15 INFORMATION SYSTEMS RESEARCH AT THE CROSSROADS: EXTERNAL VERSUS INTERNAL VIEWS

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Abstract

The advent of the third millennium provides the backdrop for exploring the current intellectual stage of IS research in both substance and matters of research methods. The paper first identifies and reflects upon two key aspects that have shaped our discipline for the past three decades and are critical for us as a community to get right if the discipline is to flourish. These aspects surround: (1) the external view of the state of the community and (2) the internal state of the community. It is our contention that, in both areas, the discipline faces significant problems. In the second part of the paper, we identify some promising steps to be taken next in IS research and its institutionalization as we cross the millennium threshold.

1. Introduction

We are thankful to the organizers of this conference for this rare venue and opportunity to offer a broad picture of what we consider central issues of IS as an academic discipline and practical profession. This opportunity allows us to step back and reflect upon what has occurred in our discipline over the past 30 years that has led to the current state of affairs and anticipate what might be the focus of attention in the future. What is of particular concern to us—and hopefully everyone else in the field—is the realization that the IS discipline has reached a state of fragmentation that carries with it the threat that its members will become emasculated through dispersal into other disciplines or business functions. The institutionalized IS discipline, as we know it now, may cease to exist. Neither practitioners nor academics can escape this peril, because without a thriving practitioner community, there is little need for an academic one.

The principal purpose of this paper is to present a bird's eye view of the intellectual landscape, in which academics—in particular—and practitioners currently find themselves. As is typical for *terra incognita*, new opportunities and perils abound and it is often difficult to tell one from the other. In order to reach some understanding of the terrain through which we must pick a path leading to a promising future, we need to look back and analyze from where we have come. In particular, we focus on how the path through the terrain that the field has chosen—either implicitly or explicitly—has led to where we are now. Naturally such a venture runs the risk of resembling more a personal reflection than an objective account of history. Nevertheless, such a reflection, biased and incomplete as it may be, is important because we believe the field seriously needs to understand where it stands. To this end, we identify and discuss what we see as the two key dilemmas facing the field in its current situation. These dilemmas revolve around the two different views on the state of the IS community. One is the external view of the community; the other is the internal view. These should help us to see where we are now in the evolution of our field. We explore this evolution under the heading "where are we now?" In the second part of the paper, we conclude with some observations about "where do we go from here?" Our intention is to identify some promising steps to be taken next in IS research if we are to be successful in the next millennium.

2. Where Are We Now?

It has been, and continues to be, common place to bemoan the lack of relevancy of IS research for professional practice. However, we contend that this is only part of the problem. Let us focus on the dependence of the IS community (consisting of both practitioners and researchers) on external actors, i.e., senior management and other business functions. We contend that the current situation is characterized by four "disconnects." First, there is a disconnect between expectations as formulated by senior management and the practice of IS departments in the way they interpret their mission. Second, there is insufficient relevance of current IS research to IS practice. Third and *a*

fortiori, there is a disconnect of current IS research from senior management expectations.[1] Fourth, there is a disconnect within the IS research community in that there are numerous research sub-communities that do not communicate with each other (internal fragmentation). In the following discussion, we lump together the disconnect of IS as a whole (including its researchers and practitioners) from senior management as "the external view of the state of the community" in that it revolves around outside (non-IS community) perceptions and expectations of IS. The second problem, which we take up below, involves the internal disconnect of the field encompassing the fragmentation of IS research within the field. We refer to this as "the internal view of the state of the research community."

2.1 The External View of the State of the Community

Our interpretation of the external state of the community is based on formal and informal interviews of hundreds of IS managers on three continents over a 10 year period which have culminated in a variety of publications (Bhattacherjee and Hirschheim 1997; Hirschheim and Lacity 2000; Hirschheim and Miller 1993; Lacity and Hirschheim 1993, 1995; Sabherwal, Hirschheim, and Goles 2000). They point to five expectations that IS managers are confronted with from their board and peers in the other business functions.[2] These five expectations stand in contrast to the regularly published "top issues facing the CIO," published in outlets such as *MIS Quarterly*, because they have more persistence about them. As our data have been accumulated over a decade, we reached the conclusion that they have continually shaped the quality of the relationships of IS practitioners with the organizational environment in two ways: horizontally with the other business functions and vertically with senior corporate management. These expectations are:

1. Lower costs of the IS function
2. Increasing speed of delivery of IS products and services
2. Comprehensive, cross-functional data availability
4. Demonstrable value add
5. Leadership in shaping corporate strategic direction

Even a cursory examination makes it apparent that the IS community has not been able to meet these "external" expectations in the past nor is likely to do so in the foreseeable future.[3]

Lower IS Costs. True, hardware functionality in terms of processor speed, bandwidth and storage capacity has been increasing while prices decreased and this trend

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[1]We do not wish to imply the identification of such disconnects that IS research must necessarily cater to industry ideology. As we will suggest below, IS research can and should help shape such ideology.

[2]In part, these expectations and where they come from, how they are formed, and what they lead to are taken up in Poora and Hirschheim (1999).

[3]Of course there have been times, such as in the early 1990s, when many IS departments did lower their budgets, typically through downsizing, but this often led to a concomitant rise in hidden IT spending in the business units that didn't show up in the corporate IS spend figure. Hence, the perception that IS costs had actually gone down during this period is somewhat illusory. And where it wasn't illusory, organizations typically suffered.

is likely to continue, but this misses the point. Lower IT costs typically do not translate into lower overall costs for IS functionality, but the IS practitioner community has historically had difficulty providing persuasive grounds as to why this is so. Simply put, IT costs only make up a fraction of overall IS costs. As the IS function delivers more and more products and services to the business units, its overall costs go up inevitably. Consider the analogy with the car industry: Consumers expect greater functionality from cars, yet don't necessarily expect them to go down in price, even given a decrease in price of some car components. So does it make sense for senior business executives to expect lower costs from their IS units?

