalation theory, Keil obtained a goodness of fit between theory and the phenomenon of interest to him. In his words:

I think my own story illustrates the need for establishing the right “fit” between the research question and the theory that is used to explore the research question. I could not have begun to understand why this project was allowed to continue for as long as it did without the right guiding theory. I needed a theory that fit the question at hand and escalation theory fit like a glove.

Thus, it would seem as if the research problem came first for Keil, but he sought and found a proper approach with which to explore this problem once he came across escalation theory.

Consider the selected theory’s historical context

It is important to consider the selected theory’s historical context and its ‘main thrust’ because ignoring debates and controversies raised concerning the use of a specific theory in other disciplinary contexts can lead to repeating the same mistakes in our own discipline. Such a view highlights the importance of the ontological and epistemological dimensions of theory. In this paper we adopt Gregor’s (2006) understanding of ontology as a framework for talking about the nature and components of theory. Key ontological questions are: What is theory? How is this term understood in the discipline? and Of what is theory composed? Epistemological questions, on the other hand, focus on issues like: How is theory constructed? and How can scientific knowledge be acquired? To recognize the ontological and epistemological underpinnings of a theory is to recognize how every theory has a starting point, evolution, and intellectual trajectory arising out of a dialectical intellectual process. While some ideas are introduced, debated, and discarded or discredited, other intellectual threads are agreed upon in an intellectual community. These ideas survive and are built upon in subsequent use and debate. Thus, to use a theory in an informed way one must be aware of historical developments (i.e., how ideas associated with the theory were challenged, and how and why some ideas survived the challenges and others did not).

One should also be aware that on a superficial level some concepts are seemingly shared by multiple theories. They use the same terms and vocabulary for similar phenomena. But to assume that the concept synonyms as defined and understood in different theories are indeed the same is potentially dangerous. For instance, while an explicit notion of agency that is acting with intent and knowing that actions have consequences can be found in most social theories, it should be noted that there are fundamental differences between the notion of and use of the term 'agency' as described in structuration theory, and the way in which it is described in actor-network theory.⁵ In simple terms it is a distinction between a characteristic argued to reside solely within the purview of humans versus one that is manifest in an extended network of human and machine actors (c.f., (Rose et al., 2005a)). Thus, while we agree with Orlikowski and Baroudi (1991) that the employment of a variety of different approaches is important and crucial to the development of IS research, we also agree with Deetz (1996) who argues for researchers to be careful about adopting more than one discourse, in terms of its different underlying assumptions, in any given research endeavor. In showing how any theory carries its own context and a history of responses to inquiries and challenges, Rose and Truex (2000) illustrate how efforts to integrate or combine social theories are problematic. They suggest that any potential benefit in combining Actor-Network Theory and Structuration Theory may not measure up to the problems one takes on board when trying to combine what may well be conflicting epistemologies (Rose, 2000; Rose et al., 2003; Rose et al., 2005a; Rose et al., 2005b).

Unless one is thoroughly inculcated into a theoretical tradition, one may not realize the problems and challenges to the theory that are part of the discourse of the discipline from which the theory is being borrowed. So it is not enough to have a casual understanding of the theory to be borrowed. If a researcher does not understand enough of the theoretical tradition from its original setting, the researcher opens his/her work to any of the same criticisms of that theory that have already been voiced in the original discipline of inquiry. For instance, uninformed, or "quick and dirty," use of linguistic concepts such as Noam Chomsky's notion of 'deep structures' became problematic in IS research when it was shown that Chomsky, himself, had abandoned the concept years before because it was being widely misunderstood and misused (c.f., (Chomsky, 1980; Chomsky, 1986; Truex and Baskerville, 1997; Truex and Baskerville, 1998)).

