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Editorial Comment: A Prospectus on the IS Field

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Ⅰ. Introduction

This editorial asserts in its title that it is a "prospectus." What is a "prospectus"? In finance, a prospectus typically involves statements and facts about the health of an institution, one that in particular is being held up for potential investment. Theoretically, the prospectus presents the pluses and minuses so that likely investors can evaluate the risk (Naturally, it can also be biased in overstating one or the other element in the risk equation).

In this editorial I am going to write such a prospectus and thereby take a "flyer" by speculating on the past history of the information systems (IS) field and what this means for its future. At this onset of this endeavor, it is important to note that we are not talking about the IS field as a whole. The entire field consists of both practitioners and academics, each having their own specialties and their own domains. They certainly interact, and in many more ways than many people realize [Straub and Ang, 2009]. In this editorial, I will focus only on the IS academic side of the house.

One of the reasons for doing this is that, in my opinion, there is much less negotiating room on the practitioner side of the house in the following sense. The field of IS practice is strongly affected by the new opportunities offered by emerging technologies. When cloud computing became a major force in the IS world, it was critical that IS practitioners investigate it and deploy it where they saw advantages. This need to follow the trends has been an important part of the IS practitioner world since the onset of computing in the 1950s.

With the possible exception of IS design science researchers, IS academics are (or should be) more driven by theory than technological trends. If a theory has to be dramatically altered every time there is a new technology on the scene, it is a poor theory. Strong theories are good for decades if not centuries or millennia. So theoretical development occurs over long periods of time rather than year-to-year, quarter-to-quarter, or even month-to-month, the time scales that describe how often emerging technologies

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need to be scanned.

IS academics, thus, are focused, compulsively some might argue, on explaining WHY events occur and HOW causation plays itself out. They do this primarily through theories. Theories offer explanations that are not evident in simple empiricism although there is certainly a place for sheer empiricism in scientific work. Sheer empiricism can provide a baseline or a background for critical theoretical work. This is why there is a designated place for a theoretical, empirical work among the categories of IS work, at least in *MISQ Quarterly* (see descriptions of submission categories at MISQ.org).

In brief, IS academics concentrate their energies on explaining what is occurring in the world of IS practitioners. Anytime their work can aid practice in being more efficient or effective, researchers are naturally delighted to have this kind of impact. But their personal rewards come mainly from advancing theory into new blue oceans of intellectual space [Straub, 2009].

Ⅱ. A Rapid and Possible Agile History of IS Academic Profession

Where has the IS academic field come from? And what has it accomplished? A quick look backwards finds the information systems (IS) discipline [also known by various acronyms such as information technology (IT), information and communications technologies (ICT), and computer or business information systems (CIS and BIS respectively)] in an enviable position in the scientific world [Grover *et al*., 2009]. Begun at a period in technological history when computers were first emerging as instrumental in societies worldwide, the field developed out of early efforts to create an academic discipline around the building of these systems and their impacts on organizations, individuals and whole political systems. Whereas there is a friendly debate over the exact birthplace of IS, by the 1970's and 1980's there is little doubt that relevant, recognizable pockets of growth were springing up in North America (e.g., Minnesota, Arizona, Texas, and Vancouver), Europe (e.g., Scandinavia, Germany, and the UK), and Austral-Asia (e.g., Singapore, Hong Kong, Australia and Korea). A quick Google scan of institutions offering IS programs and having on staff faculty who specialize in IS topics would cement the proposition that the field is widespread globally, with strong centers of growth in countries with emerging scientism like China. Membership in the primary professional society of the field, the Association for Information Systems (AIS), has increased from a charter membership base of about 1000 to 4 thousand by the year 2014. It now has representation in 90 countries around the world, with many student chapters across these regions.

The hallmark of scientific disciplines is the coalescence of ideas and the advancement of these ideas in academic journals [Abbott, 2001], and by the late 1970's the field had introduced a set of journals like *MIS Quarterly* (*MISQ*) and DATABASE and somewhat later the *Journal of Management Information Systems* (*JMIS*) and *Information Systems Research* (*ISR*). Numerous other journals have increased the footprint of IS work, some of these journals being sponsored by AIS. The *Journal of the Association of Information Systems* (*JAIS*), for example, has worked its way into the 4th position of the most highly ranked IS journals [Lowry *et al*., 2013].

