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Value of IS Research-A Response to the Rejoinders

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Abstract:

This article responds to six rejoinders to Hassan [2014] and addresses their concerns surrounding the anxiety discourse in Information Systems (IS), the need to meet or exceed stakeholder expectations, the notion of originality and what it means to IS researchers, the nature of the "core" of IS research, and different strategies in establishing an academic discipline. This response argues for a focus on internal efforts to put our house in order ahead of reputational maneuverings, and proposes a closer examination of our intellectual structures.

Keywords: Value of research; relevance; impact of research; research benefits; IS knowledge

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I. INTRODUCTION

My sincere gratitude to the Editors of the *Communications of the Association for Information Systems (AIS)* for allowing me to be involved in this discussion about the value of IS research. It has been a truly satisfying experience to read six very thoughtful responses to "Value of IS research: Is there a crisis?" [Hassan, 2014]. The select group who responded spent quality time dissecting the article and produced very insightful views that I thoroughly enjoyed reading. I believe the level of discussion surrounding this very important topic is sufficiently raised and that can only result in positive outcomes. Whenever this issue is raised at conferences and in personal conversations, I am always amazed at how many people agree with the article's assessment of the IS field, but are either left in a state of not exactly knowing what to do, or will find justifications for essentially not doing anything at all. All of the issues raised by the article were expertly discussed by the six responses, and I would like to respond to them in a thematic fashion based on a reading of each of their responses.

II. ENHANCE VALUE AND SUCCESS

Frank's [2014] assessment of the article as a statement about success is correct. We should be concerned not only about the success of the field as a whole, but more importantly the success of every single researcher, who should be expending precious time and resources in a progressive environment instead of continuously struggling to justify their work just because the field's intellectual structures are not there to support them. Frank [2014] offers yet more symptoms from the anxiety discourse that are heard whispered at conferences, but are not raised in public forums for fear of reprisal. The causes he suggests for those symptoms are also compelling and they certainly work in concert to create the circumstances in which we find ourselves. His feeling of helplessness is shared by many and based on the latest mission of the AIS leadership ("On the Road to Relevance" [Fedorowicz, 2013]), these concerns have already triggered some positive initiatives. It is as if everyone accepts the state in which we are, but is waiting for someone else to do something about it.

This general agreement on our state of affairs is reflected in Avital's [2014] seemingly negative response. A closer reading reveals that much of what Avital [2014] observes and proposes are in line with what Hassan [2014] observes and proposes. Avital's [2014] in-depth analysis of the need to: (1) clarify the subject matter of IS, (2) frame the question of value in the context of IS research, (3) foster stakeholder appreciation, all support Hassan's [2014] conclusions for needing to ask the right questions. As Avital [2014] succinctly puts it, we need to define "the domain of inquiry of IS scholars" and we need to "differentiate ourselves and be explicit about our expertise and unique contribution." These same concerns are also reflected in earlier writings [Hassan, 2011, Hassan and Will, 2006] which also referenced as Avital [2014] had done, Toulmin's [1972] concept of the discipline's "genealogy of problems." For the sake of space, I will not repeat the intimate details of many of these commonalities. I do like to address Avital's [2014] commentary of some of my more practical proposals for enhancing the value of IS research. These proposals may, as Avital [2014] suggests, appear to be naïve or even myopic, but they are very much on the same page as the framework that he proposes.

II. WHO ARE OUR PRIMARY STAKEHOLDERS?

Avital [2014] sees value as more than just an improved IS research product. This view of the value of IS research is consistent with Gordon Davis's framework that is presented in Hassan [2014], and with research being a social construction. In other words, I have no issues with accepting our stakeholders' perception to evaluate the significance of IS research. The question is, who do we consider to be our primary stakeholders? When Markus [1999] explained how our stakeholder customers have changed, she did not even consider students and journal editors as customers of IS research; her concern was that the traditional IS department was no longer the IS field's primary customer. Are we happy with the editors of our own journals (who probably happens to be our close friends) as the primary stakeholders of IS, or do we want to see editors of journals from other fields (besides management, our long-time bedfellows) acknowledge our scholarship? Are we satisfied with our PhD students consuming and replicating our research, or do we wish to see researchers from other disciplines extend and appropriate our research; or better yet, see practitioners present practice-related versions of our research at their conferences and board meetings? Internal stakeholders are critical to the development of the field, but there is an important difference between internal stakeholders and external stakeholders. Not only do external stakeholders provide a more "objective" measure of the value of IS research, they have a greater impact on the survival of the field and should therefore be the focus of our attention.