In Keil's case, his initial efforts to apply escalation theory to an IS project context were based on a limited reading of the escalation literature. Heavily influenced by Staw's (1976) classic experiment, Keil decided to begin by seeing if he could craft a realistic IT project scenario and use it to replicate Staw's results. When he failed to obtain the expected results, Keil probed the literature more deeply and came to realize that other researchers had reported that they could not replicate Staw's results (although none of these efforts, including Keil's, could be considered a "pure" replication). In a 1995 paper that presented results from a series of experiments, Keil et al (1995) explained:

⁵ Agency, in this context, relates to actions which have outcomes or consequences, in Giddens' terms 'the capability to make a difference' (Giddens, 1984). Thus an agent is, in its widest meaning 'something that produces an effect or change', such as a chemical agent, or when applied to people, 'a person who does something or instigates some activity' (Oxford English Dictionary). There is an implied causal relationship between the action and the outcome. The study of the relationship between organizations and technology involves the study of actions and their effects, the causal relationships between those actions and effects, and the relation of particular consequences to particular agents and their actions—hence agency. (Rose, Jones and Truex, 2005, pg 134.)

Frustrated by our inability to replicate Staw's results, we conducted a more exhaustive review of the literature and found that at least two other attempts to replicate Staw's experiment had failed (Singer and Singer, 1985; Armstrong et al., 1993). In light of this negative feedback, we decided not to escalate our commitment to Staw's design!

Without an in-depth view of the historical context of escalation theory, Keil fell into a trap of believing that the only true theory of escalation was Staw's self-justification theory. Keil explains:

I was not initially aware of any debate or opposing views within the literature regarding self-justification theory or whether Staw's experiment could be replicated. Brockner's (1992) review article made it seem that in spite of any challenges that might be raised about self-justification theory, it was still the best explanation for escalation behavior. So, it wasn't until I had trouble with an experiment similar to Staw's that I began considering the theory's historical context. What I learned during this phase was that: (1) Staw's experiment, though widely cited, might not be easy to replicate, and (2) my approach of providing "mundane realism" in the case scenario was at odds with the notion of a controlled laboratory experiment. As a result of what I learned, and a growing fascination with Garland's work on the sunk cost effect, I switched gears and decided to try replicating Garland's [1990] experiment in a software project context. After a false start here, I quickly began to see results that looked interesting.

While Keil's initial experiences were largely based on trial-and-error rather than on a detailed awareness of the historical roots of escalation theory, his historical and contextual awareness grew over time. The troubles faced when trying to replicate Staw's work led Keil to go back into the reference literature; and as a result, he became better versed in all the facets of escalation theory, including challenges to and extensions of that theory:

My perception is that much of the learning resulted from the trial and error process of conducting experiments and from reading the literature on escalation fairly exhaustively.

The additional learning gave him the ability to apply the theory in more interesting ways in his IS research. For example, he realized that while there had been considerable research on escalation, there had been comparatively little research on de-escalation of commitment, and he turned his attention there. Reviewer and editorial feedback provided some encouragement:

In the case of the de-escalation process model, the editors handling the manuscript encouraged us to develop a process model and this was not initially the focus of the paper but later became so. There is no question that this made for a stronger paper.

In effect, Keil's success in using the theory to help understand and explain the phenomenon at hand grew with his understanding of its historical context.

Consider how the selected theory impacts the choice of research method

The limited number of theories unique to or arising from our field (e.g.,(Keen, 1980)) along with the related issue of methodological weakness (e.g. (Orlikowski and Baroudi, 1991) have been identified as key issues afflicting IS research. Since theory and methodology are fundamentally related issues, we cannot consider the selection of theories without also considering what implications this may have on research methodology. The relative lack of "a theory of our own" and our need to borrow theories from neighboring disciplines has led some to suggest that we do not have the "established" theories necessary to perform confirmatory research, and we should thus concentrate our efforts on the exploratory or theory-building phase instead (Klein and Lyytinen, 1985). In contrast, others have called for increased methodological rigor in confirmatory research (Boudreau, 2001). We will not take sides on this issue, except to note that methodological issues should be considered too when considering the adaptation of theory in research.