Speed of delivery. One of the persistent problems confronting IS is the speed with which it can deliver products and services. As the number of products and services demanded from it increase, the function struggles to meet expectations. Just as senior management use the decline in hardware costs to buttress their belief that IS costs should go down, they also believe that new technology should aid IS to deliver products and services more expeditiously. And it does, to some extent. But just as senior management's belief that IS costs should decrease is misguided, so too is this. The IS function is increasingly attempting to balance a greater and greater number of system demands. Whether it is implementing an ERP across the organization, maintaining legacy applications, putting in a new telecommunications infrastructure, and/or preparing the organization for electronic commerce, these demands simply place an enormous burden on IS. And the burden grows daily as more and more business units request new, complex services.

Comprehensive, cross-functional data availability. Although organizations for some time have wished for and—to some extent—have been promised information systems that could deliver comprehensive data spanning the entire organization, such a desire was more pipe-dream than reality. Nor did IS organizations help themselves with the overselling of database technology, and decision support and executive information systems, promising senior executives exciting integrative possibilities now that data were accessible from across the entire organization. In fact, these systems, while potentially beneficial to management, were not the panacea they were often sold as. While they did provide richer data that was easier to access and understand, they were neither comprehensive nor truly cross-functional. Senior management needs this kind of information but perhaps until recently—with the advent of client/server based ERP= applications—such wishes could not be met. Again, in many ways this mismatch of expectations is in part IS's fault. Delivering cross-functional data and applications is not only a technological problem but a political one as well. The IS function has simply not done a good job in making this situation visible to all.

Demonstrable value-add. Perhaps the most intractable problem that IS faces is the issue of how to get management to see the value-add that the IS function provides. This issue is connected to the evaluation of IS, i.e., the evaluation of IS products and services, as discussed in the previous section of the paper. While IS evaluation, indeed, has received a fair amount of attention in research, the results have been disappointing in that no reliable method has been found to measure the value of an IS before it has been built (and even after it is built, *cf.* Smithson and Hirschheim 1998). *A fortiori* it follows that the evaluation of the IS function as a whole is even more intractable. According to Porra and Hirschheim (1999), the IS function is often perceived as "overhead"; that is, a cost

of doing business but one to be minimized. As such, business unit managers want IS costs to decrease using the rallying cry "these IS overhead costs are killing us." In such an environment, it is hard for IS to demonstrate its strategic contribution to the organization as it has had to focus its attention on justifying why it charges what it does to the business units. Simply put, if IS continues to be seen as overhead, with all the negative connotations this brings with it, it will be increasingly difficult for the IS executive to focus on its strategic role and, more specifically, get senior management to focus on IS's strategic potential.

Leadership in shaping corporate strategic direction. Somewhat paradoxical to the last point where IS is typically perceived as overhead, organizations often wish IS would take an active role in shaping a corporation's strategic direction. The belief seems to be that the IS function is uniquely suited for this role for two reasons. (1) Because IS develops systems for all of the business units in the organization, it has to understand how the different systems fit together. Therefore IS leaders are well suited to having a good overview of all the functions and systems of the organization. (2) IS is perhaps the most knowledgeable group of individuals in the organization on new technologies. Given these new technologies may provide opportunities for the organization to get into new businesses, new markets, etc., such technology expertise could prove invaluable for setting strategic direction. Hence IS is uniquely placed to provide leadership in shaping a company's strategic direction. Yet, here too we note a serious inconsistency. On the one hand, IS has been perceived as being an overhead, as too expensive, as failing to provide comprehensive data across the organization and insufficient value for money; while on the other hand, it is supposed to provide leadership in strategic direction. The first set of problems with IS has led the function to being ignored for corporate senior management positions. How often does one find a former IS director in the capacity of chief officer of a corporation? Answer: not very often. To us, it is totally inconsistent to expect IS to take a lead role in shaping strategic vision and direction without giving IS the necessary access to senior management positions and the knowledge and motivation that come with it.

So where does this leave us? Clearly there is a problem with the non-IS practitioners' view of IS. They have an unrealistic image of IS and, concomitantly, unrealistic expectations about what IS can and cannot accomplish. In placing these expectations into a historical perspective, we have reached the conclusion that the theoretical concept and image of the nature of information systems to which the IS research community has subscribed since its beginnings has been incongruous with that held by the external "consumers" of IS research. As a result, they do not look for enlightenment through IS research, because they have given up on our research a long time ago. Maybe they are right in that IS theories are truly irrelevant for practice *(horribile dictu)*. If we truly believe that at least some IS theories are, indeed, relevant for practitioners, we must have done a very poor job of communicating in a convincing way which theories are relevant for practice. (We don't want to imply that every bit of theoretical exploration has to be immediately relevant for practitioners.) There are two sides to this incongruency issue. First, the IS concept held by the IS practitioners is at best partially supported by some of the theories that guide IS research. Second, the IS concept held by non-IS practitioners, i.e., senior management or business units, is even more at odds with the academic notions of information systems and also quite different from the IS practitioners' beliefs about the nature of IS. This contributes to a credibility crises of IS as a whole that engulfs both academia and practice. In the following, we shall

concentrate on what we perceive to be the academic root cause of this problem: the IS theories underlying the "schools of thought"[4] that have produced the most influential publications since the late 1960s.

