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Muddling Along to Moving Beyond in IS Research: Getting from Good to Great

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Abstract

In this article, I argue that the IS field seems to be doing well when evaluated with sociometric techniques. However, while the progress of our field is commendable, we might have reached diminishing returns in the way we conduct research with our current modus operandi. Given that we are dealing with the most important phenomena of our time, I believe that it is time to become more ambitious and expand our impact to other domains and disciplines by creating more enduring and impactful research. I argue that four key dimensions on which we should place emphasis include: our institutionalization of a certain genre of research, monistic theorizing of our phenomena, the focus on questions for which data is easier to access, and our unwillingness to deeply engage with reference discipline theories. Addressing these through individual and collective efforts can help us expand the frontiers of our knowledge product and create broader value.

***Keywords**: Information Systems Field, Impactful Research, Field Contribution, Research Genre, Research Institutionalization, Building Theory, Reference Discipline Theory.*

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1. Introduction

Around six years ago, a group of doctoral students and I used a sociometric analysis technique to analyze whether information systems (IS) research was contributing to (or drawing from) classical reference disciplines such as computer science, organizational science, management science, and evolving reference disciplines such as marketing and economics (Grover, Ayyagari, Gokhale, Lim, & Coffey, 2006a). We found that IS was contributing back to the classical reference disciplines but not to the evolving ones. We concluded with an optimistic tone that the field has indeed progressed and will continue to offer some degree of intellectual contribution that other disciplines value. In the same issue (*JAIS,* Vol. 7, Issue 5), Wade, Biehl, and Kim (2006a) also conducted a sociometric analysis and came to the opposite conclusion—that IS is not a reference discipline in itself. Additionally, each set of authors critiqued the others' papers (Grover, Gokhale, Lim, & Ayyagari, 2006b; Wade, Biehl, & Kim, 2006b). Together, we attributed the disparate results to the use of different baskets of journals and different assumptions regarding the field as a reference discipline. A collective conclusion was that, based on citation statistics, IS seems to be contributing back to the sub-disciplines of classical reference disciplines that pertain to IS, but perhaps not to the overall disciplines.

Regardless of sociometric method used, I think it is quite clear that the IS field has progressed well. We have looked to reference disciplines to benchmark the quality of our theory and methods, and we have embraced these improvements in our institutional structures. As a result, I would argue that IS research in the top journals is now comparable with the best in other business disciplines. We could do another few dozen introspective studies to validate this claim. In doing so, we would undoubtedly find that the trend along any metric used to assess our theoretical and methodological prowess would be significantly positive (Grover, 2012).

Can we conclude, then, that IS is now on the right trajectory and, barring some temporary infatuation with incremental usage models, that we are indeed evolving appropriately? Can we say, based on the current trajectory, that our research will reach a critical mass of content that other disciplines will find difficult to ignore? After all, we deal with arguably the most important resource of our time and we seem to be studying relevant problems rigorously. I think that, while there is reason to be optimistic about the underlying positive results of sociometric analyses, these technical metrics might be insulating us from being aware of a dubious undertone that can spawn vicious circles. Subliminally, there seems to be a danger that is congruous with this special issue's theme. We are not sufficiently expanding the frontiers of our research[1]. On the contrary, our fondness for reference disciplines and theories, while clearly beneficial in terms of the academic quality of our work, might have closed the field to new ideas. Therefore, while we can claim success in demonstrating higher validity of our models, the models themselves lack innovation. We have the potential to fundamentally extend other fields in a significant way and yet we fall short. It is counterintuitive that, in achieving the aspiration level of high-quality research (as can be measured by technical introspective metrics such as citations or measures of methodological rigor), we might be reaching the point of institutionalizing mediocrity.