III. IS ORIGINAL RESEARCH EVEN POSSIBLE IN IS?

In the same way that we need to analyze who truly are our stakeholders, we also need to take a closer look at what Avital [2014] contends are truisms that are too generic to offer any practical guidance. For example, he contends that our researchers are already doing their best at being original, meaningful and insightful. Since Hassan [2014] used different terms from "meaningful" or "insightful" to describe the qualities of significant research, here I will only elaborate on originality. The reason for raising the issue of originality is because I seriously doubt we agree on what "original" research means in IS. For example, as Niederman [2014] asks, does adding one or two additional variables to an existing research framework mean the research is original? Niederman [2014] does not think so. But why not? A lot of research in IS is made up of this kind of theorizing, or they combine two or three frameworks and test the combined framework for statistical power. This kind of research has its role in what Niederman [2014] refers to as "the incremental process of testing ideas." However, they cannot be said to be original or novel, not because they use existing elements but because they lack the other characteristics of original research—being active and making evident what is not.

As alluded to in Hassan [2014], using elements of previous research does not imply that the research is not original. Most people agree that Darwin's findings were original even though he incorporated elements of Lamarckian theories into his own. What makes Darwin's findings original is the way that his research changed the rules proposed by Lamarck [1809/1960], what Foucault [1972] calls the "discursive formation". Even though Darwin's theories were original, they failed to describe what was empirically observed, which led to Darwin's notion of pangenesis being pushed aside to make way for De Vries' [1889/1910] version of pangenesis, from which comes the term "genes" and the notion of mutation that we use today. This is what "active" research means in Foucauldian terms, not merely adding or subtracting a variable or two from an existing framework, and not a researcher "actively" participating in more interventionist research methods. The same case can also be characterized as "making evident what is not" because Darwin's contribution revealed hidden laws and previously imperceptible processes. The same can be said about Mendel's research into genetics. Sociologists of knowledge suggest the reason why Mendel's discovery in 1866 remained relatively unknown until the early 1900s was because the structures of knowledge at the time did not allow for his discovery to have much value [Brannigan, 1979]. In the light of the new structures of knowledge in the early 1900s, Mendel's work became more than just a minor contribution to Darwin's general theory of evolution, it became revolutionary (or much more valuable).

We need not follow the model of originality in the natural sciences, but we should at least agree on what is considered original in IS, especially in relation to other disciplines. Originality is, of course, very closely related to differentiation, which Avital [2014] considers to be "probably one of the most urgent tasks that the IS Senior Scholars forum ought to tackle right now." If Avital [2014] concedes that we have yet to differentiate our research and show the uniqueness of our contribution, how can we say that we are already doing our best with original research?

IV. STILL LOOKING FOR THE CORE OF IS

The use of the term "value" in Hassan [2014] as opposed to "relevance" or "impact" is central to this whole debate. "Value" provides a less restrictive approach to evaluating significant research and provides a rallying point for people that may otherwise have contradictory opinions about how IS impacts its stakeholders or are relevant to them. Mendel's struggles in getting his research appreciated illustrates the distinction between value and impact and the importance of having a "core" when it comes to research. Although his findings did not create an impact on the community around him, his research was valuable, even in the situation in which there were no theories to be had in the mid 1850s for Mendel upon which to hang his hypotheses. This case study illustrates not the importance of theories, but the importance of the "genealogy of problems" withwhich his work was closely aligned. This is the concept of "core" of any discipline that I believe eludes the IS community. This core is what Toulmin [1972] calls the field's "intellectual ideals." Given this definition, is this core necessary for any field of study? As Mendel's case study shows, it is not a "theoretical core" but a core nevertheless which rallies researchers of the same feather to do work of the same kind. Theories are just one of the many outcomes from a successful intellectual enterprise based on such a "core."