In the field of IS, most theories take the form of conceptual frameworks. They provide a coherent way for reasoning about the nature and roles of information systems and their preferred ways of development; i.e., what kinds of methods and tools in principle are needed for the analysis, design, and implementation of IS? In fact, for some schools of thought, the concern for the right systems development approach came first and assumptions about the nature of information systems were implied by the preferred systems development approach with its specific methods and tools for ISD. The theoretical framework about the nature of information systems and the preferred way of IS development are an essential part of the fundamental assumptions that researchers within a certain school of thought make. These assumptions concern issues such as the nature of reality (ontology) and the preferred methods of IS research (epistemology). They also guide the formation of specific research programs, allowing certain IS problems to come into focus while at the same time obscuring other problems. For the sake of brevity, we shall refer to this complex of ideas as "IS theory." IS theory affects the selection of research problems and the formulation of research strategy more then anything else.

But what are the IS theories that have emerged from the various schools of thought that make up the history of our discipline since the 1960s? We propose that Table 1, in brief, captures the visions of the most influential founding fathers of our discipline and, moreover, their visions are still the defining ideas for current research schools. (See Klein and Hirschheim 1999 for more details.)

Issues lost. We claim that the fundamental direction of current research agendas and strategies is shaped more by the ramifications of their founding fathers' ideas and their interplay with other research schools than by senior management expectations directed at the IS function executives. This could be substantiated by tracing how the ideas from the 10 IS theories in Table 1 have proliferated through the publications of the founding fathers' students and their students' students and how much of the literature can be accounted for in this way.[5] Hence it is not too surprising that IS research is disconnected from the expectations of managers, because for the most part it is driven by its internal traditions. In this way it is like most other disciplines, its short history notwithstanding. The issues caused by these internal dynamics ultimately define what counts as knowledge and we term the issues that this raises as "lost," because they seem to have been largely ignored in the IS literature.

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[4]When we speak of "schools of thought," we are referring to key, historical research programs that have shaped the course of the IS discipline. Giving a particular research program such a designation invariably raises some controversy, but we are prepared to defend our position on the basis that history provides a reasonable metric by which to "score" the impact of particular research programs.

[5]In fact, for the Scandinavian schools of IS theory listed in Table 1, this type of analysis has already been done for the most part in Iivari and Lyytinen (1999, *cf.* especially p. 139, Figure 1). It should be uncontroversial to extend this kind of analysis to the remainder of the Western world, where most of the IS research has been published. For example, our own research has mostly been influenced by the STS, inquiring systems, and social action theories of IS. It is beyond the scope of this paper and even unnecessary to trace the principal influences for the other schools in detail, but see Klein and Hirschheim (1999).

Table 1. The Principal Schools of Thought of IS Theory

Original Source of IS Theory	Proposed theoretical concept of information systems as:
Langefors (1966, 1973)	datalogical and infological systems
Blumenthal (1969)	reporting and control systems[6]
Teichroew and Hershey (1977); Yourdon (1978)	formally specified technical systems[7]
Churchman (1971)	inquiry systems
Dickson (1968, 1981); Davis (1974)	behavioral systems
Nygaard (1975)	weapons of class struggle
Mumford and Henshall (1978); Bostrom and Heinen (1977)	socio-technical systems[8]
Checkland (1981)	human activity systems[9]
Goldkuhl and Lyytinen (1982); Winograd and Flores (1986, 1987)	linguistic systems in social action
McFarlan (1984)	competitive weapons

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[6]It would be more accurate to characterize Blumenthal's ideas primarily as a modular, incremental design strategy of building reporting and control information systems on top of transaction-based systems. The implied IS theory is that of a parametric feedback loop hierarchy of the type envisioned by Ashby and by Forrester's *Individual Dynamics*.

[7]The earlier observation that "for some of the IS theories, the concern for the right design approach came first and assumptions about the nature of the IS were implied by the preferred design approach" especially applies to the IS theories that conceive of IS as a mechanistic, technical artifact. In the case of Teichroew, his original concern was the automation of IS design including software generation from an exact "problem statement" (expressed in PSL) using AI techniques. When this proved impossible, the research moved to supporting the specification process, i.e., IS modeling. Basically, for Teichroew, an IS was whatever could be specified consistently in a repository. De facto, this was very similar to what Yourdon tried to teach with manual specifications, even though Teichroew claimed that the ISDOS project was independent from any specific methodology.

[8]As was noted in Hirschheim and Klein (1994), there are some underlying consistencies between the original STS theory of IS (*cf.* Mumford 1983), the inquiring systems notion, and the social action theory of IS. Basically, the original STS concept can be made theoretically more rigorous by interpreting it as a type of social action system.

[9]Checkland more than anyone else gave equal thought to a theoretical concept of an IS and a development strategy consistent with this concept in his SSM (soft systems methodology).

