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MAKING INFORMATION SYSTEMS RESEARCH MORE VALUABLE

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Abstract
This essay explores research characteristics beyond those of rigor or relevance to propose a framework that will make information systems (IS) research more valuable to its stakeholders. The value of IS research ought to be measured by its originality, how well it addresses the needs of its sociological and discursive environment, the extent the research makes evident what is hidden, and how closely it adheres to the laws that rarify it. The originality of the research is in turn defined by its subordination to its disciplinary subject matter and how actively the research manipulates the objects and concepts that it forges in the process of creating knowledge. These characteristics define the value of IS research and what ultimately makes the research relevant and sought after.

Keywords: Philosophy of information systems, Foucault, value of information systems research, relevance, originality, rarity, concept formation.

The Significance of the Research
In any dissertation proposal, “the significance of the research” section is probably among the more difficult sections for the doctoral candidate to defend. The significance or value of the research is often interpreted as the “need for the research.” In Gordon Davis’s (1979) popular guide to writing dissertations, this “need” is operationalized by asking the prospective doctoral candidate to 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. Unfortunately, a glaring disconnect exists between these ideal justifications for its value and what is happening in IS research. It is commonly accepted that the IS field suffers to some extent from problems of relevance (Applegate 1999; Benbasat and Zmud 1999; Davenport and Markus 1999; Glass 2001; Keen 1991; Moody 2000). Benbasat and Zmud (1999) suggested several ways of enhancing the relevance of IS research including looking first at practice before reviewing the academic literature and making sure that the research is readable. Despite efforts within the IS field, the issue appears unresolved (Desouza et al. 2006; Gray 2001). If it is true that most IS researchers do not religiously pursue Gordon Davis’s suggestions to look at societal problems and to first ask practitioners, 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 nor
suggestions from practitioners. Additionally, based on the marginal results (Farhoomand and Drury 1999) from 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 themselves spend little time in testing suppositions or accepted ideas, or studying unproved or poorly proved assertions. 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.

This issue is often blamed on rapid technological changes the field suffers from, or the dynamism of its subject matter (Davenport and Markus 1999; Farhoomand and Drury 1999). Others suggest finding a middle ground between rigor and practical relevance (Benbasat and Zmud 1999; Gray 2001). A potential solution to this dilemma lies in viewing the value of the research as something that goes beyond rigor or relevance. The value of the research is ultimately what determines whether or not it is relevant, and later practical, for the purposes of those interested in it. If a particular study is not considered valuable, it is unlikely to contribute significantly even if a middle ground is found between its rigor and its relevance, or even if it is written up in a more readable fashion. Very few studies address what is considered “valuable research” in IS. The goal of this study is to propose several constructs that IS researchers can use to assess the value of their research.

Much of what is proposed in this paper is inferred from the philosophical works of Michel Foucault and his contribution towards understanding how knowledge is created. In order for knowledge to emerge, many conditions must be fulfilled and all these conditions are described in two of his famous writings, *The Order of Things* and *The Archeology of Knowledge*. The linchpin of this sophisticated analysis, which uncovers how knowledge evolves, rests on the unique definition of the “discourse.” A detailed exposition is available in Hassan and Will (2006), and only a brief introduction is reproduced here. Foucault (1972) uniquely defines a discourse as “a group of statements in so far as they belong to the same discursive formation” (p. 117). These “statements” are sentences, symbols, propositions or formulations that relate one object of study to another within “an operative field of the enunciative function” (p. 106). A discourse is therefore made of a limited number of statements that obey a group of conditions for its existence, a set of rules Foucault calls the “discursive formation” (p. 117). This set of rules distinguishes a field of study from other fields of study, represents the identity of the field and becomes the engine of transformation that enables the field to produce knowledge.

For example, in the natural sciences, the set of rules that distinguishes the field of biology from other fields is the set of rules surrounding life’s organic structure. Biological discourse obeys this set of rules and any statement that demonstrates the operation of such enunciative functions belongs to the discourse of biology. This discourse is very different from its antecedent fields of botany or Natural History. Although they both study living beings as does biology, Natural History’s discourse involves rules of classification based on visible properties. This set of rules that defined Natural History limited what it could accomplish in the study of living beings. Biological discourse broke this rule and as a consequence of operating its new set of rules, was able to replace Natural History as the steward discipline of all living beings. This became possible because the new set of rules enabled mankind to study the “invisible” functions that relate organs of living beings to one another regardless of what shape or form they took. This new rule of “organism” in biological discourse formed its “discursive formation.” This same biological discourse can be found in other fields and disciplines such as medicine, which uses biological discourse as input into its own unique medical discourse. It is also reproduced in different forms in sociology within Spencer’s (1897) theories (“organic structure of society”), and in management, in Burns and Stalker’s (1961) theories that explain innovation. One or more of these discourses form academic fields and disciplines depending on the level of autonomy and coherency they demonstrate.

