

4 DROPPING YOUR TOOLS: The Diversity of the Research Agenda in Organizational Dynamics of Technology-Based Innovation

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Abstract

The debate between protagonists of different theoretical approaches continues in the Information Systems field, with little prospect of resolution. The debate is typically characterized by tendentious arguments as advocates from each approach offer a somewhat one-sided condemnation of other approaches. A recent debate in the Scandinavian Journal of Information Systems (SJIS) illustrates the manner in which IS researchers are polarized into opposing camps, each tending to view the other as inferior. Ironically further polarization is occurring in the manner various groups of IS scholars are simultaneously calling for order, discipline and clearer notions of the “core of the discipline” while other scholars call for greater research diversity. In order to overcome this polarization we advocate a strategy recommended by Weick (1996): Drop your tools, hold your concepts lightly and update them frequently. Three reasons for dropping our theoretical tools are suggested as a means for moving forward, both for individual researchers as well as for the research community as a whole.

Keywords

Theory, research diversity, actor network theory, structuration theory

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1 INTRODUCTION

In declaring that “technological innovation continues to be problematic for many organizations,” the call to this IFIP Working Group 8.6 working conference on Organizational Dynamics of Technology-Based Innovation asks for researchers to offer up “alternative conceptual lenses and standpoints” to help understand “why the same technology that ‘diffuses’ in one context meets only resistance and rejection in others” and to better understand the “organizational dynamics of technology-based innovation” (McMaster and Wastell 2006). This request is made because the IFIP WG 8.6 community may be too closely associated with diffusion theory in its many forms. But the very nature of the request implies that theoretical diversity is a positive and helpful end for a research community. Interestingly this request to broaden the theoretical repertoire is articulated at a time when other voices are being raised for more narrowly defining “the core of the field,” differentiating those theories and means of knowledge creation that are unique to our field and effectively bounding theoretical options rather than opting for diversification. The debate takes many forms with distinguished colleagues on either side, holding tenaciously to favored positions (Benbasat and Zmud 2003). We weigh in on this discourse by proposing a set of recommendations that are neither theoretical absolutes nor theoretical relatives, but suggest a reflexive view toward theory adoption, use, and diversification. To do this, we reference a recent debate in two IS research venues, the *Scandinavian Journal of Information Systems* (SJIS) and the *Journal of the AIS* (JAIS), in which we have had a voice.

Diversification and pluralism are important ideals to strive for, but we need to acknowledge that theoretical discourses tend to be centered on a limited set of theories that hampers the discourse. The influence of diffusion theory on the IFIP 8.6 community is one example, and there are others. There is too often a polarization between rival theories that does not push the discourse further. In order to overcome this polarization we advocate a strategy recommended by Weick (1996): Drop your tools, hold your concepts lightly and update them frequently.

This paper is structured as follows. First, in section two, we examine a recent debate in SJIS in which the topic of technological agency is used as a vehicle for illustrating the problem. The seven contributions to the debate are interesting in part because of the shadow of supremacist strategies that they cast. The debate illustrates the manner in which Information Systems researchers are polarized into opposing camps, both of which tend to view the other as inferior. In the third section, we advocate the strategy recommended by Weick (1996) in order to overcome the polarization into opposing camps: Drop your tools, hold your concepts lightly and update them frequently. We will put forward three reasons for dropping our tools: the focus on improving practice, the focus on building cumulative tradition in the mother discipline, and the focus on building cumulative tradition in the IS discipline. Finally, the paper concludes with an assessment of the relevance of dropping your tools of research as a means to move forward.

