Communications of the Association for Information Systems

Volume 34 Article 41

2-2014

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Recommended Citation
Hassan, Nik R. (2014) "Value of IS Research: Is there a Crisis?," *Communications of the Association for Information Systems*: Vol. 34 , Article 41.
DOI: 10.17705/1CAIS.03441
Available at: https://aisel.aisnet.org/cais/vol34/iss1/41

Communications of the Association for Information Systems

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Value of IS Research: Is there a Crisis?

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Abstract:

This debate section article examines the notion of value or worth of IS research. It suggests that how others value the product of Information Systems (IS) research may be the underlying problem triggering various forms of anxiety discourses that are frequently recurring. The value of IS research is examined from the perspectives of ends-means, basic and applied research, and the significance of IS problems. The article also proposes ways of maintaining the value of IS research by emphasizing originality, being "active" and not "passive", and making evident what is not. The article argues that the value of IS research is found not in duplicating research already undertaken in management, computer science, psychology, economics or its other reference disciplines, but in asking questions that other disciplines are not asking or in addressing problems that others are incapable of addressing.

Keywords: information systems (IS) research, IS theory, intellectual ideals, anxiety discourse, disciplinarity

Editor's Note: The article was handled by the Department Editor for Debates.

Volume 34, Article 41, pp. 801-816, February 2014

I. INTRODUCTION

The title of this article is intended to stimulate discussion on the underlying problem triggering the frequently recurring anxiety discourses [Lyytinen and King, 2004]. Their frequency and intensity suggest that the Information Systems (IS) community has not yet identified the underlying problem, the community is struggling with a communication deficit, or is just simply confused. For an academic field, any one of these conclusions is a cause for concern and deserves close attention. This article is not intended to be a criticism. It is more of a call for help because a sentiment of an "ever-present overlay of confusion and frustration" is felt by many, especially doctoral students who stand most to lose from the exhausting and time consuming process of grappling with these issues [Darroch and Toleman, 2006, p. 1]. This article explores the underlying problem and suggests one dimension of our intellectual enterprise that has not been closely examined—the question of value and worth of IS research. In other words, is the value and worth of IS research the crux of the underlying problem that is manifesting itself repeatedly in different forms?

Before examining what is meant by "value" of IS research, it may be useful at this point to offer some evidence of the recurring issues. Either in succession or simultaneously, the anxiety discourses have taken various forms beginning with the concern for cumulative tradition [Culnan, 1987, Culnan and Swanson, 1986, Hamilton and Ives, 1982, Keen, 1980]. Although earlier studies suggested that the field was showing cumulative tradition [Cheon et al., 1992, Cheon et al., 1993], more recent studies suggest only modest gains [Grover, 2012, Larsen and Levine, 2005]. The issue of cumulative tradition was closely followed by a discussion on the relationship of IS with its reference disciplines and its reliance on them. Most studies agree that the IS field is emerging as an independent field [Cheon et al., 1992, Culnan and Swanson, 1986, Mingers and Stowell, 1997]. The question is whether or not it has matured enough to have an impact on other fields or the general body of knowledge. Baskerville and Myers [2002] certainly think so and concluded that the IS field was ready to become a reference discipline in its own right. Using citation data, Katerattanakul et al [2006] and Grover et al [2006b] support this claim while Nerur et al [2006] and Wade et al [2006b] reached opposite conclusions. This latest disagreement concerning IS's status as a reference discipline resulted in a heated debate between the authors [Grover et al., 2006a, Straub, 2006, Wade et al., 2006a].

The discussions on cumulative tradition and reference disciplines was followed by the concern about the gap between IS theory and IS practice. These concerns took the shape of debates between 1991 and 2001 surrounding the lack of relevance of IS research [Applegate, 1999, Benbasat and Zmud, 1999, Davenport and Markus, 1999, Gray, 2001, Keen, 1991, Kock et al., 2002, Lee, 1999, Lyytinen, 1999, Moody, 2000]. In one of several panels on the issue of relevance [Kock et al., 2002], former president of the AIS, Michael Myers, warned about the futility of hoping for any change in the relevance of IS research:

> In other words, while relevance to practice is frequently proclaimed as a virtue in public, in reality practical relevance does not matter. What we say we believe we do is quite different from what we actually do. Our existing values and practices, embedded as they are in longstanding institutional practices, ensure that the frequent calls for IS research to become more relevant to practitioners are doomed to fail. (p. 339).

In the years following, concerns about relevance continued to be raised in numerous studies [Desouza et al., 2006, Grover et al., 2008, Rosemann and Vessey, 2008, Srivastava and Teo, 2005, Straub and Ang, 2011] suggesting that relevance is still of concern to the IS community. The relevance issue was followed by the concern for the field's identity and legitimacy, which evolved into the question of the field's core [Agarwal and Lucas Jr., 2005, Alter, 2003a, Benbasat and Zmud, 2003, Dufner, 2003, El Sawy, 2003, Gray, 2003, Robey, 2003] and the centrality of the IT artifact, highlighted earlier by Weber [1987] and resurrected later by others [Alter, 2003b, Orlikowski and Iacono, 2001]. Findings and conclusions from studies concerning the core of the IS field are varied and often contradictory [Grover, 2012, Lyytinen and King, 2006, Sidorova et al., 2008, Weber, 2006, Zhang et al., 2011].

