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Responding to Three Issues in Hassan (2014)

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Abstract:

This article is intended to address three of the issues raised by Hassan (2014) in his essay: "Value of Information Systems (IS) Research: Is there a Crisis?". The three issues addressed here pertain to: (1) the desirability and nature of an IS core; (2) the relative social value in the study of IS whether narrowly or broadly defined; and (3) indicators of better research. Regarding the core, an argument for a narrow definition of the core is proposed with the view of providing a target for retention of central issues in IS rather than as a defining characteristic of what is necessary for inclusion in the IS portfolio. The meaning of social value will naturally and beneficially vary among the large number of IS scholars. It is of importance to recognize and stimulate, without mandating, efforts to bridge traditional IS concerns and broader social issues. The various criteria for evaluating value in IS, or any other research domain, are challenged with counter examples. Finally, suggestions for moving forward are presented.

Keywords: the IS field, philosophy of research, value of research, IS core, evaluation of research

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I. INTRODUCTION

Hassan [2014] is presented with the goal of stimulating discussion. I found the paper to be thought provoking throughout. I have an affinity for the “relevance” side in the rigor-relevance argument, but respect that observations that are not systematic, well documented, and expressed with clarity may not realize their potential value.

The issues raised in the paper have particular significance at this time. Those of us in the Association to Advance Collegiate Schools of Business (AACSB) accredited business schools will soon have to deal with required reports on the impact of research. I am skeptical about the value of this reporting. Although I believe the inspiration for the program is to (1) stimulate research more directly addressing existing problems of organizations and (2) link the results of research we are already doing with those who can apply these lessons, I fear that the exercise will become another burden of paperwork without much positive effect. It is worth noting that Hassan [2014] generally addresses “value” and AACSB addresses “impact”. While similar and overlapping terms, “value” refers to something intrinsic to the research and “impact” to the reaction it generates. In an ideal world these might be identical, but in practical terms (1) valuable research may not be recognized as such; (2) valid lessons may be misapplied; (3) impact from one source may be mis-categorized as belonging to another; and (4) we may not be able to measure or observe what is intangible and substantial (or such measures may be overwhelmed by what is relatively easy and countable).

In this article, however, I address three aspects of Hassan’s [2014] discussion. First, I revisit issues surrounding the elusive IS core; second, I discuss the overall value of IS research in the context of alternative targets for our attention and resources; and third, I challenge some of the philosophy based distinctions for determining value of research that Hassan [2014] presents. While these perhaps reflect tendencies – for example for active to be “better” than passive for research, there are too many counter examples for these to be used in a rigorously applied evaluation process particularly at the level of individual articles and studies. I conclude with some thoughts about what the IS field needs going forward relative to value creation and research impact.

II. WOULD HAVING A CORE UNDERSTANDING OF IS BE HELPFUL?

Hassan [2014] continues a discussion about the IS core. In essence Weber [2003] suggests that a core of theoretical findings is necessary for the IS field. In contrast, King and Lyytinen [2004] and Lyytinen and King [2006] argue that such a core is not a necessity. The debate as it is framed revolves around two central concepts: necessity and theory. Why focus on whether a core is “necessary” rather than whether it is “desirable”? Would we be better off if there were a mutually agreed upon core for the field? It is difficult to see how anyone could argue that we wouldn’t be better off. As Weber [2003] argues, such a core would consist of content that is not the application of reference discipline theory, but rather applied to uniquely information system phenomena, which contrasts with Grover’s [2012, p. 262] finding that: “IS is still reliant on reference discipline theories and deviation from formulaic approaches to adapting theory are met with suspicion.” Though elusive in practice, it seems that the simultaneous and mutual effects of information technology and human stakeholders cannot be explained by either the attributes of the technology alone or by human systems alone, e.g., by single reference discipline theorizing that focuses solely on technology or human systems, but not both. Weber [2012] views “theory” relatively narrowly, emphasizing the precision of measurement and definition of constructs, relationships and the like. Given such a view (reasonably held by many) why would a core be defined exclusively by theory – particularly if there is a relatively high level of supporting evidence needed for propositions to become fully-fledged theory? Of course the discovery, nurturing, and presentation of theory pertaining to uniquely IS phenomena would be welcome, but why dismiss empirical findings, heuristics, and complex narrative in an emerging field before they have had time to receive broad recognition and thorough testing?

