Journal of the Association for Information Systems

IS RESEARCH PERSPECTIVES ARTICLE

Nothing At The Center?: Academic Legitimacy in the Information Systems Field[12]

Kalle Lyytinen
Case Western Reserve University
kalle@case.edu

John Leslie King
University of Michigan
jlking@umich.edu

Abstract

Researchers in the information system (IS) field have recently called for the field to legitimate itself by erecting a strong theoretical core at its center. This paper examines this proposition, and concludes that it is logically invalid and does not recognize ample evidence to the contrary from the history of other disciplines. We construct a broader concept of academic legitimacy around three drivers: the salience of the issues studied, the production of strong results, and the maintenance of disciplinary plasticity. This analysis suggests that to remain successful, the IS field needs intellectual discipline in boundary spanning across a "market of ideas" concerning the application of information technology in human enterprise.

Keywords: Information Systems, disciplinary identity, disciplinary legitimacy, core theory, paradigms, institutional politics, evolution of science

[1] Detmar Straub was the accepting senior editor for this paper.

[2] We are indebted to colleagues in different parts of the world where we have presented and discussed our ongoing struggle to make sense of the evolution of the IS field. These include colleagues at Case Western Reserve University, the University of Michigan, Georgia State University, the University of Western Ontario, Hong Kong University of Science and Technology, Nanyang Business School, London School of Economics, Aagdar College, the University of Colorado at Boulder, IRIS 1999, 2003, and AMCIS 2003 Philosophy track among others. We are particularly thankful to Ron Weber, Detmar Straub and Sirkka Järvenpää for their insightful comments on earlier versions of the paper.

"*Turning and turning in the widening gyre,*
The falcon cannot hear the falconer,
Things fall apart,
The centre cannot hold"[3]

William Butler Yeats (1865-1939), "The Second Coming"

Introduction

The Information Systems (IS) field arose from humble origins in the 1970s. The field is perhaps 30 years old, and is about as far along as might reasonably be expected in terms of size, quality, and institutional status. Nevertheless, the IS field continues to be haunted by feelings of inadequacy. Such sentiments are most common in North America (Benbasat and Weber, 1996; Markus, 1999; Benbasat and Zmud, 2003), but are also found in Europe (Ciborra, 1998; Stowell and Mingers, 1997). The most common manifestation of this sentiment is the lament that the IS field lacks a theoretic core, and for that reason, is rightly seen to be academically inadequate by critics inside and outside the field.

This is a serious accusation. If well founded, it is a profound indictment of the IS field, inviting appropriate sanctions unless remedied. If groundless, it imposes needless pain on the IS field, and arguably misdirects energy that would be better spent on other matters. This paper analyzes the argument that academic legitimacy hinges on the presence or absence of core theory. It finds the argument logically weak and empirically refutable. This result does not imply that stronger theory is unwelcome in the IS field, or that such theory would not help strength the field's reputation. It merely suggests that the current fixation on legitimizing the field through pursuit of a core theory around which to center inquiry and intellectual mission is misplaced.

To move the discussion forward from the debate around the nature of core theories, this paper suggests an alternative model of disciplinary legitimacy grounded in three drivers: the salience of the issues studied, the production of strong results, and the maintenance of plasticity. These principles have sustained the growth of the IS field over three decades, and established it as a legitimate actor among academic fields. They preclude a center of fixed core ideas or relationships and, if properly cultivated and enacted, they will suffice to improve the legitimacy of IS for the foreseeable future. Accordingly, the IS field must replace the notion of the fixed core with a metaphor of a center of activity (or life form) that builds identity and legitimacy for the field (Boland and Lyytinen, 2004).[4] This center is best seen as the free flowing give-and-exchange metaphor of the IS discipline as a *market of ideas in which scholars (and practitioners) exchange their views regarding the design and management of information and associated technologies in organized human enterprise*. Our essay concludes with an analysis of positions that IS scholars can take in relation to the market of ideas and how this institution can be strengthened as the true focal point of the IS discipline.

[3] We are indebted to Detmar Straub for this excellent quote
[4] This seemed to be also Keen's (1987 p. 3) idea when he wrote" "Our backgrounds, training and interests are very different. We must take that as strength, not a cause of argument."

The Anxiety Discourse

The concern about the IS field's academic legitimacy has been described elsewhere as part of an "anxiety discourse," in which evidence of inadequacy (e.g., disrespect from people in other academic fields) is traced to intellectual shortcomings in the IS field itself (King and Lyytinen, 2004). This anxiety discourse weaves through the IS literature, starting even before the field's founding in the late 1970s. This early anxiety was not specifically directed at theory, but rather at the expected difficulty of building computer-based systems that could actually provide useful information for managers (Dearden, 1972; Ackoff, 1967). Such apprehension might be unique to the IS field. The creators of artificial intelligence, which emerged at about the same time as IS, exuded optimism while facing challenges far more daunting than those of the IS field. Early pioneers in IS sustained their apprehension in spite of undeniable triumphs in the development of complex information systems, while the pioneers of artificial intelligence remained optimistic in spite of legendary failures to achieve the goals they set for themselves. There is clearly something more complex than academic legitimacy at work in the anxiety discourse.

The discourse shifted as the IS field became a more identifiable academic enterprise. It was soon recognized that the challenges of the IS field extended beyond the designing and building of systems, and into understanding the organizational mission, adoption, diffusion, and effects of such systems. These challenges could only be met by sourcing ideas from a wide array of intellectual perspectives, which researchers justified in the discourse as the adoption of intellectual standards of "reference disciplines" for IS-related inquiries (Keen, 1980, 1991). The reference discipline rubric suggested that the field lacked a coherent intellectual center, though its intellectual mission and knowledge needs did center around a shared interest in the applications of information technology to human enterprise (Kling, 1980). This initial juxtaposition of the IS field's intellectual mission and its sources – a center grounded primarily in praxis, and an intellectual periphery drawing intellectual capital from outside the field – formed the foundation for the anxiety discourse that was to follow. Soon, those within the field began to question the quality of the field's intellectual effort and status (e.g., Mumford, *et al*., 1985), and doubted that the field could retain its praxis-based center when other intellectually powerful academic fields came to recognize the importance of IT in organizations (Weber, 1987). This cast serious doubts on the theoretical status of the field, and precipitated tension between the praxis-oriented center and the intellectual periphery. Many came to doubt that IS could cohere sufficiently to survive (Banville and Landry, 1989; Maggi *et al.,* 1986, for a critical evaluation see Robey, 1996).

By the mid 1990s the anxiety discourse had evolved into an "orthodox" view that the field's survival depended on substituting its praxis-based core with a theoretical core grounded in well-defined intellectual constructs drawing on a model of research attributed to the natural sciences (Benbasat and Weber, 1996; Benbasat and Zmud, 2003).[5] Despite researchers' careful refinement of this position over a set of articles, the "core theory" view has not proved satisfactory in damping the anxiety. The discourse has remained stalled in a refrain that can be summarized as follows:

[5] This tension is also well documented in the endless debates over relevance vs. rigor observed as early as Keen's (1980) concept of reference disciplines and their relationship to the intellectual mission of the IS field. In this argument, rigor came from reference disciplines while relevance resided in the praxis-oriented core. This issue is beyond the scope of this essay, but it should be noted that preferring one attribute – in this case theoretical rigor over praxis-based relevance – lies at the heart of both stories.

> The central focus of the IS field is so important that other academic fields will want to appropriate it. Yet, the center of the IS field is theoretically too weak and diffused to generate sufficient solidarity to maintain a consistent research focus that would protect the field's long term viability. This weak condition generates disrespect from the more theoretically powerful fields that surround the IS field and that seek to appropriate the IS field's central focus. Manifestations of disrespect toward the IS field from these other fields constitute proof of the IS field's intellectual and theoretical inadequacy.

The perceived disrespect toward the IS field by other fields within management schools (e.g., attempts to push IS courses out of the required curriculum, efforts to deny IS faculty promotions) understandably generates feelings of inadequacy among IS faculty. The question arises as to whether these feelings of inadequacy should be attributed to *intellectual* and *theoretical* inadequacy, and specifically that the IS field is inadequate because it lacks a theoretical core at its center. As the following analysis shows, this is not a defensible attribution.

Legitimacy Through Theory?

The central question is whether the IS field's legitimacy rests on the presence of a theoretical core. In order to be convincing, the argument that legitimacy can be gained only through the possession of a theoretical core must be both valid and sound.[6] To examine this, it is necessary to establish the *truth conditions* for the elements of the argument, to analyze the *validity* of the inference involved in making the claim, and finally to determine whether the argument is *sound*.

Truth Conditions

Legitimacy is inherently relative and subjective: it cannot be conferred or attained through objective means. It has meaning only with respect to some socially constructed standard or expectation (Berger and Luckmann, 1966). It is very difficult to establish truth conditions for the assignment of legitimacy to any academic field. This does not mean that the concept of academic legitimacy is without meaning or import. Legitimacy reflects a field's institutional power, its problem solving capabilities, its methodical and cognitive distinctiveness, its applicable research outcomes, its value in education, and so on. Nevertheless, it remains unclear how legitimacy is established in general among academic fields (Andersen, 2000). This raises the first serious challenge to the argument that academic legitimacy is established by the presence of a theoretical core.