Herein lies the beauty of disciplinary structures. Academic fields that can clearly communicate their intellectual ideals are able to reconcile monism and pluralism, and integrate unity with diversity. There is no need to limit our field to a few core concepts, a concern raised by Chiasson [2014], because we are free to invent as many concepts as we see fit as long as it is consistent with the field's discursive formation. There is no need to choose between one concept or another because the field's intellectual ideals distinguish for members of the field what is inside the boundaries of the field and what is external to it, and what is mainstream versus what is at the fringes. I agree with Niederman [2014] that a core is important. As mentioned above, the issue is with the nature of the core. Having a core that is bounded by space and time, as is the case with the IS function in organizations, may place unnecessary limitations on an academic discipline. As Markus [1999] argued more than a decade ago, the traditional IS function

was already changing even then. It is true that IS originated from this environment. But like other established disciplines, it needs to extract itself from the environment that gave birth to it before it can mature. Glimpses of such new objects of study can be seen already. For example, social media users have no IS function or formal organization to speak of, but research in that area is no longer considered a peripheral area of study as companies are being dragged kicking and screaming into implementing social media. By limiting ourselves to the "inner core" of organizational studies, aren't we ignoring the perspectives of individual users or societal concerns stemming from these technologies? Instead, as proposed in Hassan [2014], a better choice is the discursive core that won't be bounded by any physical or temporal limitations.

V. INTERNAL VERSUS EXTERNAL LOGICS IN THE ESTABLISHMENT OF DISCIPLINES

Another possibility is that we are already doing original research, but not successfully communicating it to our stakeholders. Or as Avital [2014] suggests, "is not so much what the IS research product is, but how it is perceived by its stakeholders." Taking this to an extreme, is the content of IS research really divorced from how it is perceived and received by our stakeholders? We need to re-examine this notion if we want to IS research make any headway among its peer disciplines. Chiasson [2014] will have more to say about this, and his commentary on the knowledge-power nexus reflects to some degree such an ingrained notion. In strategic management, Hambrick and Chen [2008] elaborated on the same issue by dividing the causes of the ascendency of disciplines into what I categorize as two basic logics, the internal logics and the external logics. Drawing from Merton's [1973] explanation of how sociology became an established discipline, Hambrick and Chen [2008] described the internal logics as the process of building a discipline capable of addressing significant phenomena that other disciplines are incapable of addressing (differentiation), and coalescing new members of the field until a critical mass is reached (mobilization). The external logics include the process of justifying to the powers that be (e.g. university administrators) of the value of the discipline in order to procure departmental and journal space (legitimization) and procuring the acceptance of other disciplines after the successful rise of the incipient field (reconsolidation). Differentiation is the same call to action that Avital [2014] considers to be "one the most urgent tasks" of IS research. Differentiation is usually followed by mobilization and then legitimization, which in turn enhances differentiation and mobilization. Therefore, differentiation should be among the first efforts of any emerging field because, as the history of IS has shown, it is rather difficult to mobilize and legitimize research that is undifferentiated and can be comfortably placed in any other existing fields. Either way, Hambrick and Chen's [2008] argument suggests that the product of IS research *canno*t be divorced from how it is perceived. I will elaborate more on this in my response to Chiasson [2014].

VI. WHAT IS AN APPLIED DISCIPLINE?

The way IS researchers perceive their own work is actually the crux of my arguments for how they value it. For example, when I suggested that the issue of basic versus applied research is relevant to IS, I wasn't proposing that we should actively differentiate between basic IS and applied IS research. I was merely stating the status quo as perceived by most IS researchers that our field is an applied discipline or should be one (e.g. "It is appropriate, therefore, to position IS as an *applied discipline"* [Robey, 2003, p. 356] or "*applied discipline such as IS"* [Taylor et al., 2010, p. 647]). Avital [2014] and Niederman [2014] are correct to say that this division is unproductive for the same reasons stated in Hassan [2014]. However, I suspect that being "applied" means different things to different people. Some consider being "applied" means being relevant to practitioners. Others follow the more traditional definition of "applied" as in the sense of applied science to mean "the discipline dealing with the art or science of applying scientific knowledge to practical problems" [TheFreeDictionary, 2013]. Both perceptions of IS research are instrumental in framing how IS is valued in relation to its reference disciplines and other fields of study, and is therefore very relevant to IS. For, if IS is viewed as an applied science of say psychology, it will always be evaluated in relation to psychology and how it successfully applies psychological theories, not necessarily based on its own merits.