IS emasculation and diffusion.   From the external viewpoint, the disturbing outcome of neither researchers nor practitioners having been able to meet senior management expectations—unrealistic as they may seem to us—has been the emasculation and diffusion of the IS function. This diffusion takes the form of outsourcing and dispersing IS personnel to the various business units. The accompanying withdrawal of resources has begun to emasculate many IS departments to a skeleton of what they once were. If this trend continues, it must lead to dire consequences for the IS academic community in terms of changing curricula, fewer students, reduced grants, and last, but not least, research opportunities. Therefore, it is not surprising that Markus (1999) questions the very existence of the IS discipline in a recent provocative article. She poses the question: "What happens if the IS field as we know it goes away?" For her, the field is at a crossroads. On the one hand, it could grow to become one of the most important areas for business, since no organization can ignore the inexorable development and application of new information technology and expect to survive. On the other hand, there is a move to emasculate and devolve the field, moving IT tasks and skills into the business functions. She writes:

> With dollars and people moving from corporate IS units to business units (and even further out to IT industry firms), central IS units (not just hardware) have been downsized. Corporate IS groups that once hired hundreds of people are today mere shadows of their former selves—sometimes only five or six people. [Markus 1999, p. 184]

Lucas (1999) supports Markus' concern noting that the migration of IS skills to other disciplines is occurring and that many U.S. business school deans have adopted this "disturbing belief." Indeed, we note that some universities no longer support a vigorous and expanding IS group. But this analysis needs further scrutiny, and we offer the following two comments. The first is simply the observation that moving IS personnel to business units and closer to users need not necessarily reduce demand for IS professionals. True, but what we have in mind is the issue of a center for professional identity and institutional support. If IS tasks are widely absorbed by user departments, then the core competency of IS will be lost and with it the institutional support structures. The second weakness of the above analysis is more difficult to overcome: it leaves out the recent rise of Internet-driven demand, in particular from e-commerce development. Is it true, as one anonymous reviewer of our paper contended, that net-centric applications move (or will move) information technology

> to be the core enabler of the enterprise.…The IT network is moving into a societal artifact and most organizations are beginning to believe that survival is impossible without IT. Rather than viewing the IS field as diminishing or dispersed, the field has moved to the core of the enterprise.

In response, we note that the last sentence need not follow from the first two, because of the immense potential for standardization, outsourcing, and licensing that Internet application offers. Right now, we see much experimentation and creativity and hence high

demand for Internet web site development professionals. This was similar with ISD before the arrival of software packages as a commodity. (How many DSS programmers have been employed since spreadsheet software was standardized?) Because of the public nature of Internet web sites, we believe that within the next few years we will see a limited number of standardized and parameterized "web site packages." It is quite likely that, using such packages, skilled users can customize most of the common web applications they need. And for the few that require new development, they will be outsourced. In fact, we believe that e-commerce raises more organizational issues than technical ones: professionals with a good understanding of more than one business function and the ability to realign and integrate their processes in a way that exploits the new technologies will be in high demand. Currently most academic IS programs presume business knowledge, focusing more on IT rather than teaching how to exploit it. Maybe this is our opportunity if we are able to redefine the core of the IS discipline "to get with it." Adding a new e-commerce elective course is, however, not what we have in mind here. It is going to take more than that.

Hence, in conclusion, we as researchers have to ask ourselves whether the trend noted by Markus is the beginning of a dramatic change for the field or simply a passing fad. Will companies be forced to "backsource" as they become more and more aware of their lack of IS expertise? Is there anything that we as academics can do to illuminate the issues at stake from a longer term perspective, thereby contributing to the avoidance of costly, maybe even dangerous, mistakes?

Currently the state of IS research appears to be ill equipped to recognize let alone address this issue. In large part this is because of the continuing relevancy gap between IS research and senior management expectations. An important factor explaining the relevancy gap is the images of IS that have been developed in 30 years of IS research and which simply do not resonate with the five expectations of senior management. Therefore, the research stimulated by those images does not connect to the expectations either. A further implication of the anchoring of current research agendas to these IS images is reinforcement of the fragmentation that characterizes the internal state of the IS research community, as will be explored next.

2.2 The Internal View of the State of the IS Research Community

From an analysis of influential treatments of the foundations of IS, it becomes clear that the images of the nature of information systems (*cf.* Table 1) are not the only primary influence of the state of the IS research community. The discussion about preferred reference disciplines and paradigms has also shaped the current ways of thinking and agendas in the IS research community more than anything else.[10] Together, the effects of

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[10]We do not overlook the importance of reference discipline focus as a source of differentiation, i.e., training in preferred references disciplines and professional experience as an engineer, accountant, economist, etc. This source of influence was of particular importance during the early era of IS when there were no internally trained IS faculty. This influence works through the personality of influential researchers. It affects their vision of an IS and their paradigmatic assumptions. Hence it is indirectly acknowledged. A more detailed treatment of this source is beyond the scope of this paper.

conflicting paradigms and commitment to incompatible visions of the nature of IS have fragmented the IS research community along several dimensions to the point that it is now often called a "fragmented adhocracy."[11] Hence the greatest issue that the IS community faces internally is its fragmentation into numerous specializations (or what we might call "sects"). They are in want of intellectual synthesis that could emerge from a fruitful discourse. However, we lack a set of shared assumptions and language. Hence large conferences like ICIS, AIS, or HICSS are reincarnations of the Tower of Babel. Fruitful cross-sectional debate almost never occurs. The situation can be dated back to at least 1985 and possibly before (*cf.* Hirschheim 1986).

Traditionally the relationship between alternative paradigms was conceived as being one of the following: Dominance, synthesis, incommensurability, eclecticism, or pluralism (Morgan 1983). For example, positivism—through the centuries—has enjoyed great success. Its position was one of dominance. More recently, however, critics have surfaced calling into question positivism's dominance. A call has gone out for pluralism rather than dominance in research (*cf.* Lincoln and Guba 1985). From an historical perspective, one can distinctly see the uneasy tension that has existed in the application of positivism in the social sciences. This has given rise to what Tashakkori and Teddlie (1998) have termed "the paradigm wars": battles fought by the adherents of positivism against those from other paradigms.