This challenge is shown by the fact that none of the commentaries about the IS field's need for a theoretical core have articulated exactly what is meant by the term "theoretical core," a point acknowledged by Weber (2003). Benbasat and Zmud

[6] A valid argument is a logical deduction of the form, derive q from p→q and p, e.g.: All men are mortal; Socrates is a man; Socrates is mortal. Validity is not enough: the argument must also be sound, meaning that both the conditional claim (p→q), and the antecedent (p) must be true for the consequent (q) to be concluded. An invalid argument cannot be sound, but a valid argument can be unsound, e.g.: All men are dogs; Socrates is a man; Socrates is a dog.

(2003) attempt to address this problem by defining the rules of exclusion and inclusion that define the properties of typical IS theory. However, since there is no objectively determined definition of a "typical" IS theory, this solution must depend on a widespread agreement among the IS field as to what that term means. In fact, this solution is not a description of truth conditions, but a plea for the IS field to come into agreement on terms that can be adopted as standards against which behavior can be measured. Currently, the field is far from agreeing on such terms, and many attempts to achieve agreement have failed to affect where the field is moving. Even if such an agreement should emerge, the agreement itself would be arbitrary with respect to the truth.

The philosophy of science is replete with discussions of these problems (cf. Chalmers, 1999; Radnitzky, 1968; Habermas, 1971; Rorty, 1979). Principles deployed to argue in favor of a specific legitimating measure immediately call into question where these principles came from and how they might be justified. It is noteworthy that these higher level principles cannot be derived analytically or empirically without an infinite regression of logical induction, wherein a first tier principle is justified by appeal to a second tier principle, the second tier principle is justified by appeal to a third tier principle, and so on (Lee and Baskerville, 2003). In the case of the IS field, there is no way to decide conclusively what counts as a theoretical core. The best the field can achieve is a specific, historical, inter-subjective agreement among its members as to what will count as a good theory, at any given time, backed by a set of redeemable *warrants* (Toulmin, 1958). The next step is to determine whether the IS field has redeemable warrants for any notion of the theoretical core.

The position most often invoked to warrant the need for a theoretical core is a version of Popperian analytic philosophy.[7] According to Popper (1968), academic fields make progress through refining and refuting theories that explain phenomena falling within the domain of the field. Theories must be subject to refutation (i.e., falsifiable), through which they can be rejected or refined. New theories emerge as a result of investigators' attempts to explain problems by alternative models, formulated within the scope of established theories.[8] Popper's analytic philosophy could potentially serve as a warrant for the centrality of a theory in legitimating the IS field, but it is only one point of view, and it has been severely criticized by other philosophers for logical and empirical weakness (cf., Schilpp, 1974; Feyerabend, 1982; Lakatos and Musgrave, 1970; Stove, 1982). It is just as reasonable to suggest that theories across academic fields vary significantly due to differences in social organization and prevalent research tasks, and that no uniform relationship can be established between a given field and a specific type of theoretical core (Whitley, 1984). Overall, the anxiety discourse is fundamentally and constantly challenged by the absence of clear truth conditions for judging the claim that the presence of a theoretical core will produce legitimacy for the IS field.

[7] The authors are indebted to an anonymous reader of an earlier version of this paper who explicitly used Popper to argue in favor of particular warrants, thus revealing the centrality of Popper's views to this discussion.

[8] Popper's view has other problems with respect to the argument for legitimacy as a consequence of a strong theoretical core. His view captures only nomological explanations, and excludes widely accepted theories such as Darwinian natural selection evolutionary theory. Moreover, his view does not clarify whether academic fields are defined by theories, or theories by fields, although he expresses the opinion that the former *ought* to be the case.

Validity

Assuming that reliable truth conditions could be established for the claim that a theoretical core is necessary for legitimacy, the argument must next be evaluated for its validity. The normal argument in favor of a strong theoretical core flows through the following complementary syllogisms:

(1) All x :TC (x) → LF (x) ***entails***	**(1b) All x: ¬TC (x) → ¬LF (x)**
(2) TC (e.g., physics)	**(2b) ¬TC (e.g., is)**
(3) LF (e.g., physics)	**(3b) ¬LF (e.g., is)**

Where x = Academic discipline
TC = having a Theoretical Core,
LF = having legitimacy as an academic field

Here syllogisms (1), (2) and (3) represent a set of logically valid deductions. A theoretical core implies legitimacy for a discipline: hence if a discipline has a core, it will also be legitimate. The argument thus far follows a valid form (*modus ponens*). The problem, however, comes with the syllogisms (1b), (2b), and (3b), which the core theory argument assumes- called the logical fallacy of denying the consequent. This suggests that a *lack* of legitimacy results from the *lack* of a strong theoretical core, and thus from the denial of the antecedent (theoretical core, *modus ponens*) the denial of the consequence (legitimacy) can be inferred. The chain (1b), (2b), and (3b) cannot be *entailed* from (1), (2) and (3), and is a logical fallacy. From the absence of something one can derive everything, which is tantamount to deriving nothing. Accusation (1b), "The lack of intellectual core implies the lack of legitimacy," cannot be logically derived from accusation (1) "A strong theoretic core confers legitimacy," rendering (2b) and (3b) inconsistent. The claim that the lack of a theoretical core deprives IS of legitimacy is invalid, and not worth further consideration. The syllogism (1), (2), and (3) remains valid (*modus ponens*), and in principle, gives support to the argument for legitimacy through a theoretical core if both (1) and (2) are true.[9] However, under these circumstances, the argument that a theoretical core is necessary for legitimacy cannot be established deductively, and can be made only by empirical induction. This raises the issue of soundness.

Soundness

Premise (1) implies an inductive generalization: all academic fields with a strong theoretic core are found to be legitimate, *and* that no legitimate field can be found that lacks a sound theoretic core. This is a factual accusation, depending only on the accuracy of the facts being claimed. It is difficult to demonstrate that all academic fields with a strong theoretic core are found to be legitimate: this requires a census of all fields and their theories as well as their status as legitimate. Since a single counter-example violates the induction and causes the argument to collapse, it is much easier to test the premise by finding an example of a field that is legitimate, but that has no theoretic core.

[9] We can also state that p →q, ┐q, therefore ┐p (*modus tollens*). This suggests that *if* a discipline does not have legitimacy it does not have theoretical core *assuming* that p →q is sound. This still requires vindication of the soundness of implication as elaborated below.

There are many legitimate academic fields that can be characterized by their focus of study, by the methods their members use, by the tendencies in their opinions or findings, and by their impacts on the thinking of those outside their field, but that have no theoretical core: classics, German literature, accounting, and history, to name a few. There are also many legitimate academic fields that have possessed numerous, fundamentally different theoretical cores across their histories. Early (Western) biology was entirely motivated by the belief that all life was created by God according to the Biblical creation story, and consisted of exploring and documenting the diversity of flora and fauna found in the Creation story. Jean-Baptiste Lamarck provided one of the first theoretical mechanistic explanations for diversity in the early 19th century, but it was not widely accepted by practicing zoologists and botanists. Charles Darwin's monumental mid 19th century theory of natural selection slowly became an accepted organizing rubric for biology, but has undergone dramatic reconstruction since that time and is still disputed even among biologists. Psychology emerged from incoherent speculation about the nature of mind in the 18th and 19th centuries, only to be transformed in the late 20th century when instrumentation enabled closer study of the relationship between brain and mind. Geology explained morphology and stratification solely through 18th century concepts of erosion, especially those grounded in the Biblical flood, until the mid-20th century advent of plate tectonics upended all previous theory. Despite this turmoil of theory, the legitimacy of these fields has never really been questioned. The presence of a strong theoretic core can sometimes be *useful* in establishing or sustaining legitimacy, but it is unsound to argue that having a theoretical core is necessary for that purpose.

Legitimate academic fields have long been characterized by theoretic instability over their histories (Kuhn, 1996). Indeed, the intellectual history of the academy itself is one of instability. Universities of the late medieval period were dominated by theology and philosophy. The Renaissance university branched into the "natural philosophy" of biology and physics. Biology differentiated into botany, zoology and bacteriology in the early-19th century, reorganized around the relative complexity of organic processes (biochemistry, molecular biology, physiology, developmental biology, ecology, evolutionary biology) in the mid-20th century, and underwent yet another reorganization focusing on mechanisms such as enzyme metabolism and molecular proteomics in the late 20th century. Physics branched into chemistry, geology and other specialties during the late 18th and 19th centuries, only to be transformed in the 20th century into hybrid fields such as earth systems science (geology, atmospheric chemistry, oceanography). Most interesting, physics and biology have re-converged in structural biology (crystallography, biophysics, molecular and macromolecular biology). Despite such turbulence, the legitimacy of these fields has never truly wavered.

Legitimacy Reconsidered

The foregoing discussion deconstructs the main argument that academic legitimacy depends upon the presence of a theoretic core. If the presence of a theoretical core does not make an academic field legitimate, what does? In light of the evolution of different disciplines and an analysis of mechanisms that have made them legitimate, three factors can be seen that account *together* for disciplinary legitimacy: 1) salience of the subjects studied, 2) the strength of results from the study, and 3) the plasticity of the field with respect to changing circumstances. While none of these factors alone seems to be enough for academic legitimacy, there are no legitimate fields

that do not have all these characteristics *simultaneously.*[10] We will look at each of these factors in turn to determine where the legitimacy of the IS field resides and what can be done about it.