If I, as an interested observer, survey the landscape of IS research, I am puzzled. There are many special issues and specialized conferences that deal with various evolving topics. When I flip through pages of our journals, I see numerous box models and statistical tests. Each model might be interesting to me or useful if I engage with it. However, consolidation of these models is difficult. It seems that we have a fragmented field with evolving topics and numerous models. Everyone, this author included, is looking for a new "angle"—whether it be a new reference discipline theory, a twist on an existing model or an exciting practical question. Thus, we have numerous papers with distinct angles that are difficult to consolidate. Each paper might be doing good "quality" research, but, as a field, if we are unable to consolidate our knowledge, are we really able to expand our frontiers or have we boxed ourselves into a way of doing things? Below, I argue for the latter and take an extreme position, simply to make the case that, despite our progress, we need to make some changes. Please note that this is not a critique of our

[1] The term "expanding the frontiers of IS research" refers to creating knowledge that is deeper, more enduring and impactful, and can consequently be applied to diverse contexts as well as other fields in our digital environment. It is consistent with the theme of the Special Issue, but I take a more general tack on knowledge expansion rather than specific domains.

research—it is indubitably getting better. But it is time to ask whether we have reached a point where we are paying too high a price for the "quality" we aspire to. I see this as a natural aspect of evolutions and revolutions as the field progresses. Have we reached a point of diminishing returns with our modus operandi? I argue that we might indeed have reached this point. Specifically, that (1) our field is fragmented and knowledge consolidation is difficult, and that (2) we lack innovation in our ideas. To truly expand frontiers, we need to change in order to alleviate both these issues.

I see four underlying problems that might inhibit our ability to conduct truly groundbreaking work. First, we have institutionalized a genre of research that might close us to truly innovative thinking[2]. Second, we have largely used monistic theorizing that results in our seeing phenomena a certain way. Third, we have economized data collection, which precludes examining big problems. And fourth, we have placed adopted theory on a pedestal, which constrains how we conduct our research. Below, I elaborate on these four problems and suggest directions that are admittedly easier to recommend than to follow. However, I do believe that despite our success, ultimately, changing our mindset and softening our institutional structures will push us forward. We can and should try to truly expand the frontiers of our research and impact the world through other domains and disciplines.

2. Genre Institutionalization[3]

There is a paper style that falls in the comfort zone of most editors and reviewers. This genre has strong motivations and theoretical backrounds (largely from other disciplines) that provide a structure and logic for hypotheses. We contextualize the abstract theory from other disciplines by making it amenable to the IS phenomena we study. In some cases, the role of IS is simply to moderate exogenously, while, in others, we adapt the constructs from the theory to the IS context. We then present our methods, their standard tests, and build a set of implications for research and practice. This style might seem reasonable, and I suspect many would argue that it epitomizes what constitutes good research in our field. However, in following this genre, to a large extent, we have outsourced the crux of our innovative ideas. If we frame our problems through the lenses of theories from psychology, strategy, organizational behavior, economics, and so on, and then contextualize them into our box models and test them empirically, where are the new ideas in this chain? Clearly, there is value in leveraging knowledge from other disciplines, but must we continue to do so uncritically? Because we look for "angles" for our research, each research project interprets the theories differently for their IS context. This creates box models that are different, even if they evolve from the same highly abstract theoretical perspectives. Consolidating the models to indicate knowledge and progress in a stream then becomes a challenge. Our tools for consolidating research are still quite blunt.

So what is the solution? We need to build flexibility into the genre. Too many editors and reviewers are comfortable with this form of research. It makes it easier to assess papers and reject non-conformers. However, alternative genres that are open to pure theorizing or pure presentation of empirics (without theory) can allow us to be far more flexible in creating and taking ownership of our knowledge. Such alternative genres preclude everyone from being both a theoretician and empiricist, which allows for greater optimization rather than satisficing in the way we unitize our knowledge products. Importantly, they allow us to push the frontiers in constructing our own theories and typologies of information and IT that travel well to other disciplines and into new domains that can benefit from our scholarship.