Academic/practitioner journals allow faculty and those in IS practice to publish work that speaks to the best practices in the field. We have a number of outlets for this kind of valuable work. *MISQ Executive* was spun off of *MISQ* to offer an outlet for those whose work had key implications for practice and was, at least, intriguing to academics for possible future theorizing. *CAIS* has regular articles that also contribute in this way as does *Communications of the ACM*. This is healthy in the field has created respectable venues for work that speaks directly to practice.

Many scholars in the IS academic field are enjoying the benefits of high citation ratings. There are citations of numerous, top SI scholars being cited over 30,000 times according to Google Scholar. I will not mention the names of these individuals so as not to unduly embarrass them, but it takes little effort to discover who these scholars through simple runs of software such as Harzing's "Publish or Perish" (download free at www.harzing.com).[1)]

Research Centers focusing on information systems topics are another point of pride. I mention only a few to make my point. MIT's Center for Information Systems Research (CISR) is perhaps the oldest of these and, after over 30 years, is still held in high esteem today by major firms and public sector agencies throughout the world. The practice-oriented scientific work that comes from our field's research centers is truly noteworthy and remarkable.

Finally, conferences. The field has been blessed with numerous extremely successful conferences. The International Conference on Information Systems (ICIS) celebrates its 35th well-attended conference in Auckland New Zealand this year 2014. Absorbed under the auspices of the AIS, ICIS is the international conference that is supplemented by annual conferences in Europe and Africa (the European Conference on Information Systems or ECIS) and conferences in Asia (the Pacific Area Conference on Information Systems or PACIS), not to mention the Americas Conference on Information Systems (AMICS). One could mention many other specialized conferences such as the annual Dewald Roode Workshop on Information Systems Security Research (IFIP WG8.11/WG11.13) and other excellent niche conferences like DESRIST (International Conference on Design Science Research in Information Systems and Technology) and the Workshop on Information Systems Economics (WISE), usually held before ICIS.

One could literally write a tome on the accomplishments of the IS field in the relatively few decades it has been prominent in universities around the planet [Grover *et al*., 2009]. IS scholars have made major forays into cybersecurity, healthcare informatics, ecommerce, governmental systems, and so forth and these are areas that hold the potential for being of critical importance to societies.

Ⅲ. Toward the Future: Correcting the "Lack"

Since the current editorial comment has the term "prospectus" in its title, it cannot merely enjoin

1) Readers are encouraged to explore in this manner and consider interesting studies in IS history that could emerge from gathering such data over various periods of time.

the field to rest on its laurels and not embrace serious change in any way. When I began my term of office as Editor-in-Chief (EIC) of *MIS Quarterly* in 2008, I positioned what needed to happen at the journal in this same way [Straub, 2008]. In *Walden*, Henry David Thoreau reminisces about the springtime "busk" of the New England tribe of Mucclasse Indians. Once a year this tribe would burn all of its belongings in the center of their villages as a sign that life needs to be renewed or it will stagnate. This is an exercise that is hard to carry out since it means abandoning some of the hard-fought treasures of the past. Nevertheless, it needs to be done to invigorate the field.

Where do we lack?[2] An easy way to address this issue would be to talk about topics that we need to dwell on a lot more, to such an extent that we are viewed by society at large as the source of the most interesting insights into what has happened to date and where the world needs to go. These are all fairly obvious; they will surely morph over time, but at least there should be some long term currency of the following: (1) healthcare informatics; (2) big data and data analytics; (3) agile development; (4) digital strategy; and (5) cyber security.

Pointing out that these areas have become "hot" in the field over the last decade and seem to have "legs" to them in that they are still drawing special journal issues is not the full story, however.[3] It is perhaps too obvious that we should develop these areas much further. Isn't there another, more inspiring perspective, perhaps?

I would like to address the "lack" in the field in another way. My strong belief is that we need to extend what we have been doing into much riskier areas and to challenge the thinking of other disciplines and society as a whole in our theoretical development. Let me elaborate further on this below.

First, I claim no originality in this line of thought. A number of prominent IS scholars have been raising similar points for years. I will mention their efforts here primarily for the purposes of scholarly attribution.[4] If I have mangled their logic, I humbly apologize.