For Landry and Banville (1992), the paradigm wars can be recast into three types of researchers, each with its own outlook on paradigm appropriateness. They have characterized these types or groups as mainstream navigators, unity advocates, and knights of change. The first group, *mainstream navigators*, is composed of supporters of the dominant orthodoxy. Their epistemological roots are in logical positivism, which cements them in the functionalist paradigm. The second group, *unity advocates*, is more concerned with the acceptance of information systems as a scientific discipline than with a specific paradigm. In the unity advocates' view of the world, an immature or pre-science discipline is characterized by the existence of several competing paradigms. A more desirable state, that of a full-fledged scientific discipline, is characterized by the reign of a single dominant paradigm. They would be agreeable to using any paradigm as long as it granted them scientific respectability. Since the current state of information systems research is dominated by positivism, unity advocates tend to cluster toward this end of the paradigm dimension. The third group, *knights of change*, is of the opinion that

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[11]By applying Whitley's (1984a, 1984b) model of cognitive and social institutionalization of scientific fields (or academic disciplines), Banville and Landry (1989) conclude that the field of IS is a "fragmented adhocracy." This is so because in order to work in IS one does not need a strong consensus with one's colleagues on the significance and importance of the research problem as long as there exists some outside community for support. Nor are there widely accepted, legitimized results or procedures on which one must build "in order to construct knowledge claims which are regarded as competent and useful contributions" (Whitley 1984a, pp. 88 -123, as quoted by Banville and Landry 1989, p. 54). In addition, research involves high task uncertainty, because problem formulations are unstable, priorities vary among different research communities, and there is little control over the goals by a professional leadership establishment (such as bars or licensing boards for physicians and engineers). For example, some IS research groups may choose to define and cherish projects that do not follow the familiar patterns of engineering or empirical social science, although such groups are generally in the minority. There appears—to some extent at least —to be local autonomy to formulate research problems and standards for conducting and evaluating research results (*cf.* Goles and Hirschheim 2000).

reality is multifaceted and forged from the interpretations and interactions of individual actors. They also give credence to the belief that no single research approach can fully capture the richness and complexity of what we experience as reality. Thus they champion a collection of assorted research approaches arising from multiple paradigms.

Yet even the knights of change, with their clarion call for methodological pluralism, argue for change *within* Burrell and Morgan's (1979) four paradigms. Others argue that Burrell and Morgan's framework, by virtue of its widespread acceptance and impact, has normalized and rationalized emerging streams of research, constraining alternative perspectives.

> In time, influential frameworks can become as restraining and restrictive as those they originally challenged.…we are sometimes presented through responses to a conceptual framework …with a new, rich set of alternative perspectives through which we can continue our study and talk about our subject matter. [Frost 1996; p. 190]

In addition to the three types of paradigm warriors (mainstream navigators, unity advocates, and knights of change) identified by Landry and Banville, a new group is emerging which is calling for an end to the paradigm wars: the pacifists. These theorists and researchers argue that there are strengths and weaknesses in both the positivist and non-positivist positions and point out that the conflicting paradigms have, in spite of the best efforts of their most ardent supporters, achieved a state of coexistence (Tashakkori and Teddlie 1998). Datta (1994) has presented five compelling arguments in support of this assertion: (1) Both paradigms have been in use for a number of years; (2) There are a considerable (and growing) number of scholars arguing for the use of multiple paradigms and methods; (3) Funding agencies support research in both paradigms; (4) Both paradigms have had an influence on various policies; and (5) Much has been learned via each paradigm.

In the IS research arena, the existence of such paradigm pluralism can be found but it is not as wide spread as either Datta or Tashakkori and Teddlie suggest. Worse, the supposed interplay between researchers of different paradigms does not occur because of the communication gap that exists between the alternative paradigms.

Although the "internal and external views of the community" paint a somewhat worrying picture of the field, all may not be lost. In the next section, we offer two avenues for future work that might help to mitigate the deleterious effects of IS fragmentation, and the disconnect of expectations of—and the demands for—IS. We offer some suggestions for improving the generality of IS research and this primarily concerns the internal view of IS. From the external view, more generality should help to improve the transparency and interest of IS research to the external communities. This is part and parcel of a research strategy to address the relevancy disconnect of IS research.

3. Where Do We Go from Here?

If, as we claim, paradigmatic pluralism is more fruitful than harmful and that the notion of paradigm wars (*cf.* Datta 1994; Tashakkori and Teddlie 1998) is unhelpful, what

should be the next steps in the evolution of IS research? It seems to us that there are two arenas where research in the future needs to head. One reflects the internal view of the field and revolves around the issue of the generality, transparency and applicability of our work, and the embracing of pluralism. The second focuses on the need to involve the external community (i.e., non-IS) in what we do in a new way. This we recast in terms of the age-old "relevance vs. rigor" debate.

