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Communications of the Association for Information Systems

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Value of IS Research: Let's Not Talk Crisis – but We Can Do Better

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Abstract:

I argue that the Information Systems (IS) field is at a crossroads, not a crisis. Across a variety of metrics, the field has progressed fabulously. The quality of our journals and the research they embody is on par with other business disciplines. However, our course of drawing from reference disciplines, creating different instantiations of models, having everyone engage in theory and empirics, and limiting our actionable implications – might be creating an impediment for us. We need to expend more energy in dealing with bigger questions that characterize contemporary digital environments. This will require some thoughtful discussion and introspection – so that we can fight against the institutional forces that limit our value proposition.

Keywords: Information Systems Field, Discipline, Crossroads, Institutionalization of Research, Scripted Research, New Directions, Introspection, IS Field Success.

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I. INTRODUCTION

As a discipline, we study the Information Systems (IS) resource in organizations. Clearly, if we debate the target of our investigation, there would be little disagreement regarding its importance. Information, information technologies and the systems they create are arguably the most important resources of our time. This makes our emphasis on studying the embeddedness and impact of these systems on individuals, groups, organizations, markets and societies important too. So, if we are dealing with important topics, why do we have so many anxiety discourses? [Hassan, 2014] claims that it is due to our value proposition as seen by ourselves and others. It seems that we are not sure about whether the product we produce is uniquely valuable or even of the right constituency to benchmark that value.

There is some truth to this. Many of our introspective debates are targeted at the value question. Value could be reflected in cumulative tradition, relevance and even issues of identity, core and legitimacy. As a field, we do disagree on these issues and we have set up institutional structures that try to alleviate this uncertainty. For instance, unlike many other fields, we tend to take a defensive posture to our reviewing. We write longer reviews of papers and have larger numbers of reviewers. A defensive review posture demands greater justification of our position and larger numbers to "get it right" on average, and we still have extremely low acceptance rates in our top journals. Contrast this with some disciplines in the sciences that are far more consistent in their treatment of research and have strong self-selection of submissions, fewer reviewers, shorter reviews and higher acceptance rates.

However, while I do agree with the basic premise of Hassan's article and most of its content, it falls into the same trap of adding to the anxiety discourse. Perhaps this is an issue of framing. I am far more bullish about the progress we have made as a field [Grover, Straub and Galluch, 2009], but I also believe that we are at a point where we need to change our approach in order to continue to be successful [Grover, 2012, Grover, 2013].

I would argue that the IS field has done remarkably well since Peter Keen's admonition in 1980 [Keen, 1980]. If we put the efficacy regarding use of reference disciplines aside, the field shows progress across so many dimensions that characterize the anxiety discourse. If we examine the rigor of research methods, there is tremendous progress in the approach to methods, tools, philosophies, matching of methods to problems, engagement in method debates, etc. If we do a "brute force" assessment of all the collective abstracts of our work, we will find that it generally adheres to consistent boundary conditions. We are consistently examining "data and information systems, their development (modeling), management and strategy, and how they are related to organizations, processes, decisions, and users" with a strong emphasis on performance [Lim, Rong and Grover, 2007]. Our papers are far more rigorous in building underlying logic than those in the 1980s and theory is central to our vocabulary. There is evidence, albeit controversial, that our research is being used by other disciplines [Grover, Ayyagari, Gokhale, Lim and Coffey, 2006]. Furthermore, through ongoing debates in our forums, we are highly sensitized to the "so what" questions in our research – indicating a growing recognition that we should be addressing relevant problems as a necessary condition to our research.

Institutionally, we can also be bullish. Our institutions like AIS are robust; our major journal impact factors (JIF) are comparable with the best journals in business disciplines, and our community is far more global than it ever was. We surely have problems, but which discipline does not? All business disciplines struggle with similar issues of value. Such discourse can be healthy as long as it is constructive and actively used for ways of improvement. Circular arguments of anxiety, however, sap energy from the field and are not healthy [Grover et al., 2009].

So, whether we mechanically look at sociometric analysis or frame the discourse through different lenses, I think that we should all be in agreement, at a minimum, that the field has made progress across a variety of value metrics. However, I do believe that we are at a crossroad (not a crisis). We have learned, improved and can continue to muddle along. However, we have the opportunity to change and start addressing bigger questions. Hassan is right when he discusses the distinction between the value created for the researcher as different from the value created for those outside our community. I believe that we have indeed evolved toward creating a competent research system that gives us credence in the marketplace of business disciplines. So, while it serves our internal needs, does it create good knowledge for external constituents? That is a distinction that we can ignore or choose to embrace as we move forward.

II. IMPROVING VALUE

Below, I present four areas where we can do better. These reflect Hassan's ideas, but are framed differently.

Incrementalism - Working With the Script

What is preventing us from dealing with big ideas? There are major transformations in IT going on – but most of our research doesn't take on the big questions. As we have argued elsewhere, this is because through our learning process, we might have locked ourselves into a comfortable script for doing research [Grover and Lyytinen, 2013]. This involves picking an interesting problem and framing it with an abstract reference discipline theory. We then build our testable model that contextualizes the theory with respect to our phenomena. This mid-level model is empirically tested and subsequently enhanced with additional constructs. Our journals demand this – theory, model and data – all in one package – styled in a particular order. This script makes it easy for the system to manage – as authors and reviewers can look for consistency in the sequence, good constructs, good logic, strong validation, etc. However, there is no innovation in the sequence, the received theory is rarely challenged and the big questions are not addressed. We don't have the liberty of dealing with big questions or even more basic research within the script. So, we get rigorous research with limited impact. Our colleagues from other disciplines would look at our research and probably give it a nod of approval. But, by exclusively working in this mode, we limit ourselves in the creation of value.