[2] Having new "angles" in research papers offers novelty, which is a part of innovation. However, offering something that has not been done before, such as a new construct, measure, or method does not necessarily translate to richer and more impactful knowledge.

[3] These arguments have benefited from discussions with Kalle Lyytinen

3. Monistic[4] Theorizing

A second and related issue is that we tend to align ourselves with external disciplinary theories. After investing in a theoretical perspective, we look to frame our phenomena around that perspective's constructs or logic. Therefore, the same phenomenon (e.g., IT outsourcing) might be examined using five different theoretical perspectives in five different papers by five different teams. This blocks vibrancy of discourse, consolidation of knowledge, and innovation. While we can meta-analyze these five papers to try to understand what we have learnt, the approach often offers limited yield due to the incommensurability of the research models and constructs.

To truly engage with a phenomenon, we should either develop indigenous theory or use multiple theoretical lenses in a supra-additive manner to study it. This does not mean that we cherry pick constructs from each theory and place them in our model. That would be additive. If transaction cost economics and the resource-based view provide different predictions about our phenomena, then, by asking "why", we are engaging the theories. It is in the space of this tension between theories that new and deeper notions emerge, perhaps even new theory that we own (see Grover, Lyytinen, Srinivasen, & Tan, 2008). However, if we do not take on the challenge of looking at phenomena through multiple lenses in the same paper, then our monistic theorizing will lead to greater homogenization. We will tend to see our phenomena similarly, in ways dictated by the dominant theory. This could increase instantiations of the same theoretical perspective and reduce theoretical innovation. The number of studies that have used the same antecedents to IT adoption behaviors is a case in point. Many of the studies only vary with respect to the IT adopted. While useful in establishing stable factors, we can do better by engaging theories in a supra-additive manner. For instance, DeSanctis and Poole (1994) generate new insight into IT impact by blending institutional and decision making theoretical perspectives. Such approaches allow greater innovation in the field and a greater propensity to be able to expand the frontiers of our research.

4. Input Adaptations

As a field, we adapt in response to changes in the environment. Strong fields make adaptations that put them in a position of strength to cope with, adapt to, or even change their external environment. In the IS field, there have been changes in institutional pressures such as higher standards for tenure. There have been dramatic changes in technologies that catalyze the field. Correspondingly, the field has adapted to these changes through individual behaviors or through collective actions (i.e., via AIS). For instance, increasing co-authorship is one form of adaptation. Another is to try and use familiar theoretical frames to study new technologies. Despite these adaptations, when I look at the field, I'm not convinced that all our adaptations move the field forward. For instance, are we addressing the interesting questions of our time? IT is playing a transformational role in organizations and industries. There are fascinating issues pertaining to these transformational roles. However, there seems to be more research on technology adoption and usage than on the broader transformational implications of technologies. My observation is not intended to deprecate issues of deployment of IT. Clearly there are critical questions here. But I wonder whether the difficulty in examining organizational and industry changes due to challenges in obtaining data is causing us to make adaptations that miss the target. In other words, are our adaptations focused on trying to minimize inputs to our research (i.e., reducing effort in obtaining data) rather than on maximizing output (i.e., answering broad, important questions)? Let me reiterate. Individual and group level research is important, and it is relatively easier to obtain sample data by surveying individuals or setting up experiments. However, as a field, are we doing too much at one level of research, and too little at another? Do we need to strive for more balance? Are publication pressures precluding us from examining the really important issues of our time? Or is this simply a tradeoff we make between rigor and relevance?

[4] Refers to viewing our phenomena from a single theoretical perspective

If this is indeed a problem (and I suspect it is), we need to recalibrate. This could occur naturally through open engagement of the community in our forums and colloquia. However, the inertia and knowledge investments of stakeholders, incentive and evaluation systems, and proclivity of gatekeepers to sustain the status quo could prevent natural adaptation in a timely manner. The field could lose credibility and impact. Therefore, we need to continually assess whether we are addressing the critical questions of our time, and, through tools such as special issues, editorial policies, common data banks, we should steer the field toward them. It is through such introspection, awareness, and action that we can establish our intellectual engine in areas of broader importance in the digital age, and thereby expand the frontiers of our research.