2 CONCEPTUALIZING THE RELATIONSHIP BETWEEN TECHNOLOGY AND ORGANIZATIONS: CONSEQUENCES OF SUPREMACIST STRATEGIES

The history of IS research has been characterized by the hegemony of the positivistic research tradition (Orlikowski and Baroudi 1991). There has been a clear tendency in the field to relegate “soft” research approaches to a secondary position compared to the “hard,” positivist approach (e.g., Benbasat et al. 1987). However, Dutton (1988) has criticized Benbasat et al.’s interpretation of how qualitative case studies should be conducted because of the explicit bias toward quantitative methods. The preoccupation in the IS field with hard research approaches is manifest in the excessive reliance on positivist and quantitative strategies for IS research. Clearly, while paradigms should ideally serve as a lens to illuminate research issues, in practice they serve as blinkers to help achieve closure. A pluralist strategy would allow for different paradigms to be applied in a research situation. It would also allow for theoretical approaches to be a part of a contingent tool-box approach where their strengths could be used as appropriate (Landry and Banville 1992). In contrast, a supremacist strategy would seek to establish one theoretical approach as universally applicable and “best” in all situations (for a discussion see Fitzgerald and Howcroft 1998). In our view, the character of much of the current debate seeking to identify or establish agreement about the “center” or core of the IS discipline (Benbasat and Zmud 2003) has the character of the later strategy.

In a recent debate in SJIS, Rose, Jones, and Truex (2005) discuss the issue that has occupied center stage in IS research for decades: how do we conceptualize the relationship between technology and organizations? They raise a number of important concerns with existing approaches to this issue, and by highlighting these limitations; they challenge us to rethink our cherished assumptions to studying technology in context. With respect to the question of agency, Rose, Jones, and Truex argue that both the structuration theory (ST) and actor-network theory (ANT) perspectives are lacking in different ways. Structuration Theorists are seen to privilege human agency over technological agency, while ANT theorists go too far in their assumptions of symmetry between humans and machines. Rose et al. (2005b) described this incompatibility between ANT and ST accounts of humans and machines as *the problem of agency* and suggested some guidelines for a more consistent, theoretical treatment of agency, and a metaphor for that theoretical development: “the double dance of agency.” In doing this they are in a sense urging the IS community to drop their tools and to push the debate further.

The idea of the double dance of agency was critiqued by a number of researchers quite familiar with ANT, ST, or both. These critiques cover a range of concerns from challenging the attempt to rationalize the distinct positions in ANT and ST with regard to *agency*, to attacks on one of the theories, or attacks on the authors’ mental state. Walsham (2005, p. 153) argues that, instead of building integrative agency theories, we should “encourage a thousand theoretical flowers to bloom.” Orlikowski (2005, p. 183) suggests a distinction between human agency and material performativity as a way forward. Hanseth (2005, p. 159) denigrates the contribution of structuration theory, and privileges the contribution of actor network theory. He argues that ANT resolves central

problems in the relationship of organizations and technology whereas structuration theory, typical of other social theories, is technology blind. Hanseth suggests that instead of focusing on symmetry, researchers should focus on the notion of *hybrid collectif* an idea McMaster and Wastell (2005, p. 174) echo, arguing that “only collectives can act.” Hanseth sees the issue of symmetry to be a historical concern, one that is no longer in the forefront of theory development, insisting instead that researchers forget about symmetry and concentrate on hybrids and collectives. In response to the Rose, Jones, and Truex challenge to ANT theorists to be more specific about the way nonhumans act, McMaster and Wastell (p. 175) argue that this is really a nonissue in ANT. They counter that the challenge itself is expressed in a way that *separates and contrasts* humans and machines—precisely the dichotomy that Latour’s project sets out to undermine. McMaster and Wastell pejoratively accuse Rose et al. of having a “symmetrophobic block” (p. 175) arguing that symmetry is not the same as equivalence.

There are many interesting elements in this debate, but a common thread we see are the contours of a *supremacist strategy*—a strategy aiming at establishing one theoretical approach as universally applicable. The unfortunate consequences of the supremacist strategy could be that we end up with a debate for or against a particular theory instead of what it can tell us in the ongoing discourse in our discipline. Two of the debate participants suggested alternate lines of reasoning.

Holmström (2005, p. 167) chooses instead to consider the historical context of theories and the likely trajectory for those theories in the domain of IS research; or, what came before and what comes next? He posits that researchers need to focus on more than a purist’s notion of what came before (i.e., how Latour and Giddens formulated their ideas) but also on our own theoretical contribution (i.e., what comes next). Focusing solely on “what came first” may hamper any discussion on what comes next and the growth of the theory as a result.

Orlikowski (2005), in a terminological slight of hand, proposes to speak of “human agency” and “material performativity” in order to avoid falling into unfortunate polemical traps that hamper further understanding of technological changes. Unfortunately, however, changing our description of the nature of agency as invested in people or things does not change that nature. And as Langer and Turkle each independently illustrate in the domains of the visual arts, media, and language, our inclination as humans to personify technology does not make it human. (Langer 1953, 1957; Turkle 1984). Rather it *humanizes* technology so that we might more easily assimilate it into our value systems, routines, social structures, work, and, indeed, our collectifs.