Salience

The pragmatics of IS-related knowledge

Research and education embodied in collegiate IS programs – as in any processional field – are dependent upon the patronage of the larger society that invests in such programs in the expectation of long-term societal benefits. Fields that appear to be dealing with socially salient issues are more likely to be legitimized: they can claim resources because of their pragmatic legitimacy (Robey, 2003). As such, the salience in the IS field is about pragmatic legitimacy, which 'rests on the self-interested calculations of an organization's most immediate audiences' (Suchman, 1995, p. 578).

The IS field's origins can be traced directly to the very salience of its subject matter, not its theory *per se.* IS programs originated because modern business organizations needed IS professionals, and these organizations exerted pressures on professional schools (primarily in business administration and management) to produce them. Indeed, the early anxiety found in Ackoff (1967) and Dearden (1972) can readily be interpreted as a concern that the expectations of businesses eclipsed the abilities of both academic and practicing experts.

Salience in the IS field despite the recent downturn

Over the years these fears proved to be unfounded, and the success of information systems as a practice has been demonstrated beyond question. The salience of the IS field is still evident, as seen in the rapid build up of demand for IS professionals as the dot.com boom evolved. It was obvious to organizations that the future envisioned during that boom required many more highly trained IS professionals. Recruiters went wild, salaries skyrocketed especially in the North America, and IS programs rapidly added faculty and courses to meet the demand. The fact that the dot.com boom collapsed demonstrates that salience can be fickle, but the phenomenon of the boom illustrates how powerful a force business can be in legitimating any academic field.

Despite the repercussions of the dot.com collapse, the salience surrounding IS remains high in the future. Investment in IT continues to rise, and the application of IT to organizational functions continues to grow. In 2004, the salience of IT is still seen clearly in concerns about outsourcing and the offshore movement of high-value IT jobs. If these jobs were not important, and believed to be *increasingly* important, no one would care that they are moving. The Bureau of Labor Statistics estimates that job growth in the next five to ten years will show much greater demand for IT-related jobs than in any other area of skilled work.[11] This does not mean that the ecology of IT-related jobs will not change in the future. In response to this, the IS field must become more aggressive in finding ways to match its talent pool to the newly emerging salience at the center. Ironically, the threat that other fields might take over the salient center of the IS field has probably been reduced by the collapse of the dot.com boom and the perceived shift of IT jobs overseas.

[10] We are thus arguing for S&R&P→L. This naturally can be rejected by finding a counter-example like the one we demonstrated for the theoretical core argument.

[11] Cf. http://www.bls.gov/emp/empfastestind.htm

Salience as primary generator of legitimacy

To summarize, the IS field cannot protect its claim on salience by replacing the salience of its praxis with a core body of theory because theory has value *only* in reference to praxis. Building an edifice of theory at the center of the IS field might please observers from other academic fields who think that theory by itself is valuable, but those observers are not going to support the IS field with resources that they can just as readily claim for themselves. At most, they can offer or withhold their approval within the academic power structure. One must then ask why the IS field needs or should seek the approval of those observers, and whether this is the only way to obtain their approval. Other academicians have no resources of their own to confer on the IS field, even if they were willing to do so (and they are not). Their resources come from exactly the same place the IS field's resources come from: the larger society that sees value in the work being done. To the extent that these fields have power over the IS field, it is in their ability to wrongfully appropriate the IS field's salience and redirect the resources raised by that salience to their own ends.[12] Thus the competition between the IS field and other academic fields is ultimately not about theory at all – it is about the right to appropriate the social salience of the work being done, and the resources that perceived salience generates from the society at large. Attempting to substitute theory for praxis at the center of the IS field is actually likely to hasten the field's decline.

Strong Results

No field can capitalize on the salience of the issues it studies without providing sustained benefits from its effects. The IS field, like all fields, achieves this through the application of proven techniques that yield high-quality research and instruction.[13] Some of those techniques involve the generation and refinement of theory, and theory is often essential to the outcome, but the creation of theory *per se* is not the final game. Theory, to the extent that it has a role, is in the service of producing strong results. Strong results are the provably valuable consequences of research and instruction within the society as materialized in artifacts, behaviors, and expectations. Strong results are rather easy to demonstrate on the instructional side: smart people with good IS training usually outperform smart people without good training. This, of course, was the main original motivator for establishing the field (Ackoff, 1967; Deaden, 1972; Mason, 2004) combined with the salience of how to systematically offer information for organizational activities in ways that would harness the potential of the emerging "IT artifacts."

Strong results are also easily seen in new artifacts that fundamentally change social behaviors (like ground-breaking computer programs or technologies like operating systems or cellular phones). The IS field abounds with examples of such artifacts, ranging from tools for decision support, and group decision support to CASE tools, development methods, and large scale application systems (e.g. ERP), some of which have been influential while others have been less so. To search for rigor and systemic criteria in assessing and developing such artifacts has gained growing interest in the "sciences of the artificial" (Simon, 1996) through the development of design science and design theories (see e.g., Hevner, et al., 2004).

[12] Both authors have seen this happen as members of university committees and reviewers for research funding agencies.

[13] This is similar to what DeSanctis (2003) pointed out recently when she wrote: "Evidence of legitimacy of the IS field lies not so much in the establishment of organizations such as the Association for Information Systems (AIS), schools of information science, and university departments; instead, *the evidence of legitimacy lies in the actions of people within and between these organizations as they pursue their scholarly work*" (emphasis added).

It is harder to demonstrate strong direct results from research ideas and models due to the nature of research itself, but when this happens the impact on society can be deep – cases in point are the victory over the Ptolemaic view of the universe and the formulation of theories of relativity. In the IS field, demonstration of such theoretical advances is more difficult to pin down exactly, as reflected in the anxiety discourse. This does not mean, however, that the impact of the IS field in the theoretical plane is negligible. It has greatly advanced understanding of the impact and role of information in any organizational activity, how it relates to the design of artifacts, and how organizations operate as **socio-technical systems**. Such impact is also felt throughout different social sciences, as shown by the citation patterns of IS research within other fields (Baskerville and Myers, 2002). This does not mean, however, that strong results within IS have resulted in what Benbasat and Zmud (2003) call "cognitive legitimacy," i.e., that all IS theories are taken as granted by environmental constituencies and alternatives to IS have become unthinkable (Robey, 2003). At most, strong theories in IS have gained pragmatic legitimacy which rests as noted above "on the self-interested calculations of an organization's most immediate audiences" (Suchman, 1995, p. 578). Research that produces strong results demands long periods of sustained effort. Progress is possible only by pushing forward the frontier of the known, a bit at a time. The quality of the research process is tied to the quality of results produced. Academic fields that produce strong results typically adhere to and enforce high standards with respect to knowledge claims and the ways in which those claims are redeemed. There is no inherent value in the process itself; all value accrues through the results of the process.

The quality of the process might determine the quality of the results, but it is only an input to the results.[14] In the IS field, as with any field with praxis at its center, the processes of research must be aimed at improving the strength of the results that affect praxis. Along the way, the results often inform other areas of work beyond the IS field to create generalizable knowledge. This is a good thing, and should be welcomed and even sought, but it will not legitimate the field. It can, at best, enlighten those in other fields to the fact that results useful to them are arising from research in the IS field.

There is a persistent hint in the anxiety discourse to the effect that research tied too closely to praxis is not really research at all, or at least, that it is an inferior form of research.[15] This sentiment is probably the result of a more general confusion about the nature of research, and the dysfunctional distinction between *basic* and *applied* research. This distinction rightly discriminates between research results that are immediately applicable to some problem, and those that are not, but that have the potential to affect many problems in the future when more is known. The problem with the distinction is with the political interpretation that has evolved to favor "basic" over "applied" as truer, purer, and more worthy of praise. The primary function of this interpretation is to direct social resources away from obviously salient applied research to less salient basic research.

As noted earlier, truth claims related to legitimacy are social constructions, and the preference of basic over applied research is no exception. The preference has roots in the legacy of the medieval and Renaissance academy that preferred thought over

[14] This explains why scientists make public their methods in the process of producing the results. Quality control over process distinguishes science and scholarship from other forms of producing knowledge.

[15] This seems to be implied when Benbasat and Zmud (2003) call for theories based on nomological nets and argue for the value of a "dominant research paradigm."

practice, and even over empirical study. A more immediate story is found in the mid-20th century fable of the processes by which knowledge produces economic and social welfare. This fable can be seen in many sources, but the most widely cited is Vannevar Bush's 1945 report, *Science: The Endless Frontier.* Bush's linear model of the relationships between science, technology, and society is shown in the top diagram in Figure 1a: scientific discovery creates technological innovation, which then produces improved welfare.[16] This model is popular among some policy makers and among many scientists, but it bears little relationship to the facts. Considerable research in the history of science and technology suggests that the lower cyclical diagram (Figure 1b) is more accurate: technological innovation occurs largely apart from basic research, creates economic growth, and generates a surplus that can then be invested by society in basic research that assists but seldom directly causes technological innovation (Gillespie, 1957; Hall, 1963; Mathias, 1972; Rosenberg, 1992). The examples are plentiful. Advances in sea-travel in the 15th century forced the development of better astronomical theories. Problems with steam engines precipitated Carnot's work on thermodynamics. Difficulties in development of databases stimulated Codd's work on relational theory.