5. Theory on a Pedestal

When papers in the IS field draw from external disciplines, the adapted theories tend to be at a high level of abstraction. For the most part, we place these theories on a pedestal and treat them as immutable. Therefore, when we test hypotheses pertaining to IS phenomena based on the logic of external theory, our data is largely testing a manifestation of this theory. If a hypothesis is unsupported, then we rarely challenge the theory involved. More often, we attribute the result to a measurement or other methodology issue and recommend future research to rectify it. In sum, we only examine one-way interactions between the external theory and our context, a point made by Oswick, Fleming, and Hanlon (2011). This precludes us from challenging assumptions or controversies around the theory in its own field. Every model represents an instantiation of a theory as a constraint.

In order to alleviate the fragmentation this causes and promote innovative theory, IS researchers should relax the reference discipline theory constraint. By examining the reverse arrow, how the IS context changes the theory itself or challenges theoretical assumptions, we can not only build better theory but also contribute back to the reference discipline. This reverse influence can take place at the construct level, assumption level, or, more fundamentally, the logic level. If IS changes the logic of an abstract theory, it can have profound implications for indigenous theory and enrich the original theory. For instance, a typical one-way interaction involves importing TCE logic to predict governance structure in an organizational dyad involving an IS. The two-way interaction truly recognizes the embeddedness of IT in the context and examines how TCE predictions would change if IT relaxed the bounded rationality assumption of TCE (Malone, Yates, & Benjamin, 1987). In considering two-way interactions, rather than ignoring the assumptions, boundary conditions, or controversy in the reference discipline, the field imports all of it and engages with it. This desirable level of engagement can result in richer theories with higher content indigenous to our field and with greater propensity to expand the frontiers of our research and fundamentally extend other fields.

Table 1 summarizes the central tenets of these arguments, which includes possible directions and sample papers that are consistent with the type of research that I allude to.

Table 1. IS Research Issues, Risks, and Directions				
Issue	**Key question**	**Downside risk**	**Possible directions**	**Sample papers consistent with direction**
Genre institutionalization	Have we have institutionalized an acceptable research/paper genre?	Create rigorous research that lacks innovation	Be open to other genres: Novel theorizing (no empirics)	Nolan (1979) developed an intricate stage theory from limited data that spawned a stream of hypotheses-based empirical work Burton Jones & Gallivan (2007) provide a new foundation for system usage in organization that links levels of analyses
			Pure empirics (limited or no theory)	Wattal, Schuff, Mandviwalla, & Williams (2010) present a largely descriptive study of data and patterns on the use of social media and presidential elections that opens directions for future research. Gordon et al. (2010) empirically examine the market value of voluntary disclosures concerning information security and suggests avenues for future empirical and theoretical work.
Monistic theorizing	Do we examine phenomena through singular or additive theoretical lenses?	Homogenization of our phenomenon	Engage theories in a supra-additive manner	Sambamurthy, Bharadwaj, & Grover (2003) integrate perspectives in strategy, entrepreneurship, and digitalization to frame information technology as a digital options generator for firms. Kappos and Rivard (2008) take 3 perspectives on culture and integrates them into a model of the relationships between culture, the development and use processes, and an information system. DeSanctis and Poole (1994) blend institutional and decision making perspectives to generate unique insight into the implementation and impacts of information technologies.
Input adaptations	Do we engage in topics that reduce our effort?	Miss the transformational issues of our time	Also invest in data sets that examine questions pertaining to broader units of analyses and ITs transformational issues	Lucas and Goh (2009) extend Christensen's theory of disruptive technologies in examining the case of Kodak and why they failed. Agarwal and Lucas (2005) advocate for macro studies rather than micro studies that examine the transformational impact of information technologies.
Theory on a pedestal	Do we challenge received theory?	Focus on testing rather than creating new knowledge	Import controversies and assumptions to holistically engage the IT context with theory.	Malone et al. (1987) describe governance implications when IT challenges the bounded rationality assumption of transaction cost theory. Fichman (2004) uses real option logic to challenge valuation models in innovation, particularly pertaining to contemporary information technologies.