Building a cumulative theory assumes common and agreed notions of the “problem” and potential alternative futures. To our thinking, the current tenor of the debate does not advance that goal. So in the next section, we focus on reasons for dropping the tools of theoretical certainty so we might find common ground and advance theoretical and methodological discourse.

3 THREE REASONS FOR DROPPING THE TOOLS

As the IS discipline has evolved with relatively permeable research boundaries over the years, the diversity in theoretical underpinnings has been essential to the evolution of our

discipline. To this end, we find the challenges raised by Rose, Jones, and Truex to both researchers and practitioners to be relevant. The authors argue that when reference theories are taken into the domain of IS, different problems emerge. Among other things, they argue that there are correct and incorrect ways to use theories such that “what came first” or that being aware of what was really said in the original theories should guide use of the theory. But this is not enough. Rose, Jones, and Truex also point at the need for a continuous evolution of theories. In the following section, we build on this position by advocating for Weick’s (1996) strategy, namely, to drop your tools, hold your concepts lightly and update them frequently. We will put forward three reasons for dropping our tools: *the focus on improving practice, the focus on building cumulative tradition in the mother discipline, and the focus on building cumulative tradition in the IS discipline.*

3.1 Focus on Improving Practice

In recent years, the IS community has come under severe criticism for conducting research that has little relevance for practice. The gist of the criticism is that IS academia operates in isolation from practice and the findings of academic research efforts do not influence practice. A dynamic perspective of the interaction between IS academia and practice will help us understand better how IS academia can influence practice. This process should be continuous and subtle (Koch et al. 2002), but as it stands today, this process needs to be strengthened. Moody (2000) defines relevant research as that which “addresses a practical need,” and goes on to state that relevance and utility can only be evaluated by practitioners. However, since much research does not have direct or immediate relevance to practitioners, the question arises as to how those findings should be disseminated in a suitable form at such time as they do become relevant. While a journal like *MIS Quarterly* is found to be important to research, practitioner publications are often found to be more useful for teaching. This practice is slammed as being hypocritical by Robey and Markus (1998), who insist that academics be forced to “eat [their] own dog food.”

While the lack of a cumulative tradition within IS research is often lamented (Benbasat and Zmud 1999; Keen 1991), there are voices arguing that a cumulative tradition may actually hinder relevance in an era of rapid change (Davenport and Markus 1999; Robey and Markus 1998). In fact, results that are highly relevant to pragmatic issues might be rejected as being irrelevant merely because they are presented in an inaccessible style (Robey and Markus 1998). Too often research is driven by researchers’ own interests and the profiles of publication outlets rather than practical needs (Lyytinen 1999; Moody 2000). If academics work in isolation and then try to impose ideas on industry, they are bound to fail. IS researchers should, therefore, look to practice to identify research topics that are likely to be of future interest (Benbasat and Zmud 1999). Clearly, we need a change toward a greater appreciation for practical issues. To drop your tools, to update them and adjust them to face this challenge, is a part of this effort. In the SJIS debate, we could see no efforts in this direction. Rather, it reminds us of the analogy semiologist David Blair uses (after Ziff) when describing words as tools, whose meaning are determined *in use*. He illustrates with the notion of a screw driver, which is normally used as a tool for driving fasteners with an inclined plane, but which in a paint shop may be used to pry paint lids off cans or, in a street fight, may be used as a defensive weapon

(Blair 1990). If we allowed theories in IS to be more exposed to practice, we could also expect to see more creativity among IS scholars in adapting theories in relation to actual needs in practice. We would then indeed be “eating our own dog food.”