To whom am I referring? Some of the most innovative methodological work in this vein is by Jae-Kyu Lee and Shirley Gregor. They have presented these ideas in numerous workshops and have received a spirited reaction from audiences. Their argument is that many/most of our theories are axiomatic. Axiomatic in mathematics means that a series of relationships are self-referencing. The proof is not outside, but rather within the equations and set of variant mathematical relationships. In the IS academic world, this translates into theories that are self-evident, intuitive, and not informing. It suggests that neither the exogenous nor endogenous variables (and especially their mapping between themselves) are exciting. "Exciting" is a judgment call, certainly, but the essential message is clear enough. Our theories need to have surprising elements in them or they will be labelled as trivial by other disciplines

2) Please keep in mind in the following comments that these are only the thoughts of a single member of our community. They have no more (or less) currency or value than that.
3) e.g., Chen *et al*. [2012], Mahmood *et al*. [2010], Grover and Kohli [2012].
4) In fact, there are many others who could also be mentioned but the purpose of this editorial will be met by this shortened attribution list.

or by practice. We need to move away from these kinds of expressions and into riskier and less well-worn intellectual territory.

Varun Grover and Kalle Lyytinen have also been challenging the field to step outside of its comfort zone and theorize at different levels that is now being done. Once again, they have been presenting this work in public venues around the world. Their message is that we are succumbing to the temptation to position our theories at the mid-range rather than the higher level ranges that will allow them to be more applicable to praxis, among other advantages.

The general line of argument from these thinkers in our field (and one that I subscribe to, overall) is as follows. Our theories to date have been mostly tepid. Some have pushed the envelope, but many/most are either axiomatic, pitched at the wrong level, or so esoteric that they are neither useful for practice nor ripe for further extension or development.

It is easy enough to take "pot-shots" at theoretical models that have been systematically undermined or even abandoned over the years so I will not dwell overmuch on these. It is clear, though, that in some cases, the opprobrium is perfectly justified. Without naming names, there are theory bases that are still widely believed to be strong theories, ones that have achieved the highest fame and fortune in the IS field, that have, in well-executed latter day debunking studies, turned out to be weak theories because of the huge common methods bias associated with how they were studied. Fortunately many scholars are shying away from these theories and exploring more fruitful avenues elsewhere.

What we need more of are theories that matter. I wholeheartedly embrace the principle expressed in Lucas *et al*. [2013] that our research should reach out and speak to policy-makers to such an extent that the field could even support a new publication on "IS Letters" that would be chock full of high-level policy implications. To do this, I feel we need to abandon the traditional dependent variables of adoption and diffusion. Start with the assumption that useful systems will eventually be adopted and "infused" into organizational life. Here is where the real magic occurs, as I see it. How do organizations change as a result of these technologies? Can they control these changes or are they pawns to whatever forces emerge as systems become reality? In my view, this is where the real scholarly action should take place.

Have we seen any examples of theorizing like this? In fact, I think we have. In 1990, George Huber published an article in *Management Science* that argued that advanced IT had major impacts on the way organizations designed (and redesigned) their structures. He also argues that IT improves the quality of decisions, which had been a popular thesis to that point in time. This is an interesting theory. It predicts that organizations will end up changing their structures as a result of IT. The evidence that such changes were already underway was present even when this theory came out. In the 1990s email was flattening organizational spans of control by allowing execs to manage more employees, electronically. Many other changes were in the offing as virtual organizations like Amazon.com were waiting in the wings of the Internet revolution. The abilities of firms to cooperate digitally and to form partnerships that were implemented over information systems was likewise predictable, as were their likely changes in organizational design and structure.

I hasten to add that what is new in this theory from my perspective is that it did not take on only the more straight-forward relationships that had been heavily studied in the literature to that point in time. Admittedly, it does include variables having to do with improved decision-making (a well-trodden path), but the element of design and structural change in organizations was fresh and startling.

The backstory on this article is that it is not clear to me that it has been extensively tested even though it has received a modest number of citations. It has been cited according to Google Scholar 56 times per year for a total of 1357 citations.[5] Another of Huber's articles published in the next year on organizational learning is much more heavily cited (7205 at the rate of 313 per year). Sadly, the field did not take up the challenge of testing Huber's innovative look at organizational structures and instead has focused on low hanging fruit across a wide spectrum of IS topics.[6]

3.1 Top-of-the-tree Research in HIT

Picking fruit from the top of the tree is difficult. Theoretically it means that one has to find exogenous constructs that are both levers for management and also amenable to measurement. It means that we need to stretch to create and develop endogenous constructs that deeply matter. Once again, examples might help to elucidate this idea. Instead of studying whether patients, doctors, nurses, technicians and administrators are adopting healthcare systems (be they within organizations or on the Web), isn't the crucial social issue whether such information systems are helping with the cure? Do support groups for diabetes work? Do they help patients diagnose their own problems and induce them to more fully engage in self-managed care?