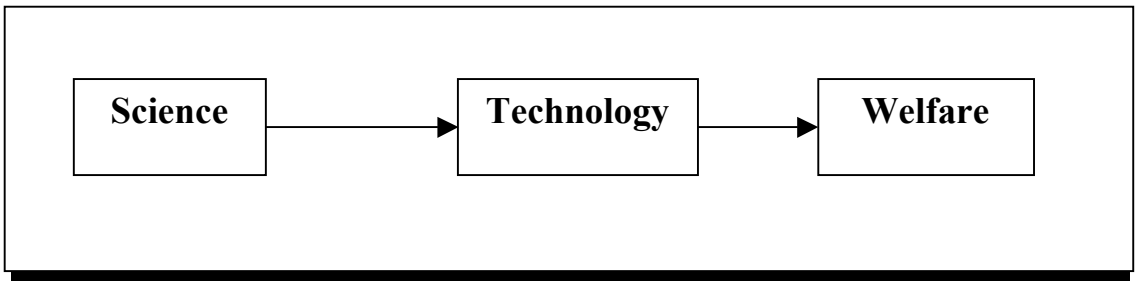

Figure 1a. Linear Model of Science, Technology and Society

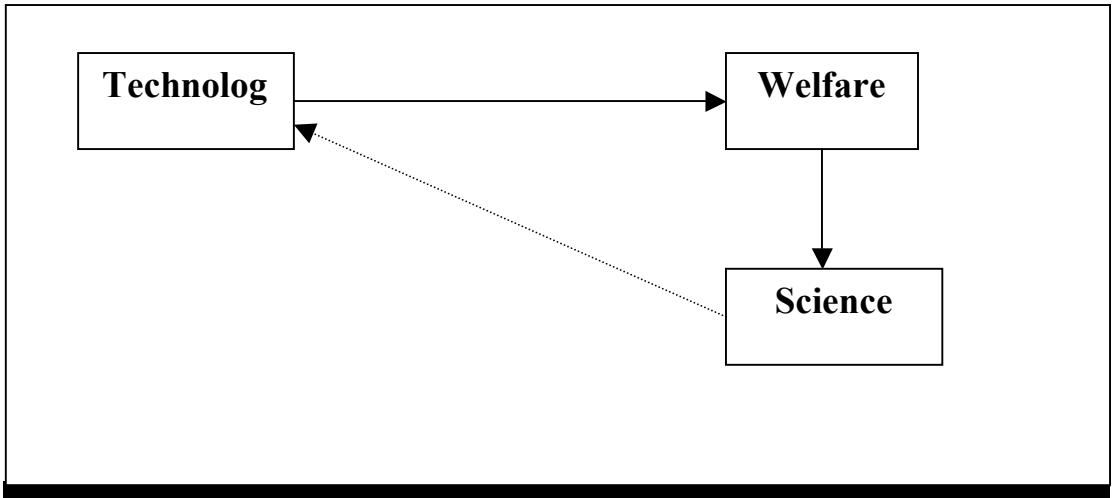

Figure 1b. Cyclical Model of Science, Technology and Society

The relationship between practice and theory as suggested by the cyclical model is much more complex and nuanced than assumed in simple linear models about scientific innovation. Donald Stokes has explored this issue in depth in his book *Pasteur's Quadrant* (Stokes, 1997). Stokes calls basic research the "quest for

[16] Bush had been President of MIT, but was serving as an advisor to President Truman. His report is credited with creating the momentum that led to the establishment of the National Science Foundation (Bush, 1945).

fundamental understanding" and applied research is "considerations of use." Each constitutes an essential dimension of a dynamic model in which research can be high or low on either dimension (Table 1). He cites Niels Bohr, a founder of quantum theory, as an example of one who contributed to fundamental understanding but relatively little to use, and Thomas Edison as an example of one who contributed to use but little to fundamental understanding. In his top quadrant, he cites Louis Pasteur as an example of one who contributed greatly to both.

Pasteur's work was often inspired by practical problems, the most famous of which was the spoilage of beer and wine. Food preservation in the early 19th century was primitive. The alcohol in beer and wine was a preservative, making these important foods, but they inevitably spoiled in a relatively short time after opening. Pasteur's development of a technique to heat the liquid to a point below boiling for a certain time (Pasteurization, later applied to milk and other products) solved the problem by killing the microorganisms in the liquid. Pasteur's fundamental insights in stereochemistry and microbiology launched modern biological chemistry. The point of Stokes' book is that the most noble research is not "basic" in the usual sense, but rather advances both use and fundamental understanding simultaneously.

Table 1. Pasteur's Quadrant (Stokes, 1997)

Research Inspired by		**Considerations for User?**	
		Low	High
Quest for Fundamental Understanding?	High	Pure Basic Research (Bohr)	User Inspired Basic Research (Pasteur)
	Low		Pure Applied Research (Edison)

Reprinted by permission of the Brookings Institution Press

As stated earlier, there is nothing wrong with the creation of solid theory in the quest for fundamental understanding in the IS field. On the contrary, this is an important endeavor, and the IS field's success in this endeavor will certainly help legitimize the IS field in academic circles. The critical thing, however, is to understand *how* improved theory achieves greater salience, and thereby to recognize the inherent limit of theory's contribution *per se* to legitimation. There is nothing special about theory: it is merely one component in the process of developing useful knowledge. The word arose from the Latin *theoria,* derived from the Greek *theros,* or "spectator." It has evolved in meaning over time to refer to a view of the world that constitutes speculation without reference to particular instances or practices. It is a "step back" from the actual phenomena to find a broader characterization that encompasses the instances at hand. It is useful *only* because it is a cost-effective means of making sense out of many particular instances and helps orient the researcher more effectively toward the world. The value of theory resides solely in its ability to make sense of what actually happens in the world. At its best, theory covers a great many

instances and many dimensions of those instances, thereby reducing the burden of understanding. In academic parlance, a good theory can cross academic boundaries and inform many different lines of inquiry.

In academic fields with praxis at their center, theory must serve praxis. For example, legal theories lay foundations for legal practice by clarifying and justifying ethical choices. In IS, theories provide ways of seeing into and intervening in socio-technical problems. Theory is only contributory to the development and constitution of the center, but it cannot be the center. Praxis-related theories sometimes arise from those working directly on the issues at the practical center, and they sometimes arise at the periphery of the field and are brought to the center. It does not matter where the theory comes from, except with respect to who gets credit for the contribution.

To summarize, the IS field's legitimacy depends on the strength of its results with respect to the praxis at the field's center. It is desirable, of course, that strong results at the center of the IS field inform other fields whenever possible, and that other fields appreciate the IS field for such contributions. It does not matter whether strong results are produced with or without theory. Most strong results are likely to be built on some theoretical components, but there is nothing necessary in this. There is no sense in presuming that theory can constitute the IS field's center and this *alone* will allow the field to remain salient.

Plasticity

Salience can be ephemeral, and for very good reasons. Once a major social problem is solved through research—or as Hughes (1993) states when the significant "reverse salients" that prevent the deployment or use of large scale technological solutions have been removed in the society—it stands to reason that the problem will no longer be socially or technologically salient-. For example, astronomy emerged as a key theoretical inquiry to overcome the reverse salients of the sea-fare, thermodynamics to overcome the challenges of building more efficient steam engines and so on. But these issues are no longer of significant theoretical importance in physics research.[17] The history of the academy demonstrates that the legitimacy of academic fields lies not in holding tight to the reasons for the field's emergence, but in keeping the field's center "plastic" by adapting to the shifting salience of the issues that might concern it. Only by keeping the field's center plastic will the field retain its capability to produce strong results, and with strong results legitimacy follows. This is no different with the IS discipline or any other field in management. Robey (2003), for example, promotes "adaptative instability" as a key strategy to ensure the IS discipline's legitimacy, while Galliers (2003) talks about the need for boundary spanning, critical reflection, and a wide range over related subject matter.

Plasticity is, however, only a correlate of legitimate fields, not a cause. Plasticity, in itself, does not produce legitimacy, although in itself it helps keep the field salient. But when smart people work on salient issues, the field can expect to produce strong results also in the future. Likewise, the IS field cannot sustain its legitimacy in the long run without being plastic. Despite its youth, the IS field has demonstrated this many times in its evolution, and the benefits of this plasticity to the field have been significant.[18]

[17] There are however some points where these old theories may need revision and salience. For example, the first Apollo launches required much more accurate calculations than Kepler's traditional laws to predict the movement of earth and moon.

[18] The often raised criticism of the IS field as being driven by "fads" can be interpreted also as a field's attempt to retain plasticity in the face of extremely fast change in its field of study.

Plasticity does not come easy, as change in academic world is not a simple matter, given the inherent conservatism of its institutions (Kuhn, 1996). The inherent conservatism of academic institutions is well-founded and justified: the costs of building and distributing academic knowledge are high. Therefore, academic institutions furiously resist any attempt to change their current organization and associated theoretical cores, believing that hard-won knowledge should not simply be pushed aside by changing salience in the issues being studied. In addition, academics know that strong results require tough and painful processes for making and redeeming claims of truth. As a result, academic change is always an agonistic struggle between established and emergent views in light of growing knowledge and changing social needs. The biographies of great scientists illustrate this as a universal experience—the greatness of scientific heroes comes precisely from overcoming resistance and fighting against the odds and prejudice inherent in prevailing scientific knowledge. Knowledge is built through an expensive and painstaking process of argument, postulation, repetitive testing, and successive approximation. Frivolous or politically motivated assaults on new and innovative ideas can stymie progress, or even result in setbacks. At the same time, it is appropriate to demand convincing arguments and evidence from upstarts. If new ideas succeed in taking the lead, they should be tested, redeemed, and found excellent. Therefore, legitimate and established fields have high standards for the establishment of fundamental knowledge; but they shed older fundamentals in favor of new when the standard is met. This concept of durability of standards speaks for strong theoretical cores in the short term—but in the longer term theoretical cores do not create legitimacy. What creates legitimacy is the capability to change and redeem any element of the theoretical lens that guides observation, explanation, and intervention (Kuhn, 1996). Established disciplines have always preserved their legitimacy by being plastic and responding to the needs and opportunities at hand.