3.1 Generality and Pluralism

IS as a field has had difficulties with generalization from its beginnings. It started with story telling of experiences that were generalized into insights that should apply to many situations (e.g., the five lessons from Ackoff [1967] or the "myths" of Dearden [1966]). Such reasoning was later debunked as "unscientific" and replaced with "rigorous" hypothesis testing. It now appears as if IS research has come full circle by returning to new forms of story telling (the politically correct term is, of course, "narratives"). Whereas the new story telling movement can point to much better and more explicit philosophical grounding than "the great wise men" had for their stories, this does not necessarily make them "more general." In fact, they lag in generality behind the insights offered by the earliest authors. Moreover, they share this weakness with the failure of positivist research to offer a few broad theories that contribute to general orientation and bring some measure of order to the perpetual confusion in our field. Hence interpretivists have little reason to be gleeful about the failures of positivism. They are no stronger in theory formulation than the positivists. Should it turn out that they are equally unable to deliver results of general interest, i.e., that reduce the complexity of coping with reality by applying to a large number of instances, they may fall into disrepute quicker than the attraction of positivism is waning. So how does one deal with this dilemma?

It appears that the generalization deficit is a concern that affects interpretivists and positivists alike, yet is ignored by both. We propose that it could be addressed by a change in paper reviewing practices in the direction of giving generalization the same weight as methodological rigor. Often authors are discouraged from generalization, because they cannot support it with the same degree of plausible evidence as narrowly conceived hypotheses. This practice discourages prospective authors from connecting specific hypotheses or ethnographic findings to broader theoretical constructs that might qualify as some form of "general theory," at least within a specific sub-community. If papers are rejected for lack of rigor, then the same should apply for lack of generalization. The degree of rigor required should be tempered in relation to the degree of generalization attempted. The more generalization, the less rigor would be expected. Unfortunately, the current theory part of most papers consists of minor building blocks for more general theories that are at best implied and at worst *ad hoc* and hence do not even exist. This leads to a multitude of hypotheses with associated significant tests or ethnographic insights with associated thick descriptions, which as a whole go nowhere. They go nowhere because their interconnections do not exist or are at best transparent to the insiders who know the literature of a specific sub-community.

We believe that a new genre of papers should be encouraged, which take a major block of specific studies and mold them into a larger theoretical framework. There are

examples of this kind of work (e.g., Ives, Hamilton, and Davis 1980; Zmud 1979), but they are far too few. Of course to engage in such work not only requires considerable effort, but typically leads to paper that are longer and conceptual rather than empirical in nature. Most journals have page limitations that specifically militate against such efforts. Thus in encouraging these new papers, journal editors would have to revise their editorial policies in the following ways: (1) engage sympathetic associate editors and reviewers to broaden their view about what types of papers are acceptable to their journals; (2) set different and realistic new page limitations for such papers; and (3) revise the scope and aims of their journals to reflect the broader focus. Additionally, there are a number of books such as Checkland and Scholes (1990) and Walsham (1993) that offer good generalizations drawn from action research and detailed field studies respectively.

3.2 Relevancy and its Relationship to Rigor

The call for better generality in our research is important for the IS community as a whole, but there is also a need to better connect IS to the outside community. One way for this to occur is through research that is more relevant: more relevant for IS practitioners and more relevant for other groups who depend on IS, such as senior management and business unit managers and personnel.

One pointer on how this might be accomplished can be found in Lynne Markus' address to the 1997 IFIP8.2 conference in Philadelphia. She argued that one of the directions the field should now take is "the appreciation of practicality in IS research" (Markus 1997, p. 18). The intent of what she terms practical research is not to replace or overshadow research that builds or tests academic theory, but rather to complement theoretical research with "rigorous research that describes and evaluates what is going on in practice" (Markus 1997, p. 18). This is underscored by the conference theme of ICIS'97 with its emphasis on "the issue of relevance and relationship of IS research to practice" (Kumar 1997, p. xvii). More recently, *MIS Quarterly* announced a renewed thrust aimed "at better imbuing rigorous research with the element of relevance to managers, consultants, and other practitioners" (Lee 1999a, p. viii). The discussions presented in Benbasat and Zmud (1999), Applegate and King (1999), Lyytinen (1999), and Lee (1999b) support this thrust.

Of course, this call for relevance in research is neither particularly new nor confined to the IS field. Ormerod (1996), for example, called for "the synergistic combination of consulting and academic research in IS." Davenport and Markus (1999), in like fashion, note the value of consulting and academic research learning from each other. Similarly, Avison et al. (1999) advocate a greater use of action research to make IS academic research more relevant to practitioners.

But this call for more relevancy in IS academic research embodies a number of underlying questions; questions that do not necessarily have simple yes/no answers. We see three basic questions that need to be addressed in the area of IS relevancy: (1) relevancy for whom; (2) the relationship between relevancy and rigor; and (3) axiology of relevancy.

(1) Relevancy for whom: When we speak of "relevancy," to whom, exactly, are we referring? One constituency for whom IS research might want to be relevant is the

external community. But who is this external constituency: is it senior management, business unit managers, society, customers, the government? Is it some of these, all of these, none of these? Each of these constituencies is likely to have a very different notion of what is relevant for them. Moreover, not only will what is relevant for each of them differ, they may in fact be in conflict. Society might wish research on IS to lead to more democratic societal forms embodying empowered citizens. Corporate executives, on the other hand, might wish IS research to focus on systems that offer new market opportunities rather than opportunities for empowered workers. Another constituency likely to have a different view of relevancy is the IS practitioner community. Here, relevant research would consist of knowledge leading to, for example, better tools and approaches for developing information systems. So clearly, when we speak of making IS research more relevant, we need to define which group we are attempting to reach. Most calls for relevancy seem to mean relevant for the IS practitioner. As we have suggested above with the set of senior management expectations of IS, clearly this is an area where research attention needs to be placed. Another external community that is the recipient of our research is other academic disciplines. In relevancy vs. rigor discussions, the focus tends to be on practitioners. The fact is, much IS research is done with an eye to other academics—academics from other departments in business schools and other faculties on campus. These communities, often at odds with each other, may have entirely different sets of goals and objectives for IS. Even if they share the applied focus of IS (like the other disciplines), that does not mean that marketing, finance, organization, or operations management have consistent expectations for their IS colleagues. Hence there is continuing pull on IS academics to make their research and teaching useful for others, i.e., to be servants of too many masters. These pressures can be especially strong for junior IS faculty if they know that their tenure committees are dominated by non-IS faculty. They need to strike alliances lest they perish even if they publish.