The choice of a research method is not made in a vacuum. Rather, method choice is driven by several factors, including: the research problem or question to be investigated; the object of the study; the experience, training, and methodological predilections of the researcher; the researcher's theoretical lens; the degree of uncertainty surrounding the phenomenon; academic politics; and the theory governing the research (Trauth, 2001). Some questions are, of course, more amenable to certain theoretical frameworks than are others. And theories resonate best with certain methodological approaches. For instance, it would be difficult to imagine conducting research under the umbrella of Actor- Network Theory without the ability to access and analyze interactions between key actors. The analysis can be of various types of texts or personal observations. Data gathering might be ethnographic or document research. But the underlying research method is essentially interpretive. So a choice of theory also has implications for the method by which a study is going to be conducted.

Actor-Network Theory also provides us with a good example of how theory and methodology are two sides of the same coin (Walsham, 1997). Like all theories, Actor-Network Theory has a set of ontological and epistemological presuppositions that should be recognized by the researcher, as they guide the research endeavors whether they are made explicit or not. An example of this can be found in the study of the diffusion of Cashcard – a digital purse with which three major banks in Sweden sought to replace cash in small transactions (Holmström and Stalder, 2001). Holmström and Stalder (2001) explored this effort by paying attention to the ANT call of arms to "follow the actors," following the Cashcard as it was rolled out. This process proved to include not only the banks attempts to enroll key allies, but also how actors stepped in and developed "anti programs" (Latour, 1987) in attempts to hinder the technology from diffusing (Holmström and Stalder, 2001). To be open to such a discovery process and the notion that the ultimate role of technology depends not only on the will of the initiators of a technology project but also on the network of supporting actors that must be enrolled, is to be sensitive toward the methodological aspects of Actor-Network Theory.

Other examples of how a choice of theory impacts the choice of method arise from research informed by a critical social theoretic perspective. Myers illustrates how ethnography and critical social theory are partners (Myers, 1997). Carr and Kemmis (1986), in the domain of educational research, link action research and critical social

theory. Truex and Rose (2004) illustrate the intersection of critical action research in the domain of enterprise infrastructure implementation. In this study the choice of a critical theoretic framework required rather more engagement between the researchers and the research subjects than that required by other theories. A further example can be found in Kvasny's work exploring the nature of the Digital Divide within the United States (Kvasny, 2002; Kvasny and Truex, 2000; Kvasny and Truex, 2001). Her work illustrates how theory and method are fundamentally tied together.

In Keil's work, there does not appear to have been much concern or consideration for how the theory impacts the choice of method. However, an examination of the escalation literature reveals that it takes a positivist⁶ perspective on the phenomenon. Table 1 illustrates how this has affected Keil's research and some of the consequences of that work.

Interestingly, Keil did not feel that the choice of escalation theory limited him in terms of the research methods he has employed.

Even if one accepts that escalation theory has a positivist bent, I don't see that it has limited me to a particular research method. I will say that I found it easy to follow the path of laboratory experiments that are the dominant form of investigation in the escalation domain, but I did not find it difficult to use other methods to examine this phenomenon. I try to remain open to a variety of research methods because I believe that the research question should drive the research method and not the other way around. In my own work, I have used laboratory experiments, field surveys, and case research.