Academic change seldom results in wholesale substitution of new for old, but instead builds a layered genealogy of knowledge that sustains a coherent focus for the field. For example, quantum mechanics and plate tectonics supplemented rather than replaced classical mechanics and erosion in physics and geology, despite their contradictory models and conflicting chains of evidence. Such change has usually been gradual in the history of science, but the pace seems to be accelerating with the rapid generation of new knowledge and changes in research practice.

Plasticity sometimes comes only with struggle. Computer science, like IS, is a relatively recent field that grew from a variety of academic roots. Like IS, it has struggled for legitimacy. In its early years it focused primarily on two areas: computability in the traditions of Alan Turing and John von Neumann; and application to practical problems such as ballistics calculation and code-breaking. These two foci were complementary. Processing and storage constraints in early computers encouraged efforts to improve performance; and theories developed around the analysis of algorithms, complexity, program correctness, and so on. The revolution in semiconductors and magnetic storage relaxed the earlier constraints, but computer science had already developed an identity and a theoretical core that was grounded in those early technology constraints. Political infighting sometimes resulted in the

---

We often see bad IS research that just follows fashion but some of the unique challenges of the field are its need for exceedingly high levels of plasticity due to the fluidity of its subject field.

stronger "theory" area pushing out the so-called "systems" area that included software and applications.[19]

This internal conflict in computer science reached a catharsis in the early 1990s with the publication of *Computing the Future: A Broader Agenda for Computer Science and Engineering*, a report of the Computer Science and Telecommunication Board of the National Research Council (Hartmanis et al., 1991).[20] The report argued that computer science must re-engage applications or face demise. Orthodox computer scientists protested the report, but it subsequently shaped the agendas of the National Science Foundation's (NSF) Directorate for Computer and Information Science and Engineering (CISE) and the Computing Research Association (CRA), especially following the stunning rise of the Internet and the World Wide Web. The idea continues to gain momentum: the NSF recently issued a report from a Blue Ribbon Panel, titled *Revolutionizing Science and Engineering Through Cyberinfrastructure,* that calls for the wholesale transformation of science and engineering research through the application of IT.[21] Its plasticity has prevented computer science as a field from being left behind in the revolution that it helped to launch.

Plasticity is a complicated matter for legitimacy because it potentially threatens identity. The IS anxiety discourse tends to equate academic identity and academic legitimacy, but they are completely different (King and Lyytinen, 2004). Identity is a necessary precondition for legitimacy, and plasticity, by threatening identity, indirectly threatens legitimacy. Yet, this threat is real only if narrow constraints are put on the issues to be studied. An important kind of identity accrues to those who are consistently open to new ideas, and who look for the systematic relationships between seemingly separate things. If the IS field can be described as the study of the design and management of information and associated technologies in organized human enterprise,[22] the resulting identity is both specific enough to be recognizable and broad enough to allow for healthy plasticity. The great strength of the field in the past has been its boundary-spanning ability. It could open the black box of information technology, and at the same time, move *beyond* the IT artifact to IT-in-application in human enterprise where the consequences of IT occur. This has remained the salient core of the IS field, and has sustained the growth of IS field for three decades. There is no reason to believe that this mission and associated plasticity will fail to sustain the field into the future.

Recent discussions about the future of the IS field reveal the strength of this tradition of plasticity. Intertwined with the call for a theoretical core has been a call for IS researchers to focus their attention on the "IT artifact" (Orlikowski and Iacono, 2001; Benbasat and Zmud, 2003; Massey *et al.,* 2001; Falkenberg *et al*., 1999; Bergman *et al.,* 2002). This call seems to spring from the need to take the praxis at the center more seriously: to focus carefully on the artifacts that are created and materialized in practice and the effects they have. The intent behind an increased focus on the IT artifact is laudable, as it is aimed at consolidating the work of the field and reinforcing the praxis at its center. Nevertheless, it would be a mistake to *predicate* the identity of the IS field on the IT artifact. This would unnecessarily narrow the borders of the

[19] More than one MIS group was formed in management schools by "systems" faculty exiled from computer science during this era of purges.

[20] The whole report is at http://www.nap.edu/books/0309047404/html/

[21] The report is at http://www.communitytechnology.org/nsf_ci_report/

[22] We use purposefully a broad definition here so that the term enterprise should be seen as a generic human activity (to conduct a way of life in a specific way)- not as a definition of an organizational form in which IT use takes place.

field in the direction of computer science, just as computer science is broadening its scope, and would run the risk of moving IS away from other areas that may become vital to the long-run salience of the field. Moreover, it makes no sense for an academic field to focus attention exclusively on any artifact because artifacts never deliver value in their own right. They are complementary assets in production, and their value cannot be understood without the context of their application.

Plasticity requires remaining flexible about the foci of study, and also about the meanings of the signifiers used to identify the field. As Wittgenstein (1953) pointed out, it is important to be careful when naming fields because the meaning of the words may break down later on. For example, the commonly used name "information systems" is too broad to establish a precise identity because it can encompass a wide array of subjects that could just as easily be claimed by computer science, library and information science, communications, journalism, or virtually any field that pursues information in a systematic manner (e.g., economics, political science). Adding an adjective such as "Management" to the front of the name narrows the focus, but the IS field already tried that and abandoned the "M" long ago, retaining it only in formalities such as the journal *Management Information Systems Quarterly*.

Similar naming difficulties have occurred in computer science. The oldest U.S. professional association of computer scientists, the Association for Computing Machinery, struggled for several years in the 1980s to shed its hopelessly old-fashioned name, "computing machinery." The members could not agree on a new name that would produce the acronym ACM, which had become the organization's *de facto* name. Thus they retained the old name and ignored the "computing machinery." Ironically, many leaders in computer science now feel that the signifier "computer" is too old-fashioned and prefer the broader "computing." Given that the names of academic fields age into placeholders for a set of plastic and fluid significations, it seems reasonable to accept the plasticity and fluidity of whatever rests at the centers of those fields.

Is There Nothing At The Center?

The title of this paper can be read several ways. First, it echoes the theme of the anxiety discourse that the IS field has no center worthy of the name—the falcon has become, in fact, a pigeon. As noted earlier, this theme can take two alternative forms. In one view, the only center worth having is a theoretical core that confers academic legitimacy--the falcon sees only the falconer. In another view, the field needs, but does not have, a consistent focus for its intellectual efforts--the falcon can see everything except the falconer. Both views spring from the essential tension, observed since the inception of the field, between its praxis-focused center and its intellectual periphery. The anxiety discourse makes the mistake of trying to move the periphery to the center. The IT artifact movement seeks to refocus and revitalize the field's relationship to praxis, and is at least consonant with the legitimacy requirements of salience, strong results, and plasticity.

The Market of Ideas as the Center

Another reading of the title implies that the IS field has been unnecessarily preoccupied with its center, and, therefore, the whole idea of having a center is a red herring. The wearisome and cyclical anxiety discourse makes it tempting to abandon the whole discussion, but that would be as misguided an action as over-investing in

the search for a theoretical center. The idea that there is something at the center, or that there *ought* to be something at the center, is obviously very compelling to any member of any field (King and Lyytinen, 2004). It is only human to seek something unique and shared with those close at hand, and IS people are as human as people in any other field. The needs of salience, the desire for strong results, and the quest for plasticity in the IS field preclude a center of fixed ideas or relationships, but they do not preclude a *center based on a kind of activity*. It is necessary to give up the metaphor of a center as a set of things and their fixed relationships to ideas, and replace that with a metaphor of a center as an activity (or life form) that builds strong identity and legitimacy for the IS field as time moves forward (Boland and Lyytinen, 2004).[23] This is accomplished by a free flowing give-and-exchange metaphor of the IS discipline as a *market of ideas.* The real center in *the IS field has been and will be constituted through a market of ideas in which scholars (and practitioners) exchange their views regarding the design and management of information and associated technologies in organized human enterprise.*[24]

For some, it might seem strange to consider a metaphor of a market as a center—a way to relate people, ideas, and artifacts--but there is much to recommend it. [25] The idea of a marketplace of ideas is by no means new.[26] Daniel Webster captured it beautifully in 1825 in a speech at the groundbreaking for the Bunker Hill monument in Massachusetts, wherein he talked about "…a vast commerce of ideas" consisting of "…marts and exchanges for intellectual discoveries" that brought great improvements in human welfare. His focus in that speech was the astonishing contribution of new knowledge to the human triumph "…over distance, over differences of language, over diversity of habits, over prejudice and over bigotry." The market had indeed produced remarkable changes in the world. By 1825 steam had begun to power transport, the cotton gin had introduced mechanized agriculture, vaccination had been discovered, artificial coal gas was lighting cities, the British Parliament had overpowered the monarchy, and democratic institutions had arisen from the American and French revolutions.

The market was by no means finished. By 1875 the Bessemer process had made cheap steel the backbone of the industrial world, the railroad industry had invented operations management, the telegraph had revolutionized communication, slavery had been abolished, compulsory education had been widely established, and higher education was becoming widely available.