With respect to healthcare professionals, the dependent variable of greatest interest, as I see it, is whether these systems lead to a markedly higher quality of care. Death rates should be systematically lower and cure rates systematically higher if we are to truly claim that systems make a difference, all things being equal, of course.

Where there have been numerous studies establishing baselines that show that investment in systems leads to more efficient and effective hospitals, isn't this something we have known about (with some caveats about how difficult the implementation process actually is) since the early days of computing? I am not saying that there is not another crop of low hanging fruit that can be picked in this vein, but I am trying to shift the debate to extend our thinking in ways that will have far greater social impact.

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5) Search was undertaken with the help of "Publish or Perish" software (www.harzing.com) on November 25, 2014.

6) See Mahmood, *et al.*, 2010 who specifically argue that infosecurity scholars need to pick less "low-hanging fruit" and more fruit from the top of the tree.

3.2 Top-of-the-tree research in Cybersecurity

Cybersecurity is another example of a domain that has to date relied heavily on low hanging fruit [see Mahmood, *et al.*, 2012 and their introduction the *MISQ* Special Issue on Cybersecurity]. Useful studies about how to improve the "defenders" of computer systems against hackers and the insider threat are, without question, of pragmatic value in many cases. But the real action is not in areas where it is fairly easy to gain access to data, i.e., the "good guys." This is low-hanging fruit. The high-hanging fruit is data about the "bad guys." How can we study them when they do not want to be studied? Mahmood *et al.* [2010] have a number of suggestions, but a simple demonstration of how this can be done can be seen in one of the papers published in the 2010 *MISQ* Special Issue. This paper was so superior that it also won the "Best Paper of the Year" Award at *MISQ* that year. Abbasi *et al.* [2010] used statistical learning theory to seek out fake websites with an astonishing degree of success. Given the gigantic detrimental impact that fraudulent websites are having on the economies of the world, this study offers major implications for how businesses and other organizations can firewall-filter out access to such sites.

3.3 Top-of-the-tree Research in Data Analytics

There is little doubt that massive data availability and analytical techniques to find meaning in this ziga-volume of data in real time is a critical topic for IS academics [Goes, 2014]. What is still unclear, even given interesting efforts to scope out the most fruitful areas of research [Chen *et al.*, 2012], is how we can raise the odds that our special expertise can be brought to bear on viable topics. As with the above areas, I would venture that high-hanging fruit is the best bet for accomplishing this laudable goal. If we apply the thinking of the past to this vision of the future, we will probably focus on whether analytics are efficient and effective for organizations. Or, in the management vein, the focus will be on how to structure this effort within organizations. Surely these are important topics. It would be hard to argue that they are not.

But the high-hanging fruit lies elsewhere. Examples again may help to show the point. Can we find surprising algorithms that differ in marked ways from past ways of indexing, filtering, processing, and selecting data records of true relevancy? Are there managerial processes that radically alter the equation for how organizations sense and feel the truly important relationships in massive data? Will business-as-usual be the "taps" sounding out at the death of organizations? What is called for in the way of innovation for big data to be instrumental in changing nearly all aspects of the way organizations meet their goals? In the case of profit-making institutions, in the way they sustain competitive advantage? Are their core competencies in big data handling and data analytics that will vault some firms forward against their competition? How does a firm develop such core competencies? Can governmental entities and non-profits also reap the benefits of big data in novel ways?

There are no end of good topics in any of the “hot” domains of the present. It is how and what we study in these domains that will break the mold. IS needs to be more of a mold-breaking field that we have been in our highly successful past.

Ⅳ. Conclusion

My prospectus on the IS field is that we have a bright future. I think it could be even brighter. We need to take on tougher tasks and develop theories that will bring fresh insights to the world. The trick is not to entirely give up studying the kinds of things we have been studying. Our work should be based on past work, whenever and wherever this is appropriate. But the secret to moving beyond the present is to relegate these well-studied constructs and relationships to a secondary consideration and to find new twists that make a difference. Research questions that reify the past/present should be RQ3 or RQ4, but not RQ1 or RQ2. The excitement should lie in the new research questions that head immediately for the high-hanging fruit. While still circling the tree at the bottom for picking up good fruit, we also need to be up in the sky seeking knowledge. This is risker as involving ladders and greater possibilities of loss and failure. But there, in the sky, the sweetest fruit will be sun-ripened to perfection.

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