In the case of the IS field, although many authors agree that IS is a combination of many discourses, the set of rules that define the unity of the IS discourse is yet to be articulated (Hassan and Will 2006). This study argues that such lack of clarity contributes to the difficulty for IS researchers to generate valuable research. The next few sections employ these Foucauldian concepts to build a foundation towards understanding the characteristics of research that can be considered valuable in IS.

**Valuable Research is Original**

The most oft-quoted characteristic of valuable research is its originality. However, what is considered original is not as straightforward. 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. In Michel Foucault’s works, original research is described as having two dimensions: (1) it is subordinated to the discourse being analyzed, and (2) it is “active” in a way that creates hitherto unknown information. Each of these dimensions is explained in detail next.

Original Research is Subordinated to Its Discursive Formation

The first criterion for research to be valuable is its subordination to the discourse being analyzed. Subordination here means the research contributes to its domain or disciplinary subject matter. Research in IS that contributes instead to its parent disciplines or other fields, such as management or computer science, is less valuable than one that contributes to the IS field itself. This characteristic of research highlights a critical beginning point for what is commonly referred as “relevance.” Often, the question raised with respect to relevance is “relevant to whom?” Instead of arguing whether or not it is relevant to practitioners or academicians, the question that is more important is whether or not it is relevant to the field itself. What the IS field offers its practitioners is only part of the solution of relevance. Very few today will disagree that IT benefits more than just the community of IS managers or practitioners. As Markus (1999) notes, the stakeholders of the IS field are no longer the same stakeholders 20 or 30 years ago. Not only do IS managers benefit from IT, every facet of society from the individual iPod user to governments, are the beneficiaries of the benefits of IT. The discursive formation of the IS field has matured to the point that it is now autonomous and capable of supporting its own unique discourse even without the sponsorship of its traditional manager stakeholder. What would be more “relevant” to the field is to predicate the value of the research relative to this discourse that it is analyzing rather than to the people the discourse benefits.

Most established fields subordinate its research to its disciplinary subject matter, making clear how its objects of study contribute to that subject matter. Consequently, the domain of the research can be mapped such that it is possible to draw a definitive hierarchy from its root, its discursive formation, to its different branches and enunciations. For example, the discursive formation of biology is based on the set of rules surrounding the organic structure of life. From this base, a number of branches are formulated at its summit, using the same regularity that characterize its roots, but more delimited and localized in its manifestations. Each new branch that grows from this tree contributes to the size and usefulness of the tree. The larger its size, the more fruit and shade it provides to its stakeholders. Thus, when Darwin introduced his notion of evolution, a new branch grew from this tree representing the field of biology. It remains subordinated to its roots, but it provides an original and valuable contribution to the discursive formation of the field. Similarly in the case of IS, a disciplinary hierarchy can be drawn from the foundations of the field, manifesting as branches in the form of management information system (MIS), geographic information systems (GIS), medical information systems or any other localized manifestations of the same regularity. Thus, any original contribution to the field as a whole can be clearly assessed, not only to the foundations, but also to its specialization.

Original Research is Active and Not Passive

Even though valuable research is always subordinated to the discursive formation of the field, 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, what Darwin (1859) was talking about is similar to what Lamarck (1809/1960) was talking about 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). The 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. For example, it is easy to see the difference between the term “structure” in organizations, and the same term “structure” in biology. These two terms are identical, but because they belong to different discourses (according to our analogy, different disciplinary trees) they provide to the researcher a different
sense of what is discussed. What has changed is how one discourse applies its rules on the term, making it possible for the new term to explain something different.