Markets of ideas are ephemeral, but they support concrete actions needed for salient issues. They are remarkably fluid, which makes them capable of responding to rapidly changing conditions requiring plasticity. They have the power to resolve

[23] This seemed to be also Keen's (1987 p. 3) idea when he wrote" "Our backgrounds, training and interests are very different. We must take that as strength, not a cause of argument."

[24] This is similar to Desanctis' (2003) statement that the people and their "social life" make a field legitimate by enabling specific types of interactions on a set of chosen topics that lock in interested participants. We agree on this characterization but want to push the argument further by clarifying how dynamic, diverse and heterogeneous communities of inquiry can coalesce around the "market of ideas".

[25] Lest this seem an unnecessary empowerment of economists, it is important to remember that economists did not invent the concept of markets, and they do not own the concept. Economists merely started studying markets along the way, and despite real progress, they are far from complete understanding of how markets function. The markets being discussed here, which consist solely of information, are among the least understood.

[26] This concept is also close to Habermas idea of a "public sphere" and critical discourse in public as a condition for advances in technology, politics and society (see Habermas 1991, see also Toulmin 1972, Rorty 1979)

uncertainty, necessary for establishing strong results. Markets embody the notion that any fair exchange is a legitimate exchange, which guarantees salience and plasticity in the long term. An ideal market allows people to move value around without unnecessary impediment or cost. The center of a market is not the physical or virtual place in which it occurs (i.e., academic institutions), or the rules under which it operates (e.g., editorial policies). It is not even the particular things being traded (e.g., a particular theory or results). The center of a market is *the empowerment of the participants, through due process, to place their own values on the things being traded*. Where values align, transactions between ideas occur, and strong results, salience, and plasticity can follow.

The idea that the market of ideas forms the *actual* center of the IS field has many additional virtues for its praxis-oriented outlook. For one, it accommodates theory and praxis with equal respect, dispelling the notion that the two are exclusive. It thus reinforces the concept of Pasteur's Quadrant, legitimating *all* aspects of value in any intellectual exchange. This idea is also in line with generating a general body of knowledge (BoK) that helps address concerns about the use of IT in human enterprise (Klein and Hirschheim, 2003). Second, it explicitly recognizes that anyone may enter or leave the market at will, which is an established tradition of the IS field (DeSanctis, 2003). Rather than lamenting this fact, as the anxiety discourse normally suggests, the IS field should embrace it as a great source of strength as diversity and intellectual quality drive strong results. By allowing and invigorating new entrants, the market can respond more quickly to fundamental changes such as rapid technological improvement and the threshold effects generated when new technologies and techniques combine in disruptive ways. Finally, by holding the market as the center, each member of the IS field can seek and find value in exchanges with like-minded colleagues. If the market of ideas is the center of the IS field, it becomes irrelevant to ask what the theoretical center of the IS field is or ought to be. The answer to this question will always be whatever the market is working on at the time, and this might call on any number of theoretical perspectives.

What does the market of ideas mean for IS academics?

The "market of ideas" describes the modal center of the field, but it does not necessarily help individual IS academics locate *their* work in the field, or see how they can add value to the market while pursuing their professional goals. In an ideal world, IS academics would never have to identify or justify the location of their work in the market, because the market would resolve issues of salience, results, and plasticity in due time. Unfortunately, the world is not ideal, markets fail, and, therefore, each IS academic must find a reasonable place to stand when affiliating with a community of colleagues and pursuing career advancement. Given that each individual must make this choice from time to time, the question is not which choice to make but, rather, how to parameterize the options available to best exploit the market of ideas while honoring the need for security among individual scholars.

Table 2 provides one way for IS academics to approach this challenge. It suggests two dimensions that frame the political discourse in the conferring of academic legitimacy. The first is a personalized view of the future of the field. A personalized view sees the field primarily as a vehicle for one's career advancement, and thus seeks to advance the researcher's career within the empowered constructs of the field. In this view, the individual sees the field as *my* field and has a personal stake in its future growth and legitimacy. In a depersonalized view, the field is seen as an incidental aggregate of people with shared interests. In this view, the field is *a* field to which I happen to belong at the moment, but I am not bound to it as a career option. The second dimension involves a choice of whether to conform to the *status quo* in

the field in terms of its theoretical constructs and modes of inquiry as currently constructed. A conforming choice accepts the field's established theoretical biases and traditions as legitimate and authoritative, embodying the identity of the field and contributing to the field's legitimacy. A non-conforming choice considers established biases and traditions as products of a constantly changing mix of facts, theories, opinions, and beliefs that emerge in response to the research being done and the larger conditions in the society, such as the salience of the issues at hand. In the non-conforming view, biases and traditions are temporary conditions of an inherently unstable and emergent intellectual enterprise.

Table 2. Parameters of Individual Position in Academic Fields

	View of Self With Respect to the Field	
View of status quo	**Personal**	**Depersonalized**
Conforming	**Cell I** Stable and established academic fields	**Cell II** External commentary on the academic realm.
Non-conforming	**Cell III** Entrepreneurial innovator, not viable in the academy	**Cell IV** Unstable and emergent academic fields

Table 2 arrays these dimensions into a 2x2 matrix, with each cell describing the position an individual might take in relation to a field with a specific outlook on how he or she might approach the "market of ideas."

Cell I is the position most often taken by individuals in established academic fields that are stable with respect to their focus of study. The paradigmatic example of this might be physics. It is no accident that many commentators concerned about the lack of academic legitimacy in the IS field hold physics up as an ideal exemplar of legitimacy, and use it as a paragon for a legitimate academic field (Weber, 2003). Physics exhibits academic power and pride; what other discipline would unselfconsciously refer to itself as the "queen of the sciences?" On the other hand, physics entertains the luxury of studying something that is "out there," whether physicists are studying it or not. There is no danger that the physical world is going to change before physicists figure it out, and even the most self-satisfied physicist does not claim to be actually creating the physical world. Perhaps it is for this reason that the biases and traditions of physics change slowly and it has a strong and well organized body of theoretical knowledge--"core theories"—which are taught to each generation of aspiring physicists. As the noted physicist Max Born once claimed, theories in physics are never abandoned until their proponents are all dead, and the field advances "funeral by funeral."

Unfortunately, the models of science as applied to physics do not apply to most academic fields, and they certainly do not apply to fields that lie at the intersection of rapid technological and social change and a need to create the world anew, as Whitley (1984) observed in his classical study of the organization of academic fields (King and Lyytinen, 2004). They certainly do not apply to the IS field, and, for this reason, aspiration to develop a strong core for IS does not look like a good strategy.

There are, however, people in the IS field who would like to locate *all* IS research in this cell and, therefore, place strong IS theories at the center. They naturally can promote and trade these ideas in the "market of ideas," but in light of the earlier discussion of salience and plasticity, efforts to make the whole IS field look like physics are folly—they will not yield strong results and therefore not increase legitimacy, as a true market for ideas will not evolve.

In Cell II a scholar holds a depersonalized view of a field, but is attentive to its intellectual biases and traditions, trying to understand *how* or *why* they emerged. This cell is commonly occupied by people who do scholarly work *about* the field by following and commenting on it. Good examples of scholars adopting this position would be philosophers, sociologists, anthropologists, scientometricians, and others who focus on particular fields and their evolution as their primary intellectual interest. Often, some of these individuals have backgrounds as practicing scholars in those fields, but for various reasons have left that work behind. Well-known examples include Peter Medawar, a Nobel-prize winning biologist who became a philosopher and sociologist of science, and Thomas Kuhn (1996), a well-known physicist who became a still more famous sociologist of science. Individuals who pursue such work typically move toward academic programs in science and technology studies, and out of the mainstream academic areas that are the focus of their commentary. Most fields also have people who "reflectively monitor" their own work by adopting the hat of a Cell II scholar.[27] The utility of this effort for the field being studied depends on the intention of the commentator. There is an important difference between genuinely depersonalized commentary that seeks to reveal how a field operates, and the promulgation of prescriptions on how the work of the field ought to be done. Thoughtful commentary helps those operating in the market of ideas put their activities in a larger context, with the goal of improving the quality of individual trades. Prescription, on the other hand, attempts to tell individuals what they should and should not be trading, and thus weakens the power of the market.

Cell III is populated by individuals who take personal advancement within the field seriously, but who do not conform to the biases and traditions prevailing in the field. The best examples of such individuals are entrepreneurial innovators who take the knowledge they have gained in their research and move outside the academy to exploit it. Thomas Alva Edison is probably the most famous example of this genre of intellectuals (Stokes, 1997), but similar examples bound in the IS field. Leo, the first information system platform (Mason, 2004), was wholly developed by such entrepreneurs, some of whom later on became significant IS academics. People like Yourdon, Jacobson, Ross, or Scheer (one of the developers of SAP and the R3 approach) emanated from or have had close relationships with academia throughout their careers but did most of their intellectual work outside it. The academic realm has traditionally not felt comfortable engaging in such activity, and most countries have legal prohibitions against universities conducting for-profit activities in overt competition with private firms. An academic who wishes to run a company must usually leave the academy to do so.[28] These individuals are driven by salience and a desire for strong practical results, but they do not normally care about academic legitimacy, and they are not happy in academia. Nevertheless, they provide valuable ideas and results in the "market of ideas" with respect to salience. This also has happened in the IS field.