They all may seem to be different topics of discussion, but when examined closely, all of these issues relate to each other at various levels. The need for cumulative tradition is closely related to whether or not the IS field has extracted itself from its reference disciplines enough to build upon its own research traditions. In order to do so, it should be able to stand on its own identity and theoretical foundations, which suggests some kind of "core." Only when the field has its own identity and theoretical foundations can it be expected to influence other fields or industry practice in a significant way. This article argues that all of these concerns are manifestations of an underlying problem in the degree of importance or worth of the product of IS research. The notion of the "value of IS research" captures this underlying problem. Anecdotal evidence from recent panels at ICIS 2012 reflect similar concerns (Table 1)

Table 1: ICIS 2012 Panels and Workshops	
Panel/Workshop	Excerpts and Comments
Niederman, F., Lyytinen, K., Ahuja, M., Tan, B. "Senior Scholars Panel"	"I've been looking back at all the work that I've done… I don't think I've done enough ... We really should be doing research to resolve significant global problems … at NUS, we've put together an inter-disciplinary team and received a big grant using social media to reduce pests and increase food production ... Let's not be too fixated about whether *MISQ* or *ISR* would publish such research ... let's target our work at *Nature* or *Science* .... to earn the respect of others outside our community" [Bernard Tan] "After we have had the satisfaction of publishing in *MISQ* or *ISR*, and at least after making tenure and Full Professor, it is time to start thinking about researching big ideas that make a difference" [Manju Ahuja]
Straub, D., Grover, V., Lyytinen, K., Weber, R. "Panel on 'Native IS Theories' at the Concurrent – ICIS 2012 Special Interest Group Philosophy and Epistemology in IS (SIGPHIL) Workshop on IS Theory: State of the Art"	"We can do very good research, but that doesn't mean we are producing good knowledge … can we create 'better' knowledge?… exciting, innovative and addressing important questions of our time … are we scripting the way we do research ... requiring us to select a theory and apply it to our phenomena, create a mid-level model and then refine it ... add mediators and moderators to it … Does this script work for good knowledge?" [Varun Grover]
Lee, A. Chiasson, M., Alter, S. and Krcmar, H. "Long Live Design Science Research! .... and Remind Me Again About Whether It Is a New Research Paradigm or a Rationale of Last Resort for Worthwhile Research That Doesn't Fit Under Any Other Umbrella"	DSR is increasingly governed by a script that makes papers easier to review but … becoming an obstacle to genuine innovation … script encourages DSR researchers to do design-related work in a way in which few, if any, designers actually design things in the real world, which is especially unfortunate for a type of research that is called design SCIENCE research [Steven Alter]

Recent postings to the Association for Information Systems (AIS) listserv on the issue of the value of IS research reflect many of the same concerns (Table 2). Selected anonymized postings are also included.

Table 2: AIS Listserv Postings	
Source	Posting
Robert B. Johnston University College Dublin	"The idea that we do good research but do not produce good knowledge is intriguing. However, we need to delve further and explain how this can be so. This observation seems to indicate that (well executed) research methods do not fit the phenomena being studied. To me, this is because much of IS research is attempting to apply reductionist methods to a holistic phenomenon, namely the embeddedness of technologies in human practices."
John J. Sullivan University of South Florida	"First, let me say that this was an eye-opener when I first read it, and it took great courage to bring this issue to light. It seems that promotion and tenure objectives compete with a desire to pursue academic ideals … So, our practical side tempers the aspirations of our idealistic side. … wouldn't practical application of research be of greater value (e.g. better knowledge) to practitioners in our field? It has been my experience that we don't seem to place as high a value on the practical application of our theories as we do the theories themselves. Isn't it better knowledge for our field if our research inspires techniques that help managers run better projects, reduce waste, develop better systems?"

