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## Setting Our Research Agendas: Institutional Ecology, Informing Sciences, or Management Fashion Theory?

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# Communications of the Association for Information Systems

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## Setting Our Research Agendas: Institutional Ecology, Informing Sciences, or Management Fashion Theory?

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### Abstract:

A new reflexive discourse is emerging in the IS research community concerning how we, as academic scholars in the Information Systems field, set and pursue our research agendas. How should we choose our research topics, how should we conduct our research, and how should we communicate our research results? This article is based on a panel discussion at the 2010 International Conference on Information Systems (ICIS) held in Saint Louis, Missouri, USA. Three distinct perspectives concerning the setting of our research agendas in Information Systems were presented and debated.

**Keywords:** IS discipline, research agendas, institutional ecology, informing sciences, management fashion theory, IS practice

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## I. INTRODUCTION

A new reflexive discourse is emerging in the IS research community concerning how we, as academic scholars in the Information Systems field, set and pursue our research agendas [Ramiller et al., 2008]. This discourse is distinct from the more well-established discourses on relevance (or irrelevance) and subject-matter coherence (e.g., concerning the “IT artifact”). How should we choose our research topics, how should we conduct our research, and how should we communicate our research results? These are obviously important questions for all IS scholars.

However, there is considerable disagreement and debate about how we should set our research agendas, as can be seen by the debate in *MIS Quarterly* [Baskerville and Myers, 2009; Gill and Bhattacharjee, 2009a, 2009b; Myers and Baskerville, 2009]. The recommendations for action vary, depending on the perspective taken. The purpose of the panel, therefore, was to present and debate the merits of three distinct perspectives on the setting of our research agendas in Information Systems.

The first perspective looks at the shaping of research agendas from the viewpoint of management fashion theory [Abrahamson, 1996; Abrahamson and Fairchild, 1999]. Management fashion theory, and, in particular, the Milan theory proposed by Baskerville and Myers [2009] suggests that IS academics should actively engage with IS practitioners in the IS fashion setting process. The key recommendation is that IS researchers should be among the leaders, and not just the followers, of fashion.

The second perspective looks at the shaping of research agendas from the lens of informing sciences [Gill and Bhattacharjee, 2009a, Gill and Bhattacharjee, 2009b]. An informing sciences perspective suggests that the extent to which IS research is informing its external clients has declined over the years. It also suggests, contrary to the viewpoint of management fashion theory, that IS fashions can be better viewed as an informing process driven by the diffusion of innovations perspective. The key recommendation is that our current publication-oriented strategies need to change so that practice-informing activities are encouraged. It is through direct engagement with practice, not through publication, that IS researchers will influence practice.

The third perspective looks at the shaping of research agendas from the viewpoint of institutional ecology [Ramiller et al., 2008]. Institutional ecology suggests that the field’s research directions constitute responses to institutionally constituted market forces that arise both within academia and in the larger economy and society. The key recommendation is that IS research should set agendas that help to foster research that better serves both theory and practice, while being less subject to the whims of industry fashion. This recommendation seems to be at odds with both management fashion theory and an informing sciences perspective.

The purpose of this article, therefore, is to briefly present the three perspectives and to reflect on the merits of each one. This article is based on a panel discussion at the 2010 International Conference on Information Systems (ICIS) held in Saint Louis, Missouri, USA.

## II. ORGANIZATION OF THE PANEL

The panel was introduced and moderated by Michael Myers. After he outlined the motivation for the panel, there were short rounds of presentations by the three panelists: Richard Baskerville, Grandon Gill, and Neil Ramiller. The presentations and debate were structured as follows. First, each presenter summarized his theoretical perspective. Second, each presenter put forward his recommendations for setting our research agendas. Third, each presenter had an opportunity to comment on the other two presentations. Following these presentations, Michael Myers (panel chair) briefly summarized the discussion and suggested key points for debate. This article incorporates comments and suggestions that emerged in the discussions with the audience and closes with a brief synthesis of our views on this topic.

## III. SETTING OUR RESEARCH AGENDAS—THREE PERSPECTIVES

### Richard Baskerville

Many Information Systems research agendas examine practice descriptively and provide explanation and understanding of the way things are, but are not seen as useful in forming new strategies for practice. Indeed, there is very limited evidence that the major findings and theories in the academic arena of Information Systems are

having any immediate impact on practice. The common explanations for this lack of impact blame the poor selection by researchers of research topics, or the esoteric, complex writing style of their research reports.

A great deal of research in Information Systems operates with the same paradigm as much of the research in management. This paradigm models itself on the natural sciences, a shared trait with other sociological disciplines such as psychology. There is nothing invalid or unambitious in such a model. Laboratory and field experiments (including field surveys) provide an excellent reflection of their natural science counterparts. Like these natural science counterparts, the knowledge delivered from such science generally regards the way things are, or have historically been. For the natural sciences, such knowledge is useful because the natural laws discovered in today's phenomena may reasonably be expected to hold over future periods of time. Such results imply predictability.