The individual in Cell IV has a depersonalized view of his or her field, and is a non-conformist with respect to established biases and traditions of the field(s). Many

[27] This essay constitutes an example of this practice.
[28] These conditions vary from country to country.

scholars in unstable and emerging fields such as the IS field find themselves in this cell *due to the nature of their intellectual mission*. The work in such fields requires boundary spanning in order to understand, explain, and create in ways that do not fit closely with well developed theoretical models within established fields in Cell I. Cell IV scholars must accept the fluidity and unbounded nature of inquiry at the emerging frontier, and often go against the established biases and traditions that reign in well established and confined disciplines. Academic life in Cell IV is risky, especially when there are no institutions to protect and legitimate the viewpoints of boundary spanners. Therefore, intellectuals in this cell face ridicule, criticism from arrogant persons, and a loss of career security. Such risks are unavoidable in unstable and emerging fields and have nothing to do with the lack of theoretical core, as the nature of the enterprise itself is about new and different contributions to the market of ideas, which accepts their indeterminacy.

Due to its youth and the nature of its intellectual mission, the IS field has most in common with Cell IV. The field arose as a rebellion against viewing the two faces of IS as separate social and technical systems. As a result, the IS field fought hard against the long-held opinion in the social sciences and management that information technology is only a minor and unimportant element in human enterprise and will continue to be so. Likewise the IS scholars have long challenged the dominant views among computer scientists that the engineering of IT artifacts does not need to take into account and understand the social and organizational issues surrounding computing.[29] As a result, the IS field has been an academic community at the social margins from its start. It does not have and, in fact, cannot afford the conditions of Cell I in order to prosper due to the salience and fast change in its subject grounding. Accordingly, Cell IV best characterizes the condition of most IS academics, especially those in academic programs such as management schools, surrounded by colleagues who believe it is their predetermined right to live in Cell I and to impose the expectations of Cell I on everyone else.

The Cell I politics happen always within the constraints of broader societal salience and, therefore, nothing in Cell I is sacred or uninfluenced by changes in Cell III and IV. When the dot com boom hit, the salience of IT changed overnight in the social sciences, and the rhetoric in many of these fields changed. They became suddenly plastic toward IT-related issues, and many people in those fields began to claim that IT, or information, was now central to their endeavors. Ergo, the IS people had potentially little to add to the discussion as they lacked the theory. This was recommended despite the fact that the history of the field witnessed a scholarly engagement on the topic over a 25-year period by people who had been many times educated as, e.g., economists or operations people! Suddenly IT was solely a "marketing" issue, or an "economics" issue.[30] Hence, theories in Cell I can be expanded and re-interpreted when salience demands that one do so. Now when the boom has gone bust, the fickle court of elite opinion is again in some of these fields that there is not much in the IT realm to worry about, and the old intellectual borders, in fact, make more sense.

---

[29] This point is made elsewhere (King and Lyytinen, 2004), but bears elaboration. It is telling that the National Science Foundation Directorate for Social, Behavioral and Economic Sciences funded very little research on the effects of computerization between 1970 and 2000. The vast majority of good social science and management research on this subject during that period was funded from the directorates responsible for computer science where it was still regarded with suspicion and at most tolerated.

[30] We are purposefully painting here a simplified account what truly happened as many people in economics, sociology, marketing etc. have now or even before the dot.com boom become our allies as new intellectual collaboration has been spawned within the IS field - an example of how it can work as a "market of ideas."

We see that there are several ways in which the market of ideas can be strengthened in light of the differences and complementary assets that each cell has to offer. The IS field will remain mostly in Cell IV as long as technological and social change is fast and the salience remains fluid. Therefore, any drastic move from this position is likely to be dangerous for the future of the field.[31] Yet, this does not mean that the field cannot benefit from closer relationships and new ways of relating the work in all cells in "the market of ideas." In fact, creating such connections and arenas has been and will continue to be a source of strength for the field.

Cell I offers to IS academics an opportunity for active engagement in rigorous theoretical work in the field where its intellectual periphery is refined, extended, and tested. Many of the recent statements in the anxiety discourse are constructive attempts to address the tension between the praxis-oriented center and the intellectual periphery, and to improve these connections. The mistake in these attempts is that they suggest this is the sole means to keep the field viable. Though the field has seen several scholars move to Cell III, there is still much to regain by forging better alliances with the visionaries and entrepreneurs. One of the authors has in the past suggested building better alliances as one means to increase the salience of the field and its capability to respond better to the research potential offered by fast technological development (Lyytinen, 1999). The value of this activity is enormous, if it is combined within the market with the quest for sustained rigor and abstraction in Cell I.

We feel that the field could also benefit greatly from better collaboration with Cell III scholars in field-specific research. Though much has been recently published on multiple topics dealing with the status and organizations of the discipline, there is a paucity of rigorous and systematic study of the evolution of the knowledge of the field and its legitimation mechanisms. Such studies would significantly improve understanding of the mechanisms that constitute the field and its future challenges while at the same time adding to the broader theoretical discourse around technology and society. The value from such inquiries is greatly improved if they increase our understanding of how the "market of ideas" operates and evolves.

Conclusion

The words of Yeats, "Things fall apart; the centre cannot hold," are an accurate description of what many in the IS field have recently observed. The goal of this essay is to show that this feeling is not because of what IS academics have done or failed to do in terms of theory; it is the nature of the work in which the field engages. IS academics are the falcons turning and turning in the widening gyre of the IT revolution. Therefore, it *is* hard to get respect and resources, as so many other falconers are watching and using the theory weapon for disciplinary violence (King and Lyytinen, 2004). This analysis shows that the tendency in the anxiety discourse to conflate the problems IS people face with the putative lack of academic legitimacy due to a weak theoretical core is both misplaced and dangerous. In its strongest form, the theoretical core argument draws on fallacious reasoning. In its weaker form, it is not grounded in careful empirical understanding of how theoretical core and legitimacy correlate in academic fields. The anxiety discourse is dangerous because it does not make the field more legitimate and at the same time makes the field move

[31] DeSanctis (2003), Galliers (2003), and Robey (2003) in their responses all suggest the same when they stress plasticity and action focus for the IS field.

away from the gyre of the IT revolution by stimulating behavior akin to that of a cargo cult.[32]

In light of the evolution of multiple disciplines, and by analyzing mechanisms that have made them legitimate, three factors *together* appear to account for disciplinary legitimacy: 1) salience of the subjects studied, 2) the strength of results from the study, and 3) the plasticity of the field with respect to changing circumstances. The IS field already has strength in its salience and its plasticity, both of which are driven by the gyre of technological change. Perhaps the real concern for the field is still a relative paucity of strong results. If so, the goal of stronger results is well worth pursuing, and better theory is likely to contribute to this goal. But the quest for better theory will contribute most *only* if it is understood to be one of many complements in the pursuit of strong results. It cannot be the sole focus, as this will place it at odds with salience and plasticity.

The IS field will make progress on all fronts, and turn and turn in the gyre, if it comes to see its center as a market in the service of the "vast commerce of ideas." The IS field should also take pride in the fact that it has played an important role in making this vast commerce of ideas possible. Like any other academic field, the IS field needs intellectual discipline. But that discipline will not be achieved by creating social conventions that define what is to be excluded and what is included, or establish rules about how members of the field must do their research. Discipline can come only from IS researchers themselves, interacting in the market of ideas that includes as part of its natural function the mechanisms for discriminating between strong and weak results. If markets work well, they lower the value on anything that is not worth much to those doing the trading. At the same time, markets often permit entry of traders with unusual wares that turn out to be of great value to everyone in the market, thus speeding the turn in the gyre. The IS field should treasure those cases where someone of exceptional insight persuades the community to go into territory no one previously thought of as IS. The center of the IS field should celebrate the diversity in methods *and* topics that join in the market, and let the market itself discriminate on intellectual quality—in the end this would best guarantee that the gyre expands as it turns.

References

Ackoff, R. L. (1967) "Management Misinformation Systems", *Management Science*, December, 15, 6, pp 147-156.

Andersen, H. (2000), "Influence and reputation in social sciences- how much do researchers agree?", *Journal of Documentation*, *56,5, pp. 674-692.*

Banville, C. and M. Landry (1989), "Can the field of MIS be disciplined?", *Communications of the ACM*, 32, 6, 48-60.

Baskerville, R. and M. D. Myers (2002), "Information Systems as a Reference Discipline", *Management Information Systems Quarterly*, 26, 1, pp. 1-14.

Benbasat, I., and R. Weber (1996), "Research Commentary: Rethinking "Diversity" in Information Systems Research", *Information Systems Research*, 7, 4, pp. 389-399.

[32] Cargo cults emerged in the islands of Melanesia after WW II when aboriginal natives equated acquisition of riches in the form of manufactured goods with the appearance of ships and airplanes that frequented the islands during the war. The natives began erecting shrines looking like ships and airplanes in the expectation that the rich "cargo" would materialize (Lindstrom, 1993).

Benbasat, I., and R. Zmud (2003) "The Identity Crisis within the IS discipline: Defining and Communicating the Disciplines's core properties", *MISQ*, 27, 2, pp. 183-194, pp. 183-194.

Bergman, M., J. King, and K. Lyytinen (2002)," Large Scale Requirements Analysis as Heterogeneous Engineering", *Scandinavian Journal of Information Systems,* 14, pp. 37-56.

Berger, Peter and T. Luckmann, (1966), *The Social Construction of Reality: A Treatise in the Sociology of Knowledge*. Garden City, NY: Doubleday.

Boland, R., and K. Lyytinen (2004), "Information Systems Research as Design: Identity, Process and Narrative", Forthcoming in IFIP WG 8.2. Conference proceedings "*Relevant Theory and Informed Practice: looking forward from a 20 year perspective on IS research*", July 15-17, Manchester, U.K.