Table 3: AIS Listserv Postings – Continued	
Ilia Bider Stockholm University	"On the question 'What kind of IS research is valuable anyway?' The following citation from Kurt Levin gives a good hint: 'There is nothing more practical than a good theory.' It is not only a statement about what is good for practice, but also a definition of what a good theory is, i.e., a theory that can be useful in practice. The next logical step is … the theory should be understood by those who do work in practice, which in turn requires radical change in the style of scientific publications in IS."
John Artz George Washington University	"I would point out that physics was around for 2,000 years … before it began to produce really impressive results. If we are willing to wait for another 2,000 years for the research in Information Systems to eventually lead to something, I am confident that the field will produce some really impressive results as well. However, if we do not wish to wait that long we might ask what we can do to speed things up a bit. … I would like to offer a few fairly simple questions, the answers to which would greatly speed up progress in the field. What are the constituents of the field? That is, what is the field about? What are we studying? For that matter, what is an information system? (The text book definition is almost worthless for research purposes). What would we like to know about the constituents of the field? How are we likely to find out the things we would like to know?"
Helen Hasan University of Wollongong	"I could not agree more that we must look for methods and theories to support research into the complex holistic phenomena you describe. I would add to this the imperative to engage in projects that can really make a difference, thus bringing quality IS out of the confines of academic publications to help solve critical world problems. The AIS's SIGGreen and SIGGLOB bring together IS researchers looking at prominent global problems where IS knowledge and expertise has much to contribute but is rarely mentioned outside of the IS community. I commend Rick Watson's idea of seeing IS as a 'solution science' tackling important big problems."
Manuel Mora Autonomous University of Aguascalientes	"Compared to Latin American IS researchers, Latin academics in Computer Sciences, Biology, or Mathematics in Latin institutions are much more recognized and published in top worldwide journals … our discipline has lost the humility to recognize the value of MIS research in small business, and outside countries of the main top five IS journals. Research in Computer Sciences, Medicine, and Biology -as examples- is conducted with the involvement of the practitioners experiencing the research problem. As a result, they really want to find a better solution, create theoretical knowledge or construct an effective research tool; they live the real problem and suffer jointly with users and enjoy the solution! In MIS such values were always present in the early 60'-80s period, but it seems we have adopted other goals … improving our academic positions and monetary incomes … We are doing IT research for the sake of ourselves. It is a real problem."
Direct Replies to Postings with Sources Withheld	
"A senior IS professor told me that people should stick with non-controversial research until they reach full professorship …. fresh PhDs therefore tend to work on "small" problems doing rigorous research rather than taking more risks and addressing bold issues. These same senior researchers are frustrated about the lack of interest in young professors to push boundaries of knowledge …. if a discipline indirectly discourages innovation and risk taking in their PhD students and early career researchers, how can these young researchers suddenly show an interest in the opposite to what they built their career on?"	
"I think if I had it to do all over again, I'd have gotten a Ph.D. in Electrical Engineering and been working on renewable energy solutions. THAT's the type of problem that gets me excited and is a real problem with real impact that really needs more help addressing … Some of the things we publish … have really no practical use, but a car that goes 500 miles on a battery charge sure does! Or a real smart-grid that levels out the usage of electricity so that we're not running so many coal-fired power plants just because we want to do dishes at 8pm instead of 3am while we're sleeping. There is plenty that IS could contribute to these important questions, but little that our field seems to value or wants to pursue. Do we have an inferiority complex that we're afraid to tackle these bigger problems and instead satisfy ourselves by demanding increasing levels of scientific rigor to answer questions that very few people care about? At least the Health IT researchers can measure much of their impact in dollars and/or lives saved."	

Other than the oft-cited notion that IS research is not relevant to IT practitioners, a few more pertinent comments concerning the value of IS research can be found in academic IS journals as well. A few of them are cited in Table3.

Table 4: Evaluation of IS research in academic journals and reports	
Source	Posting
Senn [1998, p. 23]	The discovery of critical problems in IS does not occur within the academic research community … The solution to critical problems, with later transfer to practice, does not originate or occur within the academic community
Moody [2000, p. 353]	Research is not driven by practical needs but the interests of researchers and demand from publication outlets.
Davenport and Markus [1999, p. 22]	While not all IS academics need to do relevant research, we must *value* it [emphasis added]. That is, we must read it, teach it, apply appropriate evaluation criteria when asked to review it, respect the journals that publish it, and honor those of us who do excellent relevant research. In short, we must eat our own dogfood.
Schauer [2007] Lange [2005a, 2005b]	[includes interviews with eight senior IS authors from the US and six senior IS authors from Europe] "we had very little credibility in the early days; it was a real struggle to get courses introduced into MBA programs" "The results and the research [..] are not having the impact, or they're not being perceived as this leading edge." "A major part of the research in our field is still done to get tenure, which means that you look at the small things that contribute to the discipline, rather than contribute to real problems. [..] But I think increasingly we see much more effort aimed at real problems."
Truex et al. [2006, p. 814]	A few years ago, Ramiro Montealegre and I [Mark Keil] were presenting our process model of de-escalation based on the Denver International Airport case study, and Barry Staw was facilitating the session. I remember before the session, Staw approached us and complimented us on what a fine paper we had written and how *surprised he was that it was written by some IS researchers* [emphasis added]. In 1999, I was asked to chair a session at the Academy meeting on escalation and most of the papers in the session were written by escalation researchers rather than IS researchers. I don't know how they picked me, but the fact that they did suggests that at least some of my work has made an impact outside the field of IS.

The body of evidence, in particular, the last three sets of quotes, support examining the notion of the "worth" or "value" of IS research. This idea is not new. Gordon Davis, considered the founding father of the IS field, had proposed a set of criteria for evaluating the significance of any research. In the classic guide for doctoral candidates [Davis and Parker, 1979], he proposed that the prospective doctoral candidate should look at:

(1) problems relating to social welfare, business, economics, education, and government
(2) past dissertations
(3) authorities in the field
(4) suggestions from practitioners
(5) generally accepted but unproven suppositions
(6) unproven or weakly proven assertions by an authority, and
(7) different approaches to testing important results.