⁶ Methodologically a 'positivist perspective' is one based on positivism. Positivism, methodologically defines an approach and techniques whose goal is to determine sets of causal relationships and generalizable laws. In the social-sciences it refers to a position where the logical truth of a proposition must be ultimately grounded in its accordance with the (physical) material world and where all arguments should be based on the rules of logical inference applied to propositions grounded in observable facts [Lee, 1999]

Table 1.		
Characteristics of escalation theory as a body of literature	How this has Affected Keil's work	Resulting Consequences for Keil
Predominantly factor or variance oriented ⁷	His initial work was initially variance-oriented as opposed to process-oriented.	Turned to process theory only when MISQ senior editor Sirkka Jarvenpaa began encouraging him to do so.
Most studies based on laboratory experiments with student subjects	Much of his work was based on laboratory experiments with student subjects and he continues to work in this paradigm.	Has struggled to publish some of his experiments, as some reviewers and editors cite the use of student subjects as a severe constraint that limits generalization.
Most studies focus on the individual as the unit of analysis with the emphasis on individual decision-making	Most of his work is based on individual decision making. The dependent variable of choice has been willingness to continue a troubled project.	His experiments have involved single-shot decisions because these are accepted in the escalation literature and because they are easier to conduct than experiments involving a series of decisions over time.
Predominantly positivist stance in terms of epistemology	Most of his research, even the case studies, appears to take a positivist stance.	He sees himself as a positivist at heart, so this has not been a matter of concern to him.

On the basis of Table 1, however, it appears that the choice of theory and the manner in which it was being tested *did* affect Keil's research. It certainly influenced him to start conducting laboratory experiments and to focus on the individual as the unit of analysis. He reflects on this decision:

When I began as an assistant professor at Georgia State in 1991, I quickly realized that it would be more difficult to conduct case research than it had been at Harvard. I didn't have any contacts in Atlanta and when I picked up the phone and said "I'm from Georgia State and I'd like to do a case study on your company" I didn't get quite the same reception as when I could say "I'm from the Harvard Business School." When I started reading up on the escalation literature, I began to realize that it was mostly based on laboratory experiments with student subjects, usually focusing on the individual as the unit of analysis. At the time, we were operating on the quarter system with some large undergraduate courses that had multiple sections. So, it occurred to me that the Georgia State environment was ideal for conducting laboratory experiments, because you had up to four chances to administer and refine an experiment in a given year and plenty of potential subjects willing to participate in laboratory experiments.

It appears as if the methodical stance taken by Keil is the result of not only the assumptions to be found in the original theory, but also of practical concerns about how

⁷ A variance theory deals with variables and seeks to explain the variance in some dependent variable, while a process theory deals with a series of discrete states (or events) and the probabilistic forces associated with state transitions, rather than a set of causal relations among variables (Mohr, 1982)

he could conduct controlled lab experiments to test these theories. Still, it is apparent that Keil has been aware of the underlying worldview in escalation theory and its implications for research methodology when adapting it to IS.

It has to be said that escalation as such is a very broad object of study and is rarely covered as a whole in a single study. The research method selected for a given study on escalation processes will depend on what aspects of escalation are covered in the study. Staw and Ross (1987) propose a model for analyzing and understanding the emergence of escalation situations in which they suggest four abstract classes of determinants for escalation situations: project determinants; psychological determinants; social determinants; and organizational determinants. Each of these classes of determinants can be studied by different means.

First, laboratory experiments may be fitting settings to explore psychological determinants – determinants that cause individuals to see situations from a promising and optimistic view. This may include exploring managers' unwillingness to admit that an earlier decision was wrong and peoples' tendencies to "throw good money after bad" in an attempt to turn around a failing situation. Second, interpretive methods may be appropriate to explore organizational determinants. Organizational determinants include the structural, cultural and political environment of a project, for example, top management support, administrative inertia, and interorganizational interaction. According to Keil (1995), projects are more prone to escalate when there is strong political support and when projects become institutionalized. Institutionalization occurs when a project is tied integrally to the values and purposes of the organization, and when actions are taken for granted because they are so deeply imbedded in the subculture or norms of the organization. These determinants are difficult to explore in a laboratory. Thus, escalation theory does not determine the method *per se* but it is a consideration.