Within the same discourse, it is also possible for the same terms to mean different things. In economics, Gresham’s and Locke’s formulations of relations between money and prices use the same terms as those used by Smith and Ricardo. But the latter economists apply a different set of rules of discourse on those terms, thus making their research valuable to the discipline of economics. At the same time, the use of different words also does not mean the research is original. For example, in the IS field, the different terminologies used to describe the nature of technologies employed in Internet-related businesses (e-commerce, internet commerce, online business, online collaborative technologies) may use different words, but are just shadows of each other, and demonstrate imitative rather than creative and active characteristics. The same enunciative functions are in operation in all these terms and they offer no additional value to the research efforts of the IS field. 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, only within the context of the IS field. The challenge in creating original research in IS lies in finding novel enunciative functions that manipulate the objects of study of the field in different ways from its position in the borrowed discipline.

**Valuable Research is Tied to Its Sociological and Discursive Environment**

Another aspect of value in terms of its relevance is the relevance of the research to its sociological and discursive environment. This aspect of relevance is closely related to what Gordon Davis calls “problems relating to social welfare, business, economics, education, and government.” It is easy to see how valuable Oppenheimer’s research was to the war effort, but not so obvious to see the value of Einstein’s research when it first emerged. This issue addresses another aspect of valuable research other than its originality—how much it contributes to knowledge. The problem lies in what is meant by “contribution.” An example of this process can be seen when the Apollo project required knowledge on how to put a man on the moon. When NASA went out to search for that knowledge, they had to bring together different disciplines in order to satisfy that need. The range of disciplines that contributed to the Apollo mission included engineering, physics, geology, material science, aeronautics, chemistry, biology and medicine. By virtue of their involvement in satisfying the needs of the sociological environment the credibility and prestige of these disciplines were enhanced. Each discipline needs to pay attention to how it can contribute to its sociological environment.

Knowledge did not develop solely from the needs of the sociological environment. It also developed from the evolution of modern natural sciences itself, the general “scientification” of knowledge, and the growth of the universities (Flexner 1979). In other words, valuable research is also tied to its discursive environment. This was in part why Einstein’s research became valuable even when its practical applications were not obvious at the time. The transformation of History into Natural History and later into biology provides insight into how the discursive environment makes research valuable. Before Natural History, there was nothing like it; but there was a field called history. Between the sixteenth century and the middle of the seventeenth century, the nature of discourse about living beings was all about their histories(Foucault 1970). Books were written on the History of the Nature of Birds, The Admirable History of Plants, The History of Serpents and Dragons. Given the subsistence economy of the time, these studies were relevant and valuable because they helped mankind to discover things about their surroundings. The prevailing discipline at that time that aided in this discovery happened to be the discipline of history. It was the nature of historical discourse to document and recount events that were significant or to describe the elements or parts of object being studied that were associated with those events. The study of history of plants or animals, for example, became valuable not only because certain plants could be made into medicine or food, or travelers needed to be familiar with certain predators in order to travel safely (its sociological needs), but also because certain animals provided legends and stories that could be used to inspire (its discursive needs).

Everything changed by the middle of the seventeenth century around the time Jonston wrote a *Natural History of Quadrupeds* (Foucault 1970; Foucault 1972). It wasn’t because Jonston had more things to write compared to earlier authors. During the sixteenth century, authors of history described animal names, their anatomical parts, habitat, movements or medicinal uses; Jonston also wrote about the same topics. The difference lies in Jonston being able to represent a concept that generalized what *naturally* occurred with the animals, something that was not immediately noticed by earlier authors of history—the fact that certain animals with four legs had certain characteristics different from those with two legs—become inexplicably valuable. Jonston’s contribution opened up a new kind of discourse, one that is stripped of all myth and fable and focused on exacting observations on *naturally occurring* characteristics of the same animals. These characteristics allowed observers to extend their knowledge to other living beings. History suddenly became *natural*, hence *Natural History*. Another reason why this knowledge became valuable was because it related well with the advances in the mathematical sciences (another discursive element). Descarte’s (1596-1650) mechanical interpretations of nature which placed emphasis on the quantitative (shape and size) as opposed to the qualitative (sounds, tastes and smell) values supported
Natural History’s efforts in systematically simplifying the complexity of nature using its form, magnitude, and the number and position of its different parts. The transformation of that knowledge, as a result of the discursive environment, did not stop there.