This framework predicates the value of the research on the extent the research addresses societal problems, on how it adds or follows up on past dissertations, on how far the authorities in the field agree that it is significant, or on how relevant it is to practitioners. The research is also considered valuable if it disproves a generally accepted but unproven supposition, confirms an unproven or weakly proven assertion by an authority, or provides different approaches and methods to testing important results.Based on the evidence provided so far, it does not appear that IS researchers religiously pursue Gordon Davis's suggestions to look at societal problems and to first ask practitioners. If this is true, the second and third suggestions will only reinforce the status quo because past dissertations and authorities in the field will most likely not reflect societal problems or suggestions from practitioners. Additionally, based on the modest progress shown in addressing Keen's [1980] challenge to build a cumulative tradition, the field as a whole appears to be neglecting the last suggestion of accumulating different approaches to testing important results. This leaves the fifth and sixth suggestions. A perusal of the archive of IS research suggests that due to the field’s fragmented nature, IS authors are only recently examining and testing suppositions or accepted ideas, or studying unproved or poorly proved assertions [Benbasat and Barki, 2007]. Lack of research following the sixth and seventh suggestions is probably why the IS field does not demonstrate a cumulative tradition in the first place. Hence, a Catch-22 situation develops, therefore supporting Senn's [1998] opinion that discovery of critical problems and their solutions do not take place in or originate from IS research.

II. THE MEANING OF VALUE OF IS RESEARCH

The meaning of value under discussion is not in the sense of ethical or moral values, although those may be relevant, but "value" as a property of a physical or abstract object representing its degree of importance or worth. So the kind of value we're discussing is in the sense of value that is socially determined. Some things have instrumental value while others are said to have intrinsic value. So, when researchers publish with the goal of advancing their careers, it is clear that the research has instrumental value to that researcher, but not so clear as to whether it has value to others. This sense of value is the crux of the discussion in this paper. Before the meaning of the phrase "value of IS research" can be discussed, it is necessary to clearly point to the object that is being valued—IS research. What is being evaluated is the product of the academic field or intellectual enterprise that is currently referred to as "Information Systems," "Management Information Systems" or "Computer Information Systems," which, depending on the location, will either be housed in the schools of business of four-year universities or perhaps together with computing disciplines in the schools of sciences and engineering.

It is important to distinguish between the community of practice of IS [DeSanctis, 2003] and the community of practice of the broader post-industrial [Bell, 1973] subject matter of computing and information technology because many fields of study stake their areas within the same subject matter. Management, computer science, information science, medical informatics and various engineering fields all undertake research in the same area and as a result, there is a tendency to confound the academic field with the subject matter in question. In the high-tech and digital world we live in, there is no question about the value of the subject matter of information technology, computing or information systems. Although some may prefer to include this broader community as part of the IS community [Ramesh, 2001], the discussion in this paper is about the academic field that is associated with specific professional organizations (e.g. the Association for Information Systems), the members of which regularly publish in certain journals (*MIS Quarterly*, *Information Systems Research* and others) and regularly attend specific conferences (e.g. ICIS, AMCIS and ECIS). Limiting the scope of the IS field in this way will avoid the complications associated with anyone in the field claiming that Herbert Simon and Russell Ackoff are IS scholars, however much they have contributed to the IS field. The concern of this paper is the value of IS research that is ex post facto the contributions of such scholars.

Value in Terms of Ends-Means

There are various ways of viewing what is meant by the value of research. As a pragmatist, Dewey [1939] proposes an ends-means approach to the theory of valuation. His theory of ends-means goes beyond maximizing utility and utilitarianism to explain the complex relationship between multiple levels of ends and means. A simple view of ends-means might take the form of doing research, say, to cure cancer. Such research is clearly valuable, However, just like research for space travel, many levels of ends and means factor into the evaluation of that research. For example, when a physician researches a course of action for a patient, the research is done to restore the patient to "health." Others may assume that this ultimate good called "health" is the physician's absolute end in itself. In reality, the physician does not do so. Instead, the physician views health based on the condition of the patient and the techniques of examination that help the physician overcome certain "problems" the patient has. It is not a fixed *a-priori* state, but the result of a set of inquiries. The physician may decide that a particular state of health may be unattainable. The same goes with science. An end-in-view is used as a means of directing further observations. As a result, just like a physician may order a stress test to evaluate a patient's heart condition, which is an end in itself; the evaluation of the patient's heart may end up becoming a means towards another end, perhaps relieving the patient of a clogged artery. The ends are valued based on their service towards relieving a problem based on an analytic observation of existing conditions, not some abstract ideal. In this sense, the attained end doesn't become a fixed unchangeable goal, but an organization of activities. In the case of the physician, there is an end-in-view which is the special activity of keeping the patient's heart healthy, that coordinates the organization of all sub-activities. The better the coordination of all these activities, the better the state of affairs of the patient. The next day might invite a new end-in-view, and it is this sense of progress that is required of science.