In my view the benefits to having a core to the IS discipline are potentially significant and sufficient to warrant investment of time and energy to its creation. Such a core would (1) provide a base for communicating what our central mission is to stakeholders outside of IS; (2) serve as a touchstone for those in areas less central to that core, to describe the relationship and contribution of these studies as they relate to fundamental issues (as well as intrinsically in their own domain of interest); and (3) offer a prioritization for targeting the theorizing, knowledge acquisition, and accumulation of knowledge that should characterize a cumulative tradition for the field.

What would such a core consist of? I argue for a narrow core with the decision rule being: can anyone seriously argue that a particular topic is not part of the core? The central tendency pertains to the function of creating and managing information within the organization. This includes the building and maintenance of a computing infrastructure, the acquisition, integration, building, implementation of applications and suites of applications, the IS function to integrate these resources into operations and leadership of the firm, and the management of this function including planning for new technology, working with IS personnel, interacting with vendors, and individuals and groups in other organizational functional areas.

Reasons for a narrow core include: (1) it is hard to imagine modern firms without significant resources dedicated to information, computing infrastructures, and business processes that need management; (2) it is hard to imagine that we now know enough about these activities that there is nothing valuable yet to learn; and (3) it is likely that such topics, where they generate helpful results, can be useful to our practitioner stakeholders.

I, and I believe many others, continue to have interest in the issues raised by the information function in firms. I believe this represents the origin of IS as a discipline, and I believe significant problems in this area remain to be solved [Niederman and March 2012]. The key central tendency is the ability of agents to change (and respond to changes in) both the technology and their own behaviors, attitudes, and preferences toward creating beneficial results. Furthermore, the general knowledge of how to better organize and use knowledge applied within organizations influences commerce to be more efficient, effective, and innovative, leading in turn to economic progress and opportunities for more prosperity.

I find it difficult to anticipate anyone arguing that such content would not be part of the IS core. However, I expect many important topics are excluded by this narrow definition. One informal reviewer of an earlier version of this paper suggested consideration of an “outer” core exemplified by topics such as individual use of information technology (perhaps for hedonic purposes as in games or in the role of consumer as in ecommerce). I would add others including information economics, referring to the effects of corporate investment in IS; the embedding of information in organizational products such as tools for analyzing as well as purchasing securities online; and perhaps more technical approaches to enhancing particular hardware and software products as applied in practice.

Each of these topics can be addressed in ways that more closely relate to the IS core, e.g., how different formulations of games or ecommerce evolve as socio-technical systems, or others more distantly related such as how individuals attach themselves to computing brands such as Apple or Google, or what the effect of developing avatars is on social life among teenagers. Two implications of this conceptualization of such an “inner core” and “outer core” are: (1) the border between them can be porous – it isn’t particularly important for a given study whether it is inner or outer, rather that it contributes to fundamental understandings; and (2) the point of having an inner core is not to keep other topics from being considered IS or to exclude them from publication, but rather to focus attention as a community on those areas that should not be neglected. Following Weber [2003] I share the view that IS faces significant dangers if the content of our discipline can be entirely “explained” by imported theories.

In my judgment these “inner core” topics currently represent too small a percentage of what we are publishing, particularly in the most prestigious journals. Some of this is due to a mismatch between the complexity, time-dependence, and idiosyncratic nature of the phenomena of interest and the demands for theory-driven and precisely measured studies [King, 2011]. However, some can also be attributed to mistakenly representing the IS field solely as a small handful of journals. If we look at the whole basket of 8 publications identified by the AIS senior scholars, we get a richer distribution of topics and methods. This is yet more extensive if we consider the even broader array of journals including *Decision Support Systems*, *Journal of Database Management*, and the new Association for Computing Machinery (ACM) *Transactions on MIS (Management Information Systems)*, among many others. If we include Hawaii International Conference on System Sciences (HICSS) conferences, the quantity of research aimed at core IS issues rises significantly. In my mind it is unfortunate that many universities and not only the most prestigious ones do not count these publications at all or weight them modestly in their evaluation schemes. It is a myth and, in my mind, falsehood, that each article in the “top” journals is better than any in those ranked lower. Perhaps, on average, there is more prospect of value in top journal articles, but it is lazy and wasteful to dismiss the rest, particularly in this age of widespread access to online digital libraries like ABI/INFORM™ and Science Direct.