More recently, working with Mähring, Holmstrom, and Montealegre, Keil has conducted a theory-comparative analysis of the escalation that occurred in the Denver International Airport case using actor-network theory to provide a new perspective on the case (Mähring et al., 2004). The exploration of the differences in interpretations of the same data material using an interpretivist oriented theory and a positivistic theory can be said to be a part of Keil's reflexive approach to better understanding escalation theory.

Consider the theorizing process' contribution to cumulative theory

When using a specific theory as a resource in the theorizing process, the researcher should be able to answer: What is the added value to the theorizing process when using theory x that is not added when using theory y? The answer to this question should be given considering the tradition of the field – what we know and what we don't know. To contribute to cumulative tradition, a piece of research has to step beyond that which we already know.

The importance of contributing to a cumulative tradition has been emphasized as being a critical requirement for IS research (Keen, 1980; Keen, 1990). Kraemer and Dutton (1991) cite several examples in which researchers chose to ignore earlier related research, or even coined new terms to differentiate their work from previous related research. Clearly, the absence of a cumulative tradition will create problems in terms of poorly rooted problems and results, and also a potential for reinventing the wheel.

Further, as shown by Teng and Galletta (1990) in their survey of IS researchers' perceptions of the field, a majority of IS researchers were of the opinion that IS research **has failed** to build a cumulative research tradition. In the final 2001 issue of the *MIS Quarterly*, Associate Editor Jane Webster lamented that our field, when compared to other disciplines, does little to advance theory (Lee, 2001). The IS discipline has shown a great diversity in its use of borrowed theories, but the relationship between this practice and the development of a cumulative tradition in IS is far from straight-forward. For while the continued borrowing from other disciplines adds to the richness of our field, some researchers have expressed their concern that this practice contributes to the lack of a cumulative tradition in theories native to IS itself (Alavi et al., 1989; Keen 1980). Clearly, this issue presents us with a potential challenge.

However, there is some evidence that IS as a scholarly field is emerging as an independent discipline with its own cumulative tradition (Cheon et al., 1991). This position is taken a step further by Baskerville and Myers (2002) when they argue that it is a sign of maturity that the research in our field is now becoming a source of reference for researchers in other fields. One notable example is Rogers' theory of the diffusion of innovations, widely used in the IS literature. Work in our literature has fed back into Rogers' own work. For example, in the 1995 edition of his book, he has included a section (p. 313) on critical mass and the adoption of interactive innovations.

Keil's work contributes to a cumulative tradition in two ways. First, by drawing upon the escalation of commitment literature, he was able to provide theoretical insights into the problem of runaway systems projects, thus providing both researchers and practitioners in IS and other disciplines with a deeper understanding of this phenomenon. In doing so, he has also contributed to the IS literature on software project management.

However, he did not attempt to adapt escalation theory to the context of the IS domain. Instead, he assumed that the theory would be applicable as *is* without any changes. Keil explains:

My implicit assumption was that escalation theory would be applicable without special modification to account for the IS artifact. I did get some push-back early on from reviewers who asked the question: "What is different about IT projects?" In essence, I think they were challenging me to explain "what's special about IS projects that warrants looking at them from an escalation perspective." The best argument I've come up with is that because of the very nature of IS projects, they are particularly prone to escalation problems and therefore a good place for applying escalation theory and furthering our understanding of this phenomenon. At one point back in 1993 or so, I devised an experiment to try to tease out whether IT projects were different in terms of their escalation potential, but the results were mixed and I didn't pursue it further.

Thus, it appears that Keil never directly embraced the challenge of how to adjust escalation theory to fit the IT artifact. Instead, he implicitly assumed that it did not need to be adjusted. Thus, while he appears to have been aware of the challenge of explaining why it was interesting to examine IT projects from the perspective of escalation theory, he stopped short of trying to modify the theory to fit the context.

Second, Keil has contributed to the escalation literature and has begun to be recognized within the community of scholars that conduct research on escalation. As evidence of this, Keil recounts his experiences at two Academy of Management meetings.