Hassan's [2014] analysis of Dewey's ends-means is intentionally separated from the perception of IS as an applied field to distinguish how the IS community treats its own field from both perspectives. The laymen's version of ends-means is understandably similar to how applied science is viewed. The version of ends-means proposed by Dewey is not the same as applied science in the sense normally understood. It does include pure or basic (or Avital's "exploratory") research. As Nelson [1959] clearly emphasized, most discoveries don't come from applied research, they come from basic research. All of these assumptions and practices that shape our field's intellectual structures: (1) the way we view "original" research, (2) the apparent unwillingness to differentiate our research and to continue to peg it to organization science or management, and (3) the narrow view of IS as an applied field, will displace any collective efforts to create a stronger brand or brighter halo effect, unless they themselves are addressed. How can we expect to establish partnerships if we have difficulty describing what unique contributions we bring to the plate? How do we expect to build cross-disciplinary spans of scholarly networks if we have difficulty differentiating our research? And how do we address grand challenges when we rely on reference disciplines for concepts and theories?

VII. THE IS POWER/KNOWLEDGE NEXUS

Going back to how the nature of IS research is divorced from how it is perceived, Introna [2003] suggests that the problems surrounding the status of the IS field is not an ontological or epistemological one, it is a political issue. In order for a discipline to be possible, it has to be located within a political framework of departments, schools, groups of people, which determine the way the phenomena are conceptualized and researched. As Introna [2003, p. 236] summarizes, drawing from Foucault's theories on the intimate relationship between knowledge and power:

> "Thus, being an academic discipline is first and foremost a political achievement, not an ontological or epistemological one. They are not a 'discipline' because they got it right – found the distinctive 'core' descriptions, theories, and methodologies – but because they have convinced enough people, and aligned themselves with enough people – that matter."

According to Introna [2003], claims of disciplinarity are not dependent on "any distinctive core … cumulative tradition" but are "political questions from the start" and "will only be effective if it is framed as appropriate political programme" (p. 239). Following Introna [2003], Chiasson [2014] calls for a closer examination of the micro-politics of the IS field and strategies (such as encouraging alternative genres and engaged scholarship) that will enable the field to opportunistically derive the right outcomes from the stakeholders of our research. I agree with both Introna [2003] and Chiasson [2014] on their socio-political analysis. The "political status of science" as elaborated by Foucault [1980, p. 109] is most relevant to this debate as is the source of power and authority from the "regime of truth" (p. 131) that permeates any society. I also agree with Introna [2003] that in many ways, as a field, we have done very well in this "regime of truth" for ourselves. Careers are built and sustained on this regime. What I am concerned about is the sustainability of the regime itself as it is challenged by the greater regime external to the discipline that determines what is and what is not valuable for funding and support. We may be able to make truth claims and get them supported within *our* truth regime, but ultimately someone else outside that truth regime will judge our work.

Also, a major component which has its background in Introna's [2003] Foucauldian analysis may hold back any efforts to "challenge the micro-politics of IS research practice" [Chiasson, 2014]. It has to do with the intimate relationship between the social and the technical, the political and what Introna [2003] calls the epistemological. Ever since Mulkay's [1969, 1973] elaboration and Latour's [1979] anthropological study of social influences in the construction of scientific facts, it is a given fact that politics play an important role in the social construction of science. Applying this revelation to IS, we concede that there are powerful forces at work preventing any form of innovation within the field. At the same time, Mulkay's and Latour's body of work also acknowledge that science is not, as Introna [2003] puts it, "political questions from the start." Instead, cognitive, social and technical factors intertwine to produce the discourse that is generative of power and authority. As if mirroring these conclusions in which two (cognitive and technical) out of the three factors are essentially "epistemological," Foucault dedicates two-thirds of his *Archeology of Knowledge* [Foucault, 1972] and an even larger proportion within the *Order of Things* [Foucault, 1970] to technical and intellectual details, not purely social or political arguments. The power of the regime of truth doesn't only come from the authority of the speaker, or the institution and how they outmaneuver each other to dominate; it begins first with a recognition of an object of study that is captured in formation of concepts that are ordered within statements, and the ordering of these statements to form strategies and theories. Granted, the regime of truth is supported by various social, political and other elements, but it requires a foundation, and it is the coherency demonstrated by the statements produced by the field which constitutes this foundation. Consistent with Foucault, the social cannot be separated from the technical.