In sum, I think we have done well to raise the profile of our field. If we continue on the current trajectory, we will create good work that is comparable to other business disciplines, but it will be a conservative fragmented effort. It will become increasingly difficult to consolidate our knowledge and the spark of new ideas and frames will become rare. We might also miss the critical issues in IT that are transforming human enterprise today because institutional forces (normative, mimetic, and coercive) are fostering a certain genre of work that closes the loop to innovative ideas. As a result, we rarely engage our phenomena at a supra-theoretical level in order to create the fertile ground for launching bolder ideas. We minimize our input effort by conducting research that is incremental and doable, and we treat our adopted theories as sacrosanct, rather than challenge them. Table 2 summarizes this thinking.

Table 2. Expanding Frontiers of IS Research

	What should be done?	How this expands frontiers of IS research
Research institutionalization	Allow journals to publish work where theory is not constrained by the need for data and where data is not constrained by the need for theory.	• Greater creativity in theorizing • Specialization of research roles • Better knowledge that can be applied to new domains • Focus on building stronger theories of IT, information and knowledge.
Monistic theorizing	Look for theoretical tension between theories and try and resolve it with new ideas.	• Greater creativity in theorizing • Better knowledge that can be applied to new domains.
Input adaptations	Create structures and data sets that examine broader units of analyses (organizations, industries, country, society).	• Better knowledge that can be applied to new domains • Address the bigger transformational questions of our time.
Theory on a pedestal	Import and *engage* theories from other disciplines by asking how IS phenomena can change the theory itself.	• Greater creativity in theorizing • Better knowledge that can be applied to new domains • Greater contribution back to areas important to reference disciplines.

6. Conclusion

This paper does not intend to disparage IS research or its use of reference discipline theories. They have served us well and will continue to do so (Niederman, Gregor, Grover, Lyytinen, & Saunders, 2009). However, I believe that we are now at a point where we need to carefully consider whether following the same path will yield the enduring, substantive, and transcendental knowledge (Weber, 1999) that can address the fascinating issues in our evolving digital environment.

This special issue aims to provoke and challenge the thinking in the IS field. My earlier study and other sociometric approaches demonstrate that the field has been successful in upping the quality of its research. However, while this is a remarkable accomplishment for a young field, there are indications that we might be reaching a point of diminishing returns by getting accustomed to doing things a certain way, thereby stagnating our progress. There is no reason for this. Information systems is at the center of human enterprise today and we have the tools to deal with profound and important questions. We also have the privilege of being in a rare community that is receptive to a variety of different epistemological and ontological approaches. While we have invested well in examining tradeoffs and synergies between rigor and relevance, I believe that it is time to start paying closer attention to our broader knowledge system. This would include questions about the knowledge we are collectively creating and whether it is sustainable and impactful, how well we use the talent in our field , and the extent to which we are creating research that is valid but fails to address important issues of our time. Clearly, the profound and broad impact of the digital environment gives us an opportunity to not only create innovative models for our field, but also to extend other fields in meaningful ways.

As I indicate earlier, the directions for change suggested here are easier to recommend than to follow. However, as stewards of the field, through awareness and discourse, we can slowly chip away at these changes at an individual level. Institutional changes can be slowly catalyzed by editors and other gatekeepers. If we do so, we can move in a direction that promotes groundbreaking research, which will truly expand the frontier of our knowledge.

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