Thus, research is not supposed to end when an article is published in the best journals. Getting there is just one of the many milestones which are part of an organization of activities that contribute towards the intellectual ideals of the research community. Researchers can list out various benefits of the research at different phases and different levels of value. For example, in economics, research in pricing of agricultural products and market information benefits traders, farmers, consumers and policy makers. Economic models and econometric techniques predict niches for new products and innovation in allocation of resources [Zilberman and Heiman, 1997]. Analyzing this wide impact horizon will help IS researchers determine which of their research is most valuable. Some research is not recognized for decades as was the case with Mendel's work with hereditary traits [Brannigan, 1979]. Nevertheless, that research was invaluable for molecular biology and contributed to a new state of affairs for the nascent field of genetics. These kinds of means-consequence analysis of research, a *respice finem* (have regard for the end), does not simply mean having an end-in-view for which anything goes, but examining to make sure that the consequences that will actually result are such as will be valued when they are achieved. For IS research, evaluating potential research is not only a reaction to some problem to be resolved, some need or deficiency to be made complete, it involves an evaluation of the problem itself, its significance, and how it may supply other existing needs or resolve existing conflicts. In the same way, IS research on information sharing, which may not have anything directly related to agriculture, can be applied in agriculture. Even if the information-sharing research is able to reduce pests by a mere few percentage points, it contributes to a new state of affairs in world food production [Niederman et al., 2012]. Often, the ends of the research in any area benefits those who are not in that area, just like how economic research in agriculture is supposed to benefit not economists, but agriculturists and beneficiaries of agriculture—the whole world.

Value of Basic IS Research

Notwithstanding what's being said about considering all the possible consequences of the research, the issue of the value of basic versus applied research is relevant in IS research. There is a general agreement among IS researchers that IS is an applied field. The problem with this assumption is in how it limits the contribution and therefore potential value of IS research to only "practical" problems. IS as an applied field implies that IS is dependent on theory and “science” of other fields. Therefore, progress is primarily guided not by the genius or efforts of its own scholars, but the work of scholars of its “referent disciplines.” The “applied” nature of IS also suggests that IS has little to offer other fields. As a result there are no "laws" to be discovered in IS. Limiting IS to basic and applied research is neither useful nor productive to the IS field’s continuing efforts towards maturity. At its worst, it will impede the progress of the IS field. As Thomas Huxley [1881, Vol. 3, p. 155] wrote in 1880:

> I often wish this phrase, “applied science,” had never been invented. For it suggests that there is a sort of scientific knowledge of direct practice use, which can be studied apart from another sort of scientific knowledge, which is of no practical utility, and which is termed “pure science.”

As Nelson's [1959] classic article on the economic value of basic research notes, although scientific knowledge provides value when it can be used to predict the results of a practical problem, the value of the basic research are often found in inventions that have little to do with or are not directly related to the goals of that research. And as Nelson [1959] emphasizes, history has shown that it is basic research that results in significant advances, not applied research.

Norman Campbell [1921/1951, pp. 1-3], another noted philosopher of science comments:

> Students of pure science denounce those who insist on its practical value as base-minded materialists, blind to all the higher issues of life; in their turn they are denounced as academic and practical dreamers, ignorant of all the real needs of the world… the two forms of science, whatever may be their relative value, are in fact inseparable. The practical man is coming to understand that the earnest pursuit of pure science is necessary to the development of its practical utility…And academic students are finding that the problems of practical science often offer the best incentive to the study of pure science, and that knowledge need not be intellectually uninteresting because it is commercially useful.

Two famous scholars sum up the inseparable nature of the so-called pure and applied fields. Immanuel Kant [1983], in his essay on *Theory and Practice* argued that it is incoherent to say what may be true enough in theory doesn’t apply in practice, instead, the problem is most likely because the theory is incomplete. Kurt Lewin [1951, p. 169] succinctly embodied the interrelationship between theory and practice when he wrote, “There is nothing more practical than good theory.” Politically, the division between pure and applied fields performs a useful function for the purposes of allocating resources in science studies and science policy. For researchers, the division is at best vague and unproductive.

The tension between rigor and relevance is really an outgrowth of this underlying artificial tension between the pure and the applied. The solution to the problem lies in understanding the different needs of the practitioner and those of the scholar. When the practitioner applies knowledge from research, the practitioner requires immediate solutions to problems and will often “satisfice” [Simon, 1957] instead of optimize. The practitioner weighs the cost of further investing in time and resources to reach a more valid conclusion and decides that one less rigorous is adequate for the time being. This decision of the practitioner does not mean that the practitioner will ignore results from more rigorous studies. However, in the circumstances where there are no good choices, the one most relevant to his context will be selected.

Unlike the practitioner operating under the constraints of the business environment, the IS scholar should not be burdened with difficult “satisficing” choices. The scholar searches and invents knowledge and focuses upon topics and concepts that characterize the discipline’s knowledge structure. In this search for knowledge, the scholar believes that certain laws underlie the phenomena being studied. In the case of IS, these laws help organize the immensely complex environment involving people, technology, society and politics and help explain to stakeholders the core concepts of the field. This disciplinary practice needs to take place under rigorous conditions to ensure validity and respectability. Especially in the construction or discovery of societal laws, relevance is primary, and rigor contributes to their relevance.

The significance of IS problems: Asking the right questions

Exploring the ends-means of research programs provides a way for researchers to evaluate the significance of their research towards particular ends. Viewing IS research as inseparable from basic research moves it beyond narrow practical problems and domains, and increases the value of the research as it becomes part of the body of knowledge of science. But these proposals do not address how to evaluate the significance of the problems in and of themselves. Are there such things as problems that are intrinsically valuable? Dewey [1939] considers the phrase "intrinsic value" a contradiction in terms because in order for something to be valued, it has to come from something or someone external to the object being valued. Gold is intrinsically valuable because people value its rarity and its properties. But when such properties can be cheaply manufactured and it no longer becomes rare, it is unlikely to be valuable. In the same way, problems that are significant in IS are evaluated as a result of the circumstances surrounding the problem, and those circumstances can be empirically observed and qualified in some way.