In the eighteenth century, the invention of the microscope made possible the study of not only what was visible, but also what was invisible to the naked eye. Again, the value of the prevailing discipline related to living beings was not necessarily put to question, but it lost its leading role in describing living beings to a different discourse. This occurred because mankind became curious about not only what they could see, but also about what they could not see operating under the skin and taking place between different organs of the body. Mankind became interested in life, “the ensemble of functions which resists death” (Bichat 1800, p. 2), instead of just the study of living beings, as was the subject matter of Natural History. This new subject matter became prominent as a result of Cuvier’s (1800-1805) work in emphasizing function over the structure of organs, and made possible new concepts of life such as respiration, nutrition and reproduction, all of which did not exist in knowledge before his time. All of these contributions gave birth to biology—the science of life. By virtue of this new discipline, mankind gained access to hitherto unknown secrets of life. These transformations from history to Natural History to biology demonstrate how both the sociological environment and the discursive environments make certain research valuable.

The field of IS needs to be sensitive to both its sociological environment and its discursive environment. It needs to first seriously address societal needs because IT is no longer the purview of managers or business organizations alone. IT is infused into all levels of society and its impact is pervasive if not intrusive. If IT can become instrumental in causing Trent Lott, the majority Senate Leader, to resign (Krugman 2002), IS research capable of explaining and describing more serious socio-political upheavals will be valuable to society. At the same time, the field of IS needs to be sensitive to its discursive environment. For example, in part because of the lack of progress in the field in servicing the medical field, the new medical informatics field has already progressed in leaps and bounds so much so that it now boasts its own scholars, conferences, journals and accreditation bodies, despite the obvious similar discourse that each of these two fields share. If the IS field does not measure up to the needs of both these environments, other disciplines will follow the lead of medical informatics in forging their own IS fields.

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 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 the 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. These relations are not obvious relations or “primary” everyday relations, nor are they “reflexive or secondary relations” that discourse establish in order to even speak of the object. They are instead “discursive relations” that make possible unlimited discourse on the object of study.

For example, in every firm, it is obvious that the CEO manages or directs the heads of departments and these heads of departments instructs supervisors who in turn manages a group of employees. It is also obvious that the attitudes or problems that superior might have will likely affect the subordinate under the superior’s 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 the superior and the subordinates, the tripartite relations are “secondary relations,” but still are not necessarily those that are capable of producing the valuable object of discourse. The value of the research is manifested when a space unfolds within the relations that offer discourse itself the objects of which it can speak, that determine the group of relations in order to speak of this or that object, in order to classify or analyze them. Thus, in the case of the relations between the superior 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 an enduring object that emerges every time the same kind of relations are established. One of these valuable concepts is what management theorists call “the span of control,” an object of study so valuable, it 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 what Foucault (1972) calls the
“discursive relation” that emerges between institutions, techniques, social forms and other elements in the society that has become valuable.

This kinds of objects are what the IS field needs to discover and to lay bare to its stakeholders. 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 no longer a discursive relation.

Valuable Research Adheres to the Laws of Rarity

Valuable research is research that is capable of saying more than what the words in its statements literally say. Valuable research contains rare statements that embrace a plurality of meanings and describe unlimited potential. For example, when one says, “The road to Rome is paved with gold,” the words not only mean more than what they say, they open up a space for discourse. What kind of potential exists? What kind of effort is required? How far do we need to go on this journey? These kinds of statement contain what Foucault (1972) calls the “element of rarity” (p. 118). The statements that qualify as rare have two paradoxical characteristics. It is limited by its disciplinary subject matter but at the same contains unlimited potential. This is the paradox of disciplinary knowledge. Kant stated that the very conditions that limit us as humans are the very conditions of the possibility of knowledge (Gutting 2003). Foucault (1972) describes this Kantian philosophy as the “analytic of finitude,” the theory that limitations placed on human activity are the cause of its progress; it is the limits imposed on human academic endeavors that produce the genius from which the expanse of knowledge becomes possible. It is indeed rare to find such statements and these are the kinds of statements that the IS field should seek out. It is these statements that Foucault (1972) notes become “an inexhaustible treasure from which one can always draw new and always unpredictable riches” (p. 120). Foucault describes how these statements emerge in what he calls the “laws of rarity.” These laws do not address the content of the statement or their truth value. It is quite possible for the statements to be false, but can nevertheless be rare. In the next subsections, the nature of these laws are explained and demonstrated using the examples of Michael Porter’s statements on competitive strategy and Herbert Simon’s statements on information processing.