That said, it is good for all of us in the IS field that we have prestigious IS journals. We all benefit from the accomplishments of the most prestigious journals as they demonstrate the ability to produce rigorous work, show alignment with the highest quality work in business and related disciplines, and contribute substantially to our understanding of a wide range of issues and topics. However, we also need to respect and elevate relevant exploratory work that grapples with the complexities of the environment, even if what is produced is imprecise.

A shared, multi-faceted cumulative knowledge base strikes me as a potential unifier of the community. If we had been developing a cumulative tradition since Keen's [1980] famous admonition, what would it look like today? As I get older I tend to focus increasingly on history. To me much of cloud computing is a revision of the old service bureau – though of course capacities are orders of magnitude larger and transmission by Internet has replaced trucking stacks of cards. In a cumulative tradition, we would have theory in the sense of statements that identify constructs and relationships among them that can be found in practice that could be shifted from the service bureau (or perhaps outsourcing which followed and extended the service bureau concept) to the cloud. We would have: (1) even if imprecise, a set of propositions that can be quickly applied where the two approaches share attributes; (2) opportunities to test how the explosion of capacity affects processes and outcomes; and (3) occasion to update our general and specific understandings for the time when cloud computing itself has become obsolete and in turn replaced by another approach. This would apply not only to service bureaus, outsourcing, and the cloud, but decision support systems (DSS) moving to business and predictive analytics, and group support systems (GSS) evolving into virtual teams.. In this sense it is not only a distilled and minimalist set of proven theories that would be most helpful. Rather a rich set of theories, heuristics, and observations in all stages of evolution from initiation to the strongly supported, the broad to the contingent and especially those pertaining to “inner” core issues that can be applied to emerging concerns should constitute what we strive for as a core.

III. DOES A FOCUS ON THE CORE PRECLUDE THE “REALLY IMPORTANT” STUFF?

But is this view of a core something that community members would find inspiring? One commentator in Hassan [2014] suggests that he might have better gone into electrical engineering presumably to work on solar panels or other pressing social issues. More generally, this expresses the view that perhaps the issues IS addresses are not sufficiently engaged in correcting social ills for this individual's sense of personal responsibility. I expect that this individual is not alone in this sentiment.

One approach that some in the field are taking is to study such issues of social concern using an IS lens. Numerous individuals have begun studying “green IS” [Watson et al., 2010; Watson et al., 2012] to investigate issues ranging from reducing power consumptions of data centers to algorithms that replace resources with information in the supply chain. Surely similar groups pertaining to other essential resources and issues may fit within this special interest group.

However, some may argue that the IS field and community have concentrated too much on narrow issues while computing has been perhaps the most revolutionary social force of the past half century. Walsham [2012] calls for a reorientation of the IS field in the direction of addressing societal problems. King [2011] points dramatically to seismic shifts in society resulting from the creation and distribution of computing. These phenomena are global, forward looking, cross disciplinary, contingent, idiosyncratic, and transient. As King points out, these are difficult phenomena to capture by using precision oriented research methods; see also Davison and Martinsons [2011] for a discussion of methodological exclusivity. But King also points out that deriving genuine understanding requires disciplined and organized investigation. Does establishing a focus on social benefits necessitate a shift in the way we assess quality of research and balance precision with intentions?