Beyond this, many of the disciplines on campus are not applied, and see applied research as "unacademic" and hence not valued. And if such work is not valued, this poses a problem for the IS academic whose rewards (tenure and promotion) and punishments (failed tenure and promotion cases, rejected research proposals) heavily depend on other academic groupings. IS academics can ill afford to ignore what these groups consider relevant. Many IS researchers have succumbed to this pressure by undertaking highly theoretical work, which is relevant not only for the broader university community but also one other important community: IS academics themselves. While such a strategy has helped make IS an arguably accepted discipline,[12] it has done so at the cost of practitioner relevancy.

In fact, the strategy has led to the rather dubious condition of what might be termed "the vicious cycle" of academic research. This is a particularly unflattering condition of which many academics feel we are part. n the traditional model of research, the purpose of research is to generate knowledge. The model starts out with a problem, which leads to research, which in turn leads to knowledge, which in turn informs practice, which in turn encounters new problems, which start the whole cycle over again. In the "vicious" version of this cycle model, the purpose gets distorted to one where a research problem

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[12]We are, of course, aware that not everyone would agree with this view, but we'll leave that discussion for another time.

leads to research, which leads to new research problems, which leads to research, which leads to more research problems and on without end. The feedback control loop to practice is lost; research remains entirely in the ivory tower.

To summarize, there are many recipients of IS research, each with their own particular view of what relevance means. Currently, IS research has pursued relevance in the context of relevance for academic communities. But such singularity of focus has led to a possible crisis in IS. We in the discipline need to broaden our notion of relevancy to include other groups who use or could use the knowledge generated by IS research. We need research on how all the various external groups come to understand IS and how they form their perceptions about the proper role of IS. We need to research the interaction patterns between business unit management among themselves and with senior executives of the company. And if there is a mismatch between what business unit managers and senior corporate executives see as the role of IS, then hopefully IS research can lead to persuasive arguments to solve the mismatch. Fundamentally, we clearly must extend our notion of relevancy to include stakeholders other than just IS practitioners.

(2) Relationship between relevancy and rigor: A dictionary definition of rigor typically uses terms such as severity, sternness, strictness, stringency, and harshness to describe its nature.[13] In academic research, the term rigor has become the touchstone for quality and scholarship. If it isn't rigorous, it isn't scholarly. If it isn't rigorous, it shouldn't be published. Rigor seems to have taken on a life of its own in academic research. Rigor is usually manifested in research through Greek symbols, mathematical formulas, number of experimental controls, and conforming to the standard of the best research the community of scholars interprets it has done so far. Typically, this means applying the hypothetico-deductive method—the accepted methods of science. Such a view of rigor, however, excludes other forms of scholarly research, which do not subscribe to such positivist standards.

We contend that there are many scholarly vehicles for knowledge creation and they need to be recognized as rigorous as long as they employ sound forms of reasoning and giving evidence. Indeed, we believe that any knowledge claim emanating from research should be scrutinized using a sound reasoning process. This might embrace Habermas' (1984) notion of the "force of the better argument," where competing knowledge claims are evaluated and the knowledge claim based on the better reasoning, arguments, and evidence being judged as "accepted." Such a process can be used to evaluate interpretive as well as positivist research, even though interpretive research is considered inherently more difficult to be "evaluated objectively" because the community consensus about its standards have not yet solidified. Nevertheless, it can be done. An objective evaluation would typically involve considering three aspects of the research: intelligibility, novelty, and believability. *Intelligibility* relates to the question of how well the research approach and results are comprehended, i.e., how closely others can follow them with similar qualifications. *Novelty* can be judged in at least three ways: (1) by the amount of new insight added; (2) by the significance of the research reported in terms of the implications it has for seeing important matters in a new light and/or providing a new way of thinking about the phenomenon under study; (3) by the completeness and coherence of the

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[13]For some interpretivist researchers, such notions of rigor seem totally understandable as that is the way their research often seems to be treated by reviewers, i.e., harshly!

research report(s). Can the author provide an overall picture so that its components link up to each other without major holes in the picture that is being painted? *Believability*, on the other hand, relates to how well the research arguments make sense in light of our total knowledge. The key question for believability is how well the research in method and results fits with other ideas and arguments that are taken for granted within the current state of knowledge. A first measure of this is the number of references with which it is consistent (or which it challenges). New research inevitably challenges some part of the web of beliefs that makes up the current state of knowledge that is best characterized as a "web of beliefs" that are only sparsely connected to "hard" evidence (Quine 1970). Another useful image of the state of knowledge is the fact net and its proliferation in the sense of Churchman (1971). The more references are challenged the less believable, but potentially the more significant, is the research. For believability, each author must demonstrate how his research "fixes" the net of our knowledge. If the research "takes out" certain parts of the knowledge net, the author must reconnect the loose parts. Often this requires relating to some forgotten or remote parts of the web of belief (which may be the domain of some other research community). By bringing in more references from other areas, authors can often successfully challenge major parts of a local (within one discipline) web of belief.