VIII. THE IMPOVERISHMENT OF IS RESEARCH

If the issues mentioned above are not addressed, we will be adding to the factors highlighted by Johnston and Riemer [2014] which they say may lead to the impoverishment of IS research. As they note, the danger of impoverishment is already very likely if IS research continues to be undertaken in a scripted manner. The activity of publishing just for the sake of publishing engenders a culture where quantity trumps quality causing the significance of the research to be based on how well the script is followed, not on the quality of research itself. As a field, we need to be more sensitive to the quality of our research because not only are we far lesser in number than other fields of study (e.g. management conferences attract 3-5 times more people), our resources and funding are miniscule compared to that of the natural and life sciences. We need to examine more closely how each research area contributes to the overall body of knowledge of IS. Using Kuhn's analysis, much of the research that contributes to the growth of a science takes place in the practice of normal science. Do we have a normal science to speak of? The phase of normal science is reached when the research community no longer needs to continually build the field anew and the research community can focus on the more esoteric aspects instead of wasting time on repeatedly building the research from first principles (this is also the reason why the introductions to and justifications for our research take up so much more space in our articles when compared to say computer science). Instead, normal

science allows scientists to investigate part of the field in enough “detail and depth, which would otherwise be unimaginable” [Kuhn, 1970, p. 24] and consequently be seen as significant by society. It is during the periods of stability offered by normal science that the professional aspects of the field will have the most opportunities to grow and influence society. Of course, sometimes progress can take a "quantum leap" when it undergoes a paradigm shift, which does not necessarily negate the previous efforts, but builds upon it and discovers new avenues that could not have been researched given the old paradigm. I wonder if the history of the IS field demonstrates either phase.

IX. CONCLUSION: OPTIMISM FOR REAL CHANGE

The optimism Grover [2014] shows is infectious, and I would be the first to celebrate such optimism. However, four out of the five citations Grover presents as evidence for his optimism are self-citations, which gives me reason for pause. I'm all for being positive, but I wonder if the optimism may be slightly misplaced. Many studies can be cited that support a flowery picture of our accomplishments, but at the same time many other studies will disagree. After all, it was for the most part, Grover's remarks that triggered this debate in the first place. And it were these remarks that received such enthusiastic responses from the IS community. If I were to ask myself which of Grover's opinions resonated more with the IS community, I would say it was his remarks during the Special Interest Group (SIG) Philosophy workshop more than his many other positive articles about the state of the IS field. I sincerely hope for our sakes that Grover's [2014] optimism will prevail and his prognostications that the field can indeed do better will come true. I may be misreading his response, but (1) moving away from "there is no innovation in the sequence, the received theory is rarely challenged and the big questions are not addressed," (2) working towards "creating solid foundations for our field through a battery of robust constructs," (3) not "forcing everyone to be master of all trades," (4) implementing "changes in institutional structures," (5) "challenging reference disciplines," not something we do everyday as a field, (6) accepting "papers that fall outside the script" that will ultimately move us at "par with other disciplines," all of which are not, at least in my experience, trivial pursuits or even "doing better." In my opinion they require "a small revolution."

Sometimes a crisis develops not because it is staring in one's face, or in plain sight, but because it is imperceptible. Often the dangers from this kind of crisis are even more pernicious than the obvious kind because of its obscurity. Take Avital's [2014] example of the subtle but very important difference between the products of the "IS discipline" (however one defines it) and the products of IS scholars associated with the AIS. To this day, we will find researchers in the IS community gladly claiming that luminaries such as Herbert Simon, Vint Cerf, and Claude Shannon as scholars of IS. No doubt their contributions were seminal to the IS field, but it would be quite pointless to bask in their glory because that does little to enhance the field's substantive contributions. At the very least, we become contented by being in their company. The same goes with our over-reliance on management and the organization sciences. They are having the same problems we are having and not coming any closer to a solution [Aguinis et al., 2012, Alvesson and Sandberg, 2013, Fincham and Clark, 2009]. As a field, we cannot continue ignoring the symptoms. Often, it is necessary to frame the issue as a crisis, because as our colleagues in organizational development suggest, one of the best ways to exact real change is to show the pain.

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Editor’s Note: The following reference list contains hyperlinks to World Wide Web pages. Readers who have the ability to access the Web directly from their word processor or are reading the paper on the Web, can gain direct access to these linked references. Readers are warned, however, that:

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