Different Instantiations of Models

Every one of our papers has a model. In many cases the models are incompatible since we often interpret abstract theories differently or modify abstract constructs differently for our mid-level models. So, for example, five studies of outsourcing might be using different terminology and logic. How do we consolidate this knowledge to really see how we have contributed to knowledge and created value? It's tough. Meta-analysis helps, but it cannot compensate for such incompatibility. So, even within our current modus-operandi for research, we need to place more emphasis on the front-end of construct development processes. Instead, our emphasis has been on the back-end, where we have evolved and learned how to test for reliability and validity of our constructs. We do this mechanically now. However, the point at which constructs are formed, interface with theory, and measures are developed, remains weak. Results are just as invalid for poorly developed constructs and conceptualizations that statistically exhibit high "discriminant validity" as they are for the converse. Many journals do not consider simple construct development sufficient for stand-alone papers. I think in their zeal for focusing on more substantive theoretical work, they have thrown out the baby out with the bathwater. Rather than dissipating energy on incompatible models, robust conceptualizations that are strong and widely accepted create the tools for us to build better value through consistent and cumulative research. We should work toward creating solid foundations for our field through a battery of robust constructs, particularly those pertaining to the embeddedness of information systems in social contexts.

Forcing Everyone to be a Master of All Trades

Most of our papers have both theory and data. We seem to be forcing everyone to engage in both theoretical and empirical work in order to fit the mold of our typical research publication. But, this work takes different skill sets. The great abstract thinkers in our community are "forced" to be constrained in their theorizing by empiricism in order to publish. Good empirical methodologists are forced to build complex theoretical explanations for idiosyncratic results. So, in some ways we are sub-optimizing in our labor market. In doing so, perhaps there is latent value that can yet be unleashed if it were not for our institutional expectations and structures.

Broad Abstract Social Implications vs. Concepts in Action

I also see our typical knowledge product derive "implications for practice" at the back end. Sometimes these implications seem to be forced. Most of the time these implications involve broad social implications that revolve around constructs in the model. They lack the specificity required for use – which involves simpler ideas and interventions as processes or guidelines that organizations can follow. The question of usefulness to practice requires a layer of effort (another "means-ends" cycle) that our journal and institutional processes are not incentivized to deal with. There may be more value, at least for the practitioner constituency, hidden in our research, if we could translate our broad social implications to concepts in action.

III. MOVING FORWARD

I could be accused of painting the value issues with broad brush strokes. Perhaps that is true. I do believe however, that we have done well as a field, but we can do better. If we engage in discussion across these value issues, there are solutions to be had. Some of them require changes in institutional structures; broadening the types of papers that we accept (e.g., pure theory or pure data papers), or creating formal relationships, e.g., Management Information Systems Quarterly (MISQ) and Management Information Systems Quarterly Executive (MISQE), to form

conduits from our research to practice. Others require a corresponding change in the way we do research. For instance, challenging reference discipline theories or creating interesting abstractions from observations (like Richard Nolan did with his stages of growth model) can stimulate novelty and debate [Nolan, 1979]. If we have greater ownership of the framing and ideas, we can push toward addressing the bigger questions. Even within our script, working on better front end conceptualizations of core constructs that can be used as building blocks for rich IS theories can promote improved efficiency in accumulating knowledge.

Hassan indicates IS research should add value by focusing on originality (questions other fields don't address), active research (change discourse), and making evident what is not. The mechanisms to do these require loosening of institutional structures, acceptance of papers that fall outside the script, better utilizations of our field's talent pool, and incentives to build specificity needed to put our concepts in action. This can leverage the potential knowledge in our community and enhance value for both internal and external constituencies.

So where does that leave us? There are some in our field who are willing to discard much of what we have learned and push for radically new approaches. Now, why would we do that unless there is something fundamentally wrong with what we are doing? Building models, building theory, defining and measuring constructs, etc. is not the problem. We can do better; leverage our unique individual skills, sharpen our constructs, broaden our knowledge ecology, and move away from the exclusivity of scripted research to figuring out ways to create knowledge that will excite. Softer institutional structures and sensitivity to these issues can help us move toward greater value research.

I would say that IS research has been through three phases. In Phase 1, as an emerging field we explored IT and its manifestations and struggled to find our boundary conditions as we accommodated (and welcomed) migrants from other fields. In Phase 2 we honed our methodological skills by both broadening our repertoire of tools and tightening the way we formulated and examined problems. In Phase 3 we emphasized building our theoretical tools by tightening our models and their underlying logic. In both phases 2 and 3 we copiously look toward more advanced disciplines for guidance. Now, IS is at a crossroads as we enter Phase IV. In many ways the first three phases were more about building the discipline, creating value for ourselves – and moving to par with other disciplines. Now that we have created a good system, honed our theoretical quality, tightened and broadened our use of methodologies – we can start changing things toward building good knowledge. We have the opportunity to aggressively leverage the critical importance of the target of our investigation and start building unique value not only for ourselves but also for a broader constituency of external stakeholders!

On a final note, I would like to add that anxiety discourses do no field any good if not accompanied with constructive ways of moving forward. After all, as a socially constructed field, if the stakeholders are negative, the field comprising these stakeholders is negative, and the source of the anxiety becomes a self-fulfilling prophecy. We have too much going for us to let that happen.

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