Because Everything is Never Completely Said, a Rare Statement Addresses What is Unsaid

This law states that statements are always incapable of encompassing the complexities offered by the research, so what is sought is indicated by this inability for statements to adequately circumscribe its object of study. What statements can totally encompass is no longer rare, because rarity comes from the vacuum created by the attempts of statements to understand them. Researchers need to look for the language capable of filling up these vacuums in the field. For example, in the case of management, although it is clear what management is all about—accomplishing work through other people—it was not clear what strategy was, or what the goals of strategy at the firm level were. Should companies service their stakeholders or should they service society? It was also not clear how firm strategy affected economic performance. Michael Porter (1980) focused on this vacuous space and because his statements filled this vacuum, they became valuable.

A Rare Statement is Capable of Excluding Other Statements

Rare statements are statements that are capable of excluding other statements. This is possible because the rare statements act on a limited system of statements, and are capable of identifying gaps, voids, limits and divisions in those statements. Because these rare statements are part of that same system, when they emerge, they separate or move the other elements of the system, by highlighting or pushing them into obscurity. This is how such rare statements become valuable. Before Michael Porter, management’s performance was assessed at the company level of analysis (Barney 2002). Each company did its best to outperform its competitor and measured its performance on sales and profits. Michael Porter recognized the gaps and limitations of that analysis and introduced the industry level of analysis into the field of management. This analysis is now the predominant unit of analysis in any economic-related discipline, replacing the traditional company level of analysis.
A Rare Statement Has a Unique Position

A rare statement always occupies a space with other statements in a unique way. The discursive formation of the field maps the statement in its own local field, and isolates it from the general dispersion of other statements. No other statement can claim the position that it holds unless these other statement are able to dislodge that rare statement from its position. The reason why many fields, including the IS field, would like to claim that Herbert Simon’s statements are their own, even though he was primarily a political scientist (his bachelors and PhD were in political science) is because his statements on management decision making and information processing locate themselves in a unique position within their fields. Because the field of IS is concerned with providing information for effective decision making, Simon’s theories became the natural choice for IS frameworks. For example, Simon’s decision making concept occupies a unique position in Gorry and Scott Morton’s (1971) structured and unstructured decision making framework of IS. It is rarely found anywhere else. In contrast, concepts and terms in IS research such as “collaboration,” “inter-organizational,” “methodology” and “knowledge” are placed somewhat indiscriminately and repeated in many topics, not demonstrating its uniqueness in any one.

Rare Statements are Always Appropriated and Duplicated

Rare statements are always transmitted and preserved and become the object of attention. A major sign that a statement is rare is the degree in which it is appropriated and duplicated into different forms, especially if the appropriation is performed by the parent discipline themselves. For example, it is well known that the IS field acquires much of its content from management and psychology. Evidence that statements in the IS field are valued is demonstrated if the management field appropriates IS statements not only in passing, but for their own serious conceptualizations. This can only happen when the content that the IS field has appropriated from management or psychology are transformed by interpretation, commentary and proliferated into different forms of meaning. The pre-existing networks within the appropriating fields will adjust to accommodate these statements as they are given priority status within the accepting field. One of the best examples of this appropriation and duplication is the work of Herbert Simon on information processing. The idea that the human mind processes information in the same way that computers process information was suggested by Douglas Broadbent in the 1950s when digital computers became popular. With the help of Herbert Simon this rare statement was transformed into unlimited discourse and into more rare statements in the fields of cognitive psychology, management, economics and computer science. Simon’s interest in administrative decision making coupled with the computer simulation of human cognition made possible artificial intelligence and a whole series of economic theories. This demonstrates how rare statements from one field are duplicated into different forms are appropriated by other fields.

Conclusion

This essay proposes a novel framework for making IS research more valuable. IS researchers need to pay attention to the content and nature of their research because the relevance of IS research is not only tied to the needs of its practitioners, it is also subordinated to its essence, its disciplinary subject matter, which distinguishes the IS field from other fields. When IS research actively manipulates and creates is own objects and concepts instead of passively testing or corroborating what belongs to other fields; when IS research seriously considers the needs of its surrounding environment and is sensitive to the demands of changes in the academic and discursive environment; when it uncovers hidden insights, lays bare the underlying problems and opportunities, gives voice to the relationships that are silent, discovers the order within the chaos; when IS research focuses on what others consider unfathomable, exerts its influence over existing knowledge, finds its unique position in the grand scheme of things, and becomes capable of transforming itself into different forms and applications—that is when IS research will be sought after, valued and relevant.

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