No human activity, research included, operates in a vacuum. The intellectual ideal of the associated field in which the research is performed determines the significance of the problem because the problems arising are closely related to the means for resolving them. In the business disciplines, money making, profitability and efficiency may be their end view. In these disciplines, the problems related to those same concerns become significant. Even though business management and economics overlap and inspire each other, business management ideals do not totally overlap economic ideals. The former is about doing work through other people, while the latter is about human needs, wants and how they are satisfied. Therefore a problem in a field becomes significant when it addresses and concerns the intellectual ideals of that field. And the intellectual ideals of the field concern issues which the field is especially designed to address. Unfortunately, in the IS field, there is no agreement on its intellectual ideals, which therefore makes it difficult for any IS researcher to say if a particular problem is its specialty and therefore is significant or not. What is usually provided as an excuse for not agreeing on such an ideal is the field's multidisciplinary origins. This is not a valid excuse because when examined closely, many established disciplines started out as multidisciplinary fields. Molecular biology, biochemistry, management, even political science had origins in other fields of study, but their scholars were able to invent, create and synthesize the differing multidisciplinary discourses into their respective unique, independent and autonomous fields of study having their own specialties.

In the absence of any guiding rules to describe the IS field, perhaps it is instructive to identify what is NOT considered IS. Since IS research emerged as a formal field of study in the 1960s [Davis, 2000], it is reasonable to assume that as a discourse, it developed some time before that period. Indeed, historical studies concerning IS trace its discourse back to the implementation of the first office computer in England in the early 1950s [Hirschheim and Klein, 2012] and as early as the late 1940s [Hassan and Will, 2006]. During that period, all the so-called reference disciplines of IS had been around for decades [Culnan, 1986, Culnan, 1987, Culnan and Swanson, 1986, Davis, 2000] – management and decision sciences, organizational psychology and organizational behavior, computer science, economics and systems theory. It is clear that IS is not only management/decision sciences or organizational psychology/behavior because these disciplines focus primarily on the human element within organizations. IS is also not only computer science that focuses primarily on algorithms and machine structures. Clearly, IS is not only economics and systems theory. It has, in its short history, created an interdisciplinary space that straddles the discourses of all these disciplines and is often referred to as the socio-technical space. The question is, what constitutes the field of IS in its own right rather than a confluence of elements from somewhere else? What makes IS an academic field in its own right was because there were questions that were not asked by these other disciplines that many people felt should be asked. These other disciplines did not address significant problems that were emerging as a result of the invention of the general purpose computer.

Novel questions on computer “operations” and “use” were being asked in different scholarly articles, and in the 1950s, books were being written to answer questions that fell outside all these other [Canning, 1956, Kozmetzky and Kircher, 1956, Laubach and Thompson, 1955]: (1) What other possible ways can businesses harness the power of computers? (2) How can organizations persuade their employees to use computers? (3) How should an organization plan the scheduling of applications waiting to be implemented? (4) How should organizations select the equipment needed for each application? (5) How do organizations staff the planning, design, programming, and operation of such systems? (6) How can the necessary cooperation be enlisted from within the organization for the planning, design, and operation of such a system? (7) How can the benefits be evaluated and presented in an understandable form to those paying for it or using it? (8) What characterizes the communication between humans and computing machines? (9) How can IS be designed as mental and cognitive support for its users? In those days, these questions fell under the umbrella subject of "data processing" and were given the acronym EDP and became the concern of practitioner DP departments. These are new questions that no one has asked before.

Certain practices of computing such as “goals and methods of adoption,” “company-wide support for the adoption,” and “measuring and monitoring the progress of adoption” become significant problems that needed solutions. But as Markus [1999] argues, given the current environment, the traditional concerns of the centralized mainframe-driven IS department may no longer be significant. It is not that these problems are not characteristic of IS. As Toulmin [1972] says, academic fields not only address specific problems, they address a genealogy of problems. The kinds of and genealogy of problems are similar and constitute the nature and characteristic of the discipline, but the specific problems have moved on. The kinds of questions being asked may be the reason why there are concerns about IS research not generating "good knowledge." Disciplines need to demonstrate a kind of vitality as it addresses society's problems. It reads its environment, finds opportunities, responds to threats and charts its way based on what it encounters. Addressing these questions in the same way how management addresses them, or how psychology researches them, or how sociology studies them will not make up a significant contribution because the IS field will be duplicating what they have already done. As a result, IS becomes like management, psychology or sociology and loses any identity it has struggled to build. The genesis of a significant contribution is asking the questions that these disciplines have not asked or are incapable of asking. When this happens, IS researchers will have little choice but to be creative in conceptualizing and approaching their own research.