Another source of possible concern pertains to what often seems like a strong lack of concern among practitioners for what we do. I think there are a number of reasons this might be the case. Perhaps those in industry consider many of the core issues solved – at least solved enough that other issues appear more salient. I frequently talk to IS senior leaders through groups like the Society for Information Management (SIM) and ask what it is they don't know that they would like to know about IS and its role in organizations. I invariably get four answers: metrics, personnel (mostly how to be sure there will be a future supply at a desired price), security, and the technology of the day. In recent times the technology of the day has been mobile and business analytics, but not long ago it was cloud computing, with enterprise computing and ecommerce before that. I can't remember when I've seen a paper on applying metrics within the firm in a leading IS journal. Perhaps this is a topic best left for the consultants who can help each firm resolve its own key performance indicators and tailor custom solutions [Bernard and Gallupe, 2013]. We do have a stream of IS personnel issues in top journals that deal with matters like career paths [Joseph et al., 2012] and the effectiveness of Human Resource (HR) programs for IS personnel [Ferratt, Prasad, Enns, 2012]. Security policies and their effects are of great interest, but much of this work and concern is on the technical side – how to build a better firewall or create stronger encryption – and perhaps that is best left to the computer science folks. Of course we have our share of papers on one aspect or another of the technology of the day. It is interesting how quickly a topic like business process reengineering moves from critical and urgent to almost an after-thought.

While we typically seek general solutions to business problems, practitioners are often more concerned only with the solution in their particular case. In my experience, practitioners are rarely concerned with precision and proof but rather appreciate frameworks or instructions that increase marginally the probability of success in a particular instance or alternatively simply stimulate innovative thinking. This does not mean that scholars should entirely shift

focus from universal principles and verifiable findings. But it does highlight the existence of a gap between practice and academia. We cannot force practitioners to take an interest in issues outside their immediate concern, but we can (1) conceive a pathway by which our investigations do address concerns of practice in some way (we see this with the “implications for practice” sections which exist in many publications, but, to my reading, are often pro forma and generic) and (2) we can address some publication effort toward bridging this gap. A number of journals including *Management Information Systems Quarterly (MISQ) Executive* aim to do this, but perhaps expanding this effort should be a priority for the community. Textbooks generally translate the latest thinking on topics of educational interest and present them to future practitioners. Perhaps academics in their roles as consultants also perform this task to some degree.

IV. HOW DO WE KNOW VALUE WHEN WE SEE IT?

Hassan [2014] presents a number of perspectives on the nature of value in research. I am in agreement that the terms “basic” and “applied” research are problematic. As broad indicators of directions on a continuum they may have value, but there are far too many examples of research with some basic and some applied characteristics to make assigning particular studies to one or another category a straightforward task. I prefer the idea that we have grand, mid-range, and detail level theory where the grand theory states broad principles, the detail level theory applied fairly narrowly to observable relationships, and the mid-range represents a fluid intermediate ground. For example, DeLone and McLean’s model [DeLone and McLean, 2003; Petter, DeLone, and McLean, 2013] broadly links information quality to application success; a mid-range theory might propose that information quality is more important to success than user characteristics in business analytics; a detail level theory might state that for predictive analytics more than a 2% error rate in underlying data (hypothetically) will prohibit successful application. In general the “grand” theory will appear more basic and the detail level more applied, but with a feedback loop the “applied” should influence the formulation of the “grand” as it is updated to account for the actual observations, and the “grand” should open realms of detail testing and application.

Similarly, the stated sources of value in research seem reasonable on the surface and might generate pairs of examples illustrating their point, but it is hard to envision where they’d be useful in evaluating particular research. Consider ends and means. I want a productive workforce. How do I do it? I increase morale. How do I do it? I create low intensity competitions among workers. Each of these is both an ends and a means. I want a more productive workforce as an end measure, but also as a means to earn more money and perform a higher level of service. If I increase morale without increasing productivity, I still have a partly positive result. In terms of research, I can envision these various activities as elements in a complex system where each component receives from and contributes to the others with complex cycles of mutual influence. I don’t see how this concept would generate better research (or allow me to recognize it) rather than lead to arguments about in which category a particular study belongs.

The concept of novelty is also troubling. Does adding a variable to a nomological net constitute novelty, whether or not it materially affects the overall quality of the model? Does gathering data showing that a previously recognized construct in a nomological net doesn’t have influence in a different context constitute novelty? Surely we respect the revolutionary groundbreaking idea, but it is at peril that we repudiate the workaday incremental process of testing ideas and gaining nuanced knowledge. Vessey and Ward [2013] proposed a theory regarding strategic alignment. In a methodical fashion they examine what is known about such strategic alignment and propose the application of co-evolutionary theory with the result of a set of propositions. In my mind this is an exemplary, stimulating, and innovative approach, but as a devil’s advocate I could argue that the paper largely recombines existing elements. I wouldn’t know how to resolve whether or not this is or is not “novel”. To put it another way, while I can see the positive in the truly novel, I can also see an equal amount of positive in what is not “novel”, thus it is hard to see how this can be useful as a way to distinguish the value of research.