The notion of theory and praxis also raises two related questions: (1) How does the theory “fit” the problem at hand? (2) Are there methodological issues arising from the choice of a theory? Gregor (2006) distinguishes between five interrelated types of theory: (1) theory for analyzing; (2) theory for explaining, (3) theory for predicting; (4) theory for explaining and predicting; and (5) 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. We find this relevant to the call for papers of this WG 8.6 gathering wherein the conveners wish to consider alternative theoretical frameworks to those now closely identified with the work of this research community. It is relevant to the issue of IS *in practice* and IS research *as a practice*. In research fields, the focus of attention varies over time and theories come in and go out of fashion (Jones 2000). Given that a poor fit of theory-to-question or problem stands to result in misleading or uninteresting findings, we need to be particularly careful in the rush to find new or different theoretical lenses to apply to a domain of inquiry. This theme is more fully developed elsewhere (see Truex et al. 2006) and is raised only as a cautionary note in considering the call for theoretical diversity.

The second concern when considering the impact on practice is the effective match of theory and research method. Simply put, different theories are more or less amenable to different types of data, different means of acquiring data, and different means of inquiring that data. Theory and methodology are fundamentally related issues such that we cannot consider the selection of theories without also considering what implications this may have on research methodology. This topic is also treated in a much fuller way elsewhere (see Truex et al. 2006). So again, we raise this as cautionary note because researchers need to be aware of the costs and implications of the choice of theory in considering the impact on their own practice of research or the practice of IS design, development, and management.

We share the concern that IS academia operates in isolation from practice such that academic research efforts do not significantly influence practice in the same way as some of our other academic brethren such as computer science or system engineering. The way in which academics cling to pet theories and, too often, resist further adaptation of it, contributes to this unfortunate state of affairs. In our mind, a reconsideration of the value proposition our research offers and further consideration of the real needs or practitioners would help reduce our isolation and infuse the field with fresh insights.

3.2 Focus on Building on Cumulative Tradition in the Mother Discipline

Holmström (2005) finds the challenges raised by Rose et al. to both researchers and practitioners valuable. In particular, Holmström states that he

find[s] the argument that there are correct and incorrect ways to use theories to be an interesting and important challenge to our field. When it comes to theories one cannot only take the good bits and leave the bad bits behind. If a

researcher does not understand enough of the theoretical tradition from its original setting, s/he is likely to open the work up to any of the same criticisms of that theory that have already been voiced in the original discipline.

However, the relation to the discipline wherein the theory of interest has emerged is rarely reflected upon. Even more rare is the effort to actually contribute to that discipline. An exception can be found in Truex et al. (2006) wherein the authors suggest that researchers, borrowing theories from other disciplines, have the responsibility to do so knowledgeably, with fidelity, and with current knowledge of the discourse surrounding those theories in the home discipline, but also that they have responsibility to actually attempt to further the discourse by use of the theory in a second discipline. They state

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 (p. 30).

For them, “there is a pressing need to pay attention to cumulative tradition when adapting theories to IS research.” They illustrate via Keil's use of escalation theory how work in IS research settings may contribute to the cumulative tradition and feedback into the discourse in the home field. This is a way in which escalation theory can be dropped in order to pursue a better explanation of technological change. Such a willingness to adapt theoretical tools is rare but looks like a promising route to pursue.

Perhaps arguing that IS researchers need to make contributions back to the mother discipline is a tad arrogant and ambitious. But we are of the opinion that our field has made theoretical contributions that should be noted elsewhere. We see the development of extensions and refinements to escalation theory made by Keil and his colleagues or the refinement and extensions of Habermas' theory of communicative action by Klein, Lyytinen, Hirschheim, and others as examples.

3.3 Focus on Building on Cumulative Tradition in the IS Discipline

The key challenge for an IS researcher approaching a theory from another discipline for use within IS is to invest the 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. But while 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, the faithfulness toward original theories is only a part of such reflexivity. For this reason, we want to elaborate on the importance of considering not only a theory's historical context, but also the theorizing process' contribution to cumulative theory.

Weick (1995) points out that “theory is a continuum” and as theories move from visions to detailed constructs and propositions they lose some of their accuracy and

become more of an approximation, but they also become increasingly useful to the discipline. Building on Weick's description of the theorizing process, Truex et al. explore how social theories should be adapted to IS research and argue that both the historical context of the theory and the contribution of the theorizing process to cumulative theory should be considered. This is in concert with Weick's idea of embedding your theoretical contribution in the context of what came before and what comes next (p. 389). This includes not only the life cycle of one's own research process but, more importantly, the ongoing discourse in the particular discipline in which one is immersed. Such development depends on the generalization that Yin (1994) labels as an "analytical generalization," where the researcher "is striving to generalize a particular set of results to some broader theory" (p. 36).