A few years ago, Ramiro Montealegre and I were presenting our process model of de-escalation based on the Denver International Airport case study, and Barry Staw was facilitating the session. I remember before the session, Staw approached us and complimented us on what a fine paper we had written and how surprised he was that it was written by some IS researchers. In 1999, I was asked to chair a session at the Academy meeting on escalation and most of the papers in the session were written by escalation researchers rather than IS researchers. I don't know how they picked me, but the fact that they did suggests that at least some of my work has made an impact outside the field of IS.

In addition to his work on de-escalation (Keil and Robey, 1999; Montealegre and Keil, 2000), Keil's application of real options theory (Keil and Flatto, 1999) and his comparison of theory-informed models capable of discriminating between projects that escalate and those that do not escalate represent contributions back to the reference discipline. His work on de-escalation and his work relating escalation and real options were cited by McGrath et al. in an article that was published in *The Academy of Management Review* (McGrath et al., 2004). The McGrath et al., (2004) article, written as a response to an article on real options, was one of several articles that touched on the debate about whether or not real options thinking promotes escalation of commitment on projects. More recently, Keil has pushed out into related areas such as the reluctance to transmit bad news in organizations, thereby drawing a linkage to escalation that can ultimately expand the theory base in another direction. His work in this area was recently cited by Zardkoohi (2004) in an article that was published in *The Academy of Management Review*.

Since his work has mostly been published in IS journals, however, Keil's work remains off the radar screen of many escalation researchers, and has only slowly begun to be referenced by researchers who publish in mainstream management journals. His contributions toward building the escalation literature would undoubtedly have greater impact if he had chosen to publish his work in journals such as *Administrative Science Quarterly*, *Organization Science*, and the *Academy of Management Journal*.

Conclusions

This paper set out to shed light on the process of adapting theories to IS research. Given the extensive use of theories in IS, and the overall importance of theories in the research process, the importance of this question cannot be over-emphasized. The importance of theory can be illustrated in various ways. For instance, Straub et al. (1994) found that theory was considered the most important criteria to judge the quality of IS articles by the IS community, and Rao and Jarvenpaa (1994) noted that theory provides the 'channel' for genuine understanding between researchers. Having said this, it should be noted that the discussion on what theoretical approach to make use of can only be taken to a certain point. We hold that there is no thing such as "a best theory," and instead of focusing solely on theories, as such, we should also focus on theorizing, a process in which theories are a part (Weick, 1984). Weick (1989) understands theorizing as "disciplined imagination," and theories as something that add discipline to the research process. This is a pragmatic point, and one that we find valuable and essential. Theory must grow to keep up with the emergent underlying phenomena it purports to model.

We have focused this discussion on a specific domain of this theorizing process, namely that of properly adapting theories to inform one's research. There are several good reasons why an IS researcher should be cautious when adapting a theory to better make sense of a phenomenon at hand. The best reason is perhaps the very practical observation that any paper that does not select and make use of a theory in an appropriate and reflexive manner will be routinely rejected when submitted to journals (Sutton and Staw, 1995). Thus, there is a very practical reason to reflect further on proper procedures for adapting theories.

The purpose of this paper was to present a set of recommendations for adapting theories to IS research. Following a self-reflexive analysis of theory adaptation from the domain of escalation theory we have put forward four recommendations for theory selection: (1) consider the fit between the selected theory and the phenomenon of interest, (2) consider the theory's historical context, (3) consider how the theory impacts the choice of research method, and (4) consider the theorizing process' contribution to cumulative theory. One might, of course, ask what is new in these recommendations? In our reading of the literature, we find these four notions are not linked as a cogent unit. We concede that many of these points are implicit in theory adaptation practice by some researchers. But the key point is "implicit". It is our intention, and we hope one of our contributions, therefore, to link these ideas explicitly and to make it clear that while they are not the only issues to be considered in theory adaptation, they are an essential and parsimonious set.

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