III. MAINTAINING THE VALUE OF IS RESEARCH

Assuming that IS researchers have figured out and are asking the right questions that other disciplines have not asked before or are incapable of asking, how do we know that the responses to those questions are truly significant? Perhaps we may ask the right questions, but end up relying totally on our reference disciplines for goals, concepts, theories, methods and results. Instead of innovating and creating our own concepts and theories, we reuse tired old concepts or extend existing theories in ways that contribute little to understanding of the IS field itself. As Foucault [1970, 1972] explains, significant contributions to the body of knowledge follows a distinctive route that is reflected in how the discourse in question positions itself within the constellation of disciplines.

Valuable Research is Original

The most oft-quoted characteristic of valuable research is its originality. However, what is considered original is not as straight-forward. Were Darwin’s [1859] notions of evolution original when in fact, Lamarck [1809/1960] had already described the same ideas half a century before him? Was Saussure’s theory of semiology original, whereas Pierce and Locke before him had already defined semiotics? Why did Newton write in his letter to Hooke in 1676, "If I have seen further it is by standing on ye shoulders of giants." In light of this statement, how much of his work was really original? Originality cannot simply mean the idea or notion was *not* documented earlier. Isaac Newton’s contribution was monumental, even if the same ideas had originated from Galileo or Robert Hooke before him. We need to grasp how Isaac Newton made them original even when the ideas themselves appear to be identical to those that antedated them. Within the context of the originality of IS research, Richard Hamming’s [1969, p. 10] warning in his Turing Award speech is especially poignant:

‘Indeed, one of my major complaints about the computer field’ is that whereas Newton could say, 'If I have seen a little farther than others, it is because I have stood on the shoulders of giants,' I am forced to say, 'Today we stand on each other's feet.' Perhaps the central problem we face in all of computer science is how we are to get to the situation where we build on top of the work of others rather than redoing so much of it in a trivially different way. Science is supposed to be cumulative, not almost endless duplication of the same kind of things.

It is vitally important for the IS field to grasp this notion of originality. A major part of being original comes in asking the questions that no one else had asked. Asking the right questions is the starting point of creating original research. Foucault suggests that once the discourse is established, it needs to sustain itself by remaining "active."

Valuable Research is Active and Not Passive

Foucault [1972] says that original research is always “active” and not “passive.” What Foucault means by the active nature of the research is how the research puts into operation a new set of rules that changes the way the object of the discourse is manipulated, how the concepts are employed and how theories are formed. Even though every statement in a specific discourse bears certain regularities such that it is saying the same kind of thing another statement in the field is saying, original research provides some sense that is “different.” For example, Darwin’s [1859] ideas were similar to Lamarck’s [1809/1960] half a century earlier. Within this regularity, it can be said that Darwin copied Lamarck. But the discursive practice of Darwin is different from Lamarck. Lamarck offered a discourse closer to cosmology rather than biology [Foucault, 1972]. Research may use exactly the same words as its antecedent, but those words imply different concepts and may even become part of a different theory. Thus, the term “structure” in organizations is used in a different way from the same term “structure” in biology. Each one belongs to different discourses (according to our analogy, different disciplinary trees) and provides a different sense of what is discussed. Each discourse applies its unique rules on the terminology, making it possible for the discourse to create new concepts to explain something different. This creative work needs to be encouraged in the IS field. Is "architecture" in the IS field the same as "architecture" in computer science? When systems analysts extract "requirements" to build a system, are these the same "requirements" that software engineers collect when they undertake "requirements engineering?"

As history has shown, it is always possible for the same terms to mean different things. In economics, Gresham’s [Le Branchu, 1934] and Locke’s [1696] formulations of relations between money and prices in the 17th century use the same terms as those used by Smith [1776] and Ricardo [1817] in the 18th and 19th centuries. But the latter economists apply a different set of rules of discourse to those terms, thus making their research valuable to the discipline of economics. On the other hand, the use of different words does not always imply the research is original. In the IS field, the many words used to describe the nature of Internet-related businesses (e.g., e-commerce, internet commerce, online business) may be different, but they don’t imply the use of any different rules. These words are mere shadows of each other and demonstrate imitative rather than creative and active characteristics. The activity of searching for concepts and theories in other disciplines and then creating acronyms to represent those same concepts and theories in the IS field add little if anything to the field. They merely represent passive shadows of the same concepts and theories, with the exception that some kind of technology is involved. The challenge in creating original research in IS lies in inventing concepts that manipulate the objects of study of the field in different ways from their position in the borrowed discipline.

Valuable Research Makes Evident What is Not

Research contributions can also be measured in terms of the degree that the research lays bare or in terms of how it uncovers the shroud that prevents mankind from understanding the object of study. For anything to be of value there will always be an unconscious, sometimes, intentional power to blind, to hinder or prevent its discovery [Foucault, 1972]. The greater is the exposure, the greater is the value of the research. For example, the secret of genetics was hidden until Mendel uncovered it. Even after its discovery, the environment around Mendel actively sought to stop him from making his findings known [Brannigan, 1979]. The same occurred with the discovery of light as waves. Newton’s particle theory effectively hindered Huygen’s wave theory of light to be accepted as valid [Foucault, 1972]. Similarly, the workings of firms and how they create value was hidden until Porter [1980] stripped the layers that surrounded the chains of relationships within the firm. The value of the research is reflected by the difficulty of enunciating something new, something that enlightens the observer concerning certain relations between social institutions and processes, norms, types of classification and techniques.