Hence intelligibility and believability are inextricably linked (and often inversely related to the degree of novelty). Ultimately they are a measure of what the research community terms "the validity of the research." Although all research projects must produce results that are intelligible, novel, and believable for the community of scholars to bestow the label "contribution to the state of knowledge" upon them, the criteria are perhaps more subjective in interpretive research projects. To this end, Klein and Myers (1999) offer assistance. They present a set of criteria on how to judge knowledge claims generated from interpretivist research. We feel this is critically important for the IS discipline because interpretivist research can often offer better insights for practice than its counterpart. Practitioners can usually better relate to interpretive research as the research is closer to practice, involves actual case studies, involves real people in real situations, and is undertaken in real world settings. They talk about the results offering new insights, and the results are more translatable into the ways people actually work in organizations. However, interpretive research is typically weak in implying clear advice on what to do or how to improve matters in practice. This leads us to the values and ethics of relevancy, which is the issue we turn to next.

(3) Axiology of relevancy: Practice necessarily involves decisions and actions that are intrinsically value laden. If one speaks of improving practice, or providing insights (from research) which improve practice, there is no way to escape the issue of what values guide the research and for what purposes the results of the research are to be used. Fay's (1975) cogent treatment of values shows how they invariably find their way into political practice and, hence, why it undesirable to ignore this important domain. Yet, most IS research has shied away from value laden research issues or simply taken ideological values for granted (like more effective management control is desirable). However, as the debate in Accounting shows,[14] industry executives are often quite open

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[14]There has been a recent call from the big accounting firms for research and teaching of ethics in accounting departments in universities.

to critical treatment of value questions that have ethical implications. Could the same not also hold in the domain of IS? And if so, how could such a program fertilize new and relevant IS research?

Knowing about values and their behavioral implications is a type of knowledge—normative knowledge—and that an important form of normative knowledge is that of the 'rule'. Rules are important in the practice of any profession: e.g., management, engineering, medicine, law, and information systems. To characterize the kinds of rules typical for normative knowledge, it is useful to bear in mind Kant's (*cf.* 1785, 1788 — for translations see Kant 1964, 1929) distinction between rules of skill, rules of prudence, and categorical rules (to which we shall refer as ethical values). *Rules of skill* are concerned with the physical propensity and dexterity to carry out certain operations to achieve specified ends. *Rules of prudence* are concerned with judgments to achieve ends that informed and reasonable people would not question as being worthwhile, such as designing systems that are acceptable to their intended users. *Categorical rules* are concerned with choices where the ends themselves are in question, such as a choice between developing information systems that are preferable to one set of stakeholders (e.g., workers on the shop floor) or another (e.g., a company's shareholders). Categorical rules are particularly important when our discipline has to deal with social value conflicts in practice. They are concerned with ultimate values or ethical standards. Elsewhere, we (Klein and Hirschheim 1998) provide a fuller treatment of the practical relevance of research into ultimate value standards (or design ideals). This should be of interest to highest levels of policy formation. Elsewhere (Hirschheim and Klein 1994; Iivari, Hirshheim, and Klein 1998) we have also dealt with rules of prudence—what are more or less prudent principles of ISD in broadly characterized situations. Rules of prudence are best researched through comparative analysis of classes of similar methodologies—approaches as they are called in Iivari, Hirschheim, and Klein. This level of value discourse is of practical significance to project managers or IS executives who are concerned with finding the proper set of methods and tools for their organization. However, many more research contributions are needed in this area as "maybe the biggest challenge for the future of IS research is to come to grips with the need for normative knowledge in our field that goes beyond rules of skill" (Klein, 1999, p. 23).

4. Conclusions

In the introduction, we noted the perils of emasculation and dispersal of the IS function, which to some extent has already happened. However, in addressing this issue through increased relevance and improved generalization, we have become more confident that the challenges we all face could be successfully met by reforming certain institutional practices. We see the need for increasing the amount of research directed at understanding non-IS practitioners and engaging them in a discourse about a realistic set of expectations for what the IS function can and cannot deliver. We also need to provide well-articulated arguments to the IS practitioners by which they can state their case to senior management that a thriving IS department is needed along with the other functional units.

Another important change in institutional publication practices concerns redefining the concept of rigor in research. It needs to be augmented to include a wide range of

scholarly inference and evidence giving, on the one hand, and tightened, on the other, to include the linking of detailed models or hypotheses to broader theory to arrive at expanded categories of knowledge. Finally, we note that while pluralism is here to stay, we need to worry more about communicating across the narrow boundaries of our preferred academic sub-communities. For that purpose, all publication venues, in particular the large conferences such as AIS, ICIS, HICSS, and ECIS and all first tier journals need to provide some visible vehicles (e.g., special sessions, special subsections or issues) for broad syntheses that are interesting and comprehensible to all members of the IS community.[15] We need more historical analyses of the various areas that make up the IS domain. And we need to get the IS community to truly value such contributions. In the current situation, we seem to have an overabundance of specialty papers for in-group members with the result that the IS community as a whole suffers from a serious communication gap. The current publication culture of narrowly focused, highly specialized papers is one of the major impediments to making our research more relevant to practitioners. We simply must attempt the difficult, but invaluable, syntheses that pull together research results from the various sub-communities into broader analyses of potential interest to practitioner communities.

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[15]In computer science, the journal *ACM Computing Surveys* serves such a purpose. There is no equivalent in our field, although the new e-journal *MIS Review* should come close. IS needs more outlets like this.

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