With this in mind, Rose et al. (2005a, 2005b) might be criticized for concentrating on what came before rather than what comes next. They ask us to remain faithful to the original ideas from Giddens and Latour. But, following Weick's idea of theorizing, we argue that there is an even higher standard. Namely, we must seek not only to remain faithful to the key elements in the original theory, *but also* to develop the theory further. This begs the question of how can one be faithful to an original theory and develop it further. We suggest that to be ready to drop your tools, then, is to be ready to develop them further in line with a theory-development ideal.

To illustrate, we use Orlikowski's efforts to adapt Giddens' notions to the realm of research on information technologies use. Orlikowski (1992), recalling "interpretive flexibility," emphasizes that "there is flexibility in how people design, interpret, and use technology, but that this flexibility is a function of the material components comprising the artifact, the institutional context in which a technology is developed and used, and the power, knowledge, and interests of human actors" as well as time (p. 421). To her credit, her adaptation is a development of Giddens' original ideas to the practicalities of IS. Even so, a problematic issue in Orlikowski's model is that she puts technology *between* human agency and structure, and thus reestablishes a dualism that Giddens' structuration theory overcomes (Jones 1998; Rose and Truex 2000). In a later work Orlikowski (2000) appears to recognize this and proposes a "practice lens" as a means of overcoming these inconsistencies, thus bringing her characterization closer in line to Giddens' intent.

However, an important point should not be missed: those who, like Orlikowski, adopt structuration theory for their research are adapting and, willingly or not, developing Giddens' original ideas as they fit them to the practicalities of IS. They are conducting a kind of action research project on the theory itself. Such work illustrates the challenges of adapting general theories of society to the particulars of organizational life and IT research.

Jones (1998), for example, describes problems involved with applying structuration theory when trying to be specific about how information systems are used in organizations. In an attempt to modify structuration theory, he tries "to move beyond the pure subjectivism of Giddens structuration and to incorporate a form of material agency" (Jones 1998, p. 299). This point is elaborated in the SJIS debate in which there are two basic positions. There are, on the one hand, arguments advocating the need to remain faithful to the main thrust of Giddens' original ideas. On the other hand, there are arguments advocating the necessity to adopt Giddens' original ideas to the particulars of IS

research. A good researcher will be able to combine these two positions, but in order to do so will need to be open to the idea of dropping theoretical tools in order to contribute to cumulative tradition in IS.

4 CONCLUSIONS

This paper is influenced by the work of those scholars who have become sensitive to the need of further developing our explanations of technological change by adopting and trying on theories that have not been applied previously in the IS research arena. In so doing, they are effectively helping us all drop our tools and renew our ideas and the character of our discipline.

With this in mind, we ask: Why is it, then, that an approach such as ANT has failed to make a similar impact as have positivistic accounts of technological change? One reason can be found in the character of the intellectual debate surrounding ANT; the debate is concerned with arguments for or against ANT rather than what ANT can tell us. There are a number of such debates in our field. They are often characterized by tendentious arguments as advocates, protagonists of different theoretical approaches, offer a somewhat one-sided condemnation of other approaches. These debates have little prospect of resolution unless participants do not loosen their grip on their tools. The debate on technological agency illustrates the manner in which IS researchers can be polarized into opposing camps, each of which tend to view the other as inferior.

This casts a shadow of supremacist strategies over a discourse and the field loses as a result. For if we accept that theories shape what we notice and ignore and what we believe is and is not important and that, as Lyytinen and King (2004) assert, better theory is likely to contribute to stronger results, we are obliged to loosen our grip while exploring theoretical notions. It is our hope, accordingly, that as the IFIP WG 8.6 community seeks to cast a wider theoretical net over the issue of the organizational dynamics of technology-based innovation, by dropping a diffusionist-centric approach it may also focus on improving practice, focus on building cumulative tradition in the mother discipline, and focus on building cumulative tradition in the IS discipline.

In order for a community to have the capacity for ongoing renewal, it has to identify the elements holding it back. Today it seems as if the ways in which researchers hold on to theories and defend their usage is such an element, which pervades the field and is not limited to the IFIP 8.6 community. In order to make change happen, we believe that dropping the theoretical tools that hold us back is a key, but we need to do this in an informed fashion.

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