Similarly, distinction between active and passive research sounds promising but difficult to implement. Is sending a survey to knowledgeable people less active than watching computer users in an observational study? Is it active to simply report the findings of hypotheses tests? I recently reviewed a paper that used grounded theory method where the overall coding was mildly informative but the discovery of some “outliers” and unusual responses was provocative. Is this active or passive? It is actively being alert for potential theory generation, but passive in the sense of accepting what is seen rather than having addressed it directly and purposefully.

The criteria of “making evident what is not” is also puzzling because something evident in engineering (like prototyping) when applied to programming may be ground breaking. The prototyping per se is already evident to one audience but is not evident to another. The “evident-ness” is not an attribute of the research topic but of the topic, its context, and the timing of shifting knowledge from one domain to another. I don’t see how this concept would help in evaluating a particular research project or even a research stream.

It is easy to find counterexamples and to criticize, but the question remains what *does* signal the creation of value? I think much of our difficulty with this question revolves around the unit of analysis. Is the individual study, the organizational unit, e.g., the university or department, or the field as a whole the unit of analysis? There may be obvious value in particular studies or the accumulation of a set of studies, each of which is somewhat marginal, may have emergent value, e.g., three supportive and three unsupportive studies may together suggest a differentiated population. A particular department or university may produce a set of papers that individually don't say much but when combined with work from others around the world may represent important components in larger work. Is there any way to judge the contribution of a particular work when it is NOT combined with the others? Poor methodology might doom it, but does good methodology guarantee positive value? Different studies make different contributions and it is not clear in my mind what, if any, process could strip away their nuance to make them measurable and standardized for simple processing. It reminds me of tomatoes engineered to be easy to pick and difficult to bruise for shipping, but that have no juice and no flavor.

V. FINAL COMMENTS

The question for the field as a whole, however, must be: at the end of the day are we in fact providing useful (or potentially useful) knowledge regarding the information function in organizations and the various topics of the "outer core" of IS issues? I think the answer is likely to always be "yes, but we could do more." To the optimist our current state needs "a little more", to the contrarian it requires "a lot more". We are a large enough group with diverse enough values that even if we could define a definitive percentage of achievement it will never be enough for some and more than enough for others.

The quest for ways to continually improve the amount of value we create and disseminate should include: (1) a focus on the direct and indirect applicability of the questions we ask in our studies and research streams -- that there should be value in knowing the answer, (2) our ability to assemble knowledge and value across individual studies and topics, (3) our ability to apply what we learn to evolving circumstances, (4) a balance between general principles (grand theory) and applications (detail theory), and (5) mechanisms for effectively bridging communication gaps between researchers and stakeholders.

Finally, I want to acknowledge that this article, like Hassan [2014] is intended to stimulate thought. If, by some miracle, it is the catalyst to a suddenly emergent consensus on these issues, so much the better. Different premises, I assume, will lead to different conclusions. I am aware that these observations are based on my personal experience as an American having studied and taught for 25 years in the US. It is clear to me that if I were located in Europe or Asia it is almost certain that I would see the world of IS much differently.

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Fred Niederman serves as the Shaughnessy Endowed Professor of MIS at Saint Louis University. He is a proponent of grounded theory and theory building as a way to enrich the MIS discipline and build intellectual content customized specifically to our field of practice. He has published more than one hundred articles in leading research journals and refereed conference proceedings. He serves on editorial boards for *TMIS*, *Journal of Association of Information Systems*, *Communications of Association of Information Systems*, *Human Resource Management*, *Journal of International Management*, *IEEE Transactions on Engineering Management* and the *Journal of Global Information Management*. He has edited or co-edited special issues for *CACM*, *DATABASE*, *Journal of Global Information Management*, *Journal of Organizational Computing and E-Commerce* and *Human Resource Management*. He recently served as co-program chair for the 2010 ICIS conference in St. Louis, Missouri, is an active member in the MIS "senior scholars and is proud to be counted as a member of the "circle of compadres" for the PhD Project.

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