Foucault [1972] calls these hidden relations “discursive relations” because they make it possible for people to have unlimited discourse on the object of study. These relations are to be distinguished from the everyday relations that can be visibly observed. For example, in every firm, it is obvious that the CEO manages or directs the heads of departments and these heads of departments instruct supervisors who in turn manage a group of employees. It is also obvious that the attitudes or problems that supervisors might have will likely affect the subordinates under their care. These relations Foucault [1972] calls the “primary relations” between the different individuals or institutions. When a researcher writes about or reflects on the relations occurring between supervisors and their subordinates, the tripartite relations Foucault calls “secondary relations.” Both kinds of relations are useful and often become the material for consultants and practitioners but are still incapable of producing the "unlimited" discourse from discursive relations.

Discursive relations are relations that uncover objects of study to be revealed, provide a set of rules such that it is possible to speak of this or that object of study, and to classify or analyze them [Foucault, 1972]. Thus, in the case of the relations between the supervisor and their subordinates, what is valuable is not the content of the tasks delegated to the subordinates and how well the subordinates perform them, nor is it the reflections of the researcher studying and writing about those tasks; what is valuable is the discovery of enduring objects that emerge every time the same kind of rules are established. One instance of such valuable concepts is what management theorists have been calling “the span of control,” a concept that not only explains a variety of effects taking place in management, and is able to classify or analyze the relations themselves, it has sustained itself through the nearly a century of evolution in management research. This is an object of study that emerges out of a "discursive relation," the result of creative work which contributes to the value of the research.

Research that borrows theories from other disciplines and applies them in the context of IS may be useful, but they need to be measured according to how much they disclose or uncover. For example, in IS, stating that collaborative technologies increase the level of participation in an organization only reflects the "primary relations" between the technology and its implementers. It does not provide anything new nor does it uncover any insights that might assist the organization in improving its performance beyond what it already knows. This does not mean that these kinds of research are not useful. Most of these types of research become very useful in a consulting environment. They become proven methods and techniques that consultants can employ to increase the effectiveness and efficiency of the organization. But they are not as valuable as far as the body of knowledge of IS is concerned. Similarly, in the early days of IT when technology was not as prevalent, it may not be obvious that when people are comfortable with technology, they tend to use it more, but today, with the ubiquity of technology, the relations between perceived comfort with technology and its use have become primary or secondary relations, and may no longer support a discursive relation worth a research program.

IV. CONCLUSION

The article began by suggesting that the underlying problem triggering the recurring anxiety discourses is the worth or value of IS research. The notion of the value of research is not just about relevance or usefulness of research. It is closely related to the intellectual ideals of the academic field, its goals and mission, intellectual structures, activities and relationship with its environment. Various aspects of "value" are explored including the pragmatic view of ends-means, the unproductive political view of differentiating between basic and applied research, and the need to ask the right questions in evaluating the significance of IS problems. The value that IS research provides comes from addressing the questions that other disciplines have not addressed or are incapable of addressing. Much of these questions are located at the boundaries between knowledge and disciplines. Disciplinary studies note that the most interesting and most productive research occurs at these cross-disciplinary boundaries [Gieryn, 1983, Klein, 1990]. Keen [1991, p. 27] alluded to these kinds of research when he emphasized the potential value of IS research which should be at the "forefront of intellectual debate and investigation about the application of information technology across every aspect of business, government and society." Somewhere in the IS field's history, its potential or value was obscured such that IS was omitted from the draft version of the Association to Advance Collegiate Schools of Business (AACSB) Accreditation Standards document [Ives et al., 2002]. Despite such setbacks, the IS field is still among the few fields capable of addressing the questions that many disciplines today are incapable of answering.

The unique intellectual structure that the IS field has developed thus far provides a unique formation for research that straddles both the human and technical domains. Since the early 60s, many scholars agree that today's societal needs are unique and unprecedented [Bell, 1973, Castells, 1996, Machlup, 1962]. Ever since scholars began writing about how computers have ushered in a new information age, the management of the information created by such a revolution was appropriated by several different fields of study ranging from computer science to information science. But even the computer science field admits that the landscape of information and its technological enablers has created new problems such that the concerns surrounding them have become philosophically "virgin territory" [Floridi, 2003]. New conceptual problems, unprecedented issues, novel theories and ideas are increasingly demanding new approaches. This virgin territory is not exclusively a computing issue, nor is it exclusively a management issue. This void is pregnant with questions that are not being addressed. The IS field offers a complementary "non-mechanistic" lens to computer science that views information in the way Floridi [2003] describes as "demiurgic" (a creational power) making "possible the construction, conceptualization, semanticization and finally the moral stewardship of reality, both natural and artificial" (p. 465). The IS field offers management and the human sciences the bridge to the realm of "technoscience" [Latour, 1987] that accepts technology as necessary agent in the evolution and progress of knowledge [Ihde, 1979]. The question is, will IS scholars pick up this challenge and begin charting their own direction in the wilderness of today's digital world?

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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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Communications of the Association for Information Systems

ISSN: 1529-3181