Bush, V. (1945), *Science The Endless Frontier.* A Report to the President by Vannevar Bush, Director of the Office of Scientific Research and Development. Washington, DC: US Government Printing Office.

Ciborra, C. (1998), "Crisis and Foundations: an inquiry into the nature and limits of models and methods in information systems discipline", *Journal of Strategic Information Systems*, 7, 1, pp. 5-16.

Chalmers, A. (1999), *What is this thing called science?,* (3rd edition) Hackett, Indianapolis.

Dearden, J. (1972), "MIS is a Mirage", *Harvard Business Review*, January-February, 90-99.

DeSanctis G. (2003), "The Social Life of Information Systems Research", *Journal of the Association of for Information Systems*, 4, 6, pp. 360-376.

Falkenberg, E., K. Lyytinen, and A. Verrijn-Stuart. (eds) (1999), *Information System Concepts- An Integrated Discipline Emerging*, Kluwer.

Galliers, R. (2003), "Change as Crisis of Growth? Towards a Transdisciplinary view of information systems as a filed of study", *Journal of the Association of for Information Systems*. 4, 6, pp. 337-351.

Gillespie, C. (1957), *The natural history of industry*. Isis, 1957, No. 48.

Feyerabend, P. K. (1982), *Science in a Free Society*, London, New Left Books.

Habermas, J. (1971), *Knowledge and Human Interests*, Beacon Press, Boston.

Habermas, J., (1991), *The transformation of the public sphere*, MIT Press, Boston.

Hall, A. (1963). "The historical relations of science and technology". Inaugural lecture, London, 1963, as reported in Rosenberg, N., *Inside the Black Box: Technology and Economics.* Cambridge: Cambridge University Press, 1982.

Hartmanis, J. and H. Lin (eds) (1991), *Computing in the Future: A Broader Agenda for Computer Science and Engineering*, National Academy Press, Washington D. C.

Hevner, A., S. March, J. Park, and S. Ram (2004), "Design Science in Information Systems Research", *MIS Quarterly*, 28, 1, pp. 75-106.

Hughes, T. P. (1993). The Evolution of Large Technological Systems. In W. E. Bijker & T. P. Hughes & T. J. Pinch (Eds.), *The social construction of technological systems: New directions in the sociology and history of technology* (pp. 51-82). Cambridge: MIT Press.

Keen, P. (1980), "MIS Research: reference disciplines and cumulative tradition", *Proceedings of the 1st International Conference on Information Systems*, Philadelphia, Dec 1980, pp. 9-18.

Keen, P. (1987), "MIS research: current status, trends and needs", in Buckinham, R.A., Hirschheim R., Land F., Tully C (eds), *Information Systems Education: Recommendation and Implementation*, British Computer Society Monogrpahs in Informatics. Cambridge, Cambridge University press, 1-13.

Keen, P., G. W. (1991): "Relevance and Rigor in Information Systems Research: Improving the Quality, Confidence, Cohesion and Impact", in Nissen H-E., Klein H.,

Hirschheim R. (eds): *Information Systems research: Contemporary Approaches and Emergent Traditions*, North-Holland, pp. 27-49.

Klein, H., and R. Hirschheim (2003), "Crisis in the IS field? A Critical reflection on the state of the discipline", *Journal of the AIS,* vol 4, Article 10.

King, J., and K. Lyytinen (2004), "Grasp and reach", accepted for publication *MISQ*.

Kling, R. (1980), "Social Analyses of Computing: Theoretical Perspectives in Recent Empirical Research", *Computing Surveys*, 12,1, 61-110

Kuhn, T. (1996), *The Structure of Scientific Revolutions*, Chicago University Press, 3rd edition, Chicago.

Lakatos, I., and A. Musgrave (eds) (1970), *Criticism and the Growth of Knowledge*, Cambridge, Cambridge University Press.

Lee, A., and R. Baskerville (2003), "Generalizing Generalizability in Information Systems Research", *Information Systems Research*, 14,3, pp. 221-243.

Lindstrom, L. (1993), *Cargo Cult: Strange Stories of Desire From Melanesia and Beyond.* Honolulu, University of Hawaii Press.

Lyytinen, K. (1999), "Empirical Research in Information Systems: On the Relevance of Practice in Thinking of IS Research", *MIS Quarterly,* 23, 1, pp. 25-28.

Maggi, L., R. Zmud, and J. Wetherbe (Eds) (1986), *Proceedings of the Seventh International Conference on Information Systems*, December 15-17, San Diego, California.

Markus, L. (1999), "Thinking the Unthinkable: What happens if the IS field as we know it Goes Away?", in W. Curry, R. Galliers (eds) *Rethinking Management Information Systems*, Oxford University Press, 175-203.

Mason, R. (2004*), "*The Legacy of LEO: Lessons Learned from an English Tea and Cake Company's
Pioneering Efforts in Information Systems", *Journal of the Association for Information Systems,* Vol 5, Issue 5.

Massey, A., B. Wheeler, and P. Keen (2001), "Technology Matters", in Dickson G., DeSanctis G. (eds) *Information technology and the future enterprise*, Prentice Hall, New Jersey, pp. 25-48.

Mathias, P. *Science and Society, 1600-1900.* Cambridge: Cambridge University Press, 1972.

Popper, K. (1968), *Conjectures and Refutations*, Harper&Row, 1968.

Mumford, E., R. Hirschheim, G. Fitzgerald, and T. Wood-Harper (1985), *Information System Research Methods*, North Holland, Amsterdam.

Orlikowski, W., and S. Iacono (2001), "Desperately Seeking the "IT" in IT research- a call to theorizing the IT artifact", Information Systems Research, June 2001, pp. 121-134.

Robey, D. (1996), "Research Commentary: Diversity in Information Systems Research: Threat, Promise and Responsibility," *Information Systems Research*, 7, 4, pp. 400- 408.

Robey, D. (2003), "Identity, Legitimacy, and the dominant Research Paradigm: an alternative prescription for the IS discipline", *Journal of the Association of for Information Systems*. 4, 6, pp. 352-359.

Radnitzky, F. (1968), *Contemporary Schools of Metascience*, Liber, Gothenburg.

Rorty, A.(1979), *Philosophy and the Mirror of Nature,* ,Princeton, Princeton University Press.

Rosenberg, N. (1992), "Science and technology in the twentieth century", In In Dosi, G., Gianetti, R., and Toninelli, P.A. (eds.), *Technology and Enterprise in a Historical Perspective*, Oxford: Clarendon Press, 63-96.

Schilpp, P. A. (ed) (1974), *The Philosophy of Karl Popper*, La Salle, Illinois, Open Court.

Simon, H. (1996), *The Sciences of Artificial*, MIT Press, 3rd edition, Cambridge, Ma.

Stokes, D. E. (1997), *Pasteur's Quadrant: Basic Science and Technological Innovation*, Washington, DC: Brookings Institution Press.

Stove, D. (1982), *Popper and After: Four Modern Irrationalists,* Pergamon Press, London.
Stowell F., and J. Mingers (1997), "Information Systems: an Emerging Discipline?- an Introduction", in Stowell F, Mingers J. (eds) *Information Systems: An Emerging Disicpline*, McGraw Hill.
Suchman, M. (1995): "Managing Legitimacy: Strategic and Institutional Approaches", *Academy of Management Review*, 20, 3, pp. 517-610.
Toulmin, S. (1958), *Uses of Argument*, Cambridge University Press, Cambridge.
Toulmin S. (1972), *Human Understanding*, Clarendon Press, Oxford.
Weber, R. (1987), "Towards a Theory of Artifacts: a paradigmatic basis for Information Systems Research", *The Journal of Information Systems*, 1,2, pp. 3-19.
Weber, R. (2003), "Still desperately seeking the IT artifact", *Editors Comments MISQ*, 27, 2, pp. iii-xi.
Whitley, R. (1984), *The intellectual and social organization of sciences*, Oxford, Clarendon Press.
Wittgenstein, L. (1953), *Philosophical Investigations*, London, Basil Blackwell.

About the authors

Kalle Lyytinen is Iris S. Wolstein professor at Case Western Reserve University. He serves currently on the editorial boards of several leading IS journals including, AIS journal, Information Systems Research, JSIS, Information & Organization, Requirements Engineering Journal, and Information Systems Journal. He has published over 150 scientific articles and conference papers and edited or written eight books on topics related to system design, method engineering, implementation, software risk assessment, computer supported cooperative work, standardization, and ubiquitous computing. He is currently involved in research projects that study the impact of 3D modeling techniques on architecture and construction, design and use of ubiquitous applications in health care and logistics, and the diffusion of broadband wireless services and the impact of standardization processes on these processes. He is also developing a high level requirements model for large scale systems. He teaches mobile business and digital law classes with some faculty at the law school.

John Leslie King is Dean and Professor in the School of Information at the University of Michigan. His research concerns the relationship between technical and social change, especially with respect to information technology in complex organizations and institutions. He has authored more than 150 articles and books based on his research, and has served as Editor-in-Chief of Information Systems Research and associate editor for many other journals. He currently serves on the Baard of Directors of the Computing Research Association, the Advisory Committee of the Directorate for Computer and Information Science and Engineering of the National Science Foundation, and as a Senior Scientific Advisor to NSF. Prior to joining Michigan he was on the faculty of UC Irvine.

Journal of the Association for Information Systems
ISSN: 1536-9323