Consider, for example, the enormous amount of work dedicated to the study of technology adoption in information systems. We have strong theory and deep evidence to explain why people do, or do not, use particular information technologies. Technology adoption is a fundamental human act. Although the research is social rather than natural science, it is hard to understand why such "social laws" (albeit probabilistic) would not be immensely useful for managing future technology adoption settings. This seems like good, solid, basic research, with results that hold over future periods of time. Why should these results be less useful than those of the natural sciences?

The impact on practice of such scientific Information Systems discoveries has been disappointing. The common explanations mentioned above would suggest that an understanding of technology adoption is either useless in practice or too poorly explained and too poorly disseminated to be useful in practice. In this way, the blame for the lack of practicality in the topic, or the transfer of the knowledge, is laid at the feet of the researcher. These explanations demand that researchers, laden with the burden for this problem, should choose more practical research agendas and better explain to their audiences how practitioners should apply their results.

Such strategies contradict much of what we know about technology transfer. We know for example, that "technology push" strategies alone are unlikely to succeed. We know that more interactive models combining "technology push" with "technology pull" are more likely to succeed. We also know that shared knowledge is important in transferring innovations across the boundary [Baskerville, 2001; Kline, 1985]. Similarly, strategies for knowledge-push without knowledge-pull seem doomed. It seems like a failed strategy to have researchers compensate for the lack of demand by more diligently seeking to push their unwanted research results across the boundaries to practice.

How, then, should we develop the demand? One short-term strategy might suggest improving the ability of researchers to sense and respond to needs and demands from practice for research. This might be viewed as deferring to practice in setting the research agendas, perhaps by soliciting research proposals based on practical needs. One issue with this strategy is the incredibly near-term problem focus of practice. Research takes time, and long delays in problem solution are rarely amenable to the immediate needs of practice. Similarly, near-term, less theoretical research results can be difficult to publish in leading journals. This near-term focus is not a good strategy for researchers facing publication expectations.

It might be more promising to establish such agenda setting as a form of collaboration between researchers and practitioners. This is a fundamental element of engaged scholarship [Van de Ven, 2007] and might be best exemplified by action research where known theories are tested against real problems and immediately researched when shown to be invalid. However, such collaboration is complicated by the distinctly different kinds of knowledge held by researchers and practitioners, different priorities, and unnatural problems in the "knowledge food chain" [McKelvey, 2006]. The notion here is that the usual flow of knowledge is from Researchers (as teachers) who train Students who become Consultants who inform Practitioners. Because theory and practice involve distinctly different kinds of knowledge, collaboration pushes knowledge the wrong way in the chain. Such collaboration amounts to expecting the researchers to fundamentally become practitioners and vice-versa.

If we seek a realistic means of bridging the gap between research and practice, it might be more promising to consider if practice is faulty, rather than assuming the research is faulty. Instead of assuming that the issue is a research issue, or even a noncooperation issue, perhaps it is simply a practice issue. The question becomes, "How can researchers improve the ability of practice to usefully deploy research results in their work?"

Consider, for example, the Information Systems professional discipline. Like other management disciplines, its practice is punctuated with fads and fashions. These waves of interest in new ideas about Information Systems usually rise and fall, even though some topics are more enduring than others. These have included such fashionable directions as office automation, reengineering, computer-aided systems engineering, and electronic commerce [Baskerville and Myers, 2009]. This tendency toward fashion is shared with management disciplines and despite

being generally deplored remains common practice [Abrahamson, 1991]. Fads that are followed senselessly can be costly and damaging to organizations.

Such fashion cycles provide an opportunity for researchers to participate in practice, but in ways that do not necessarily upset knowledge chains or reset research agendas. Actually, models of managerial fashion-setting incorporate business schools among the gurus, consultants, and mass media that typically launch new management fashions [Abrahamson and Fairchild, 1999]. It is an opportunity for researchers to share (perhaps in public fora) the meaning of prior research results that relate to new fashions. Rather than discouraging researchers from participating in such fads, it could help break down the research-practice gap to advance such participation. This might require a bit of courage on the part of the researchers, but it could improve practice by helping everyone understand the serious and known implications of new fads. Indeed, it could be the case that Information Systems is already more of a participant than many management disciplines; there is evidence that fashions cross the Information Systems researcher–practitioner boundary anyway [Baskerville and Myers, 2009], with research sometimes leading, but usually following practical interest.

The idea behind such fashion-setting leadership from within the research community is one of improving practice. Such an improvement is not made by resetting research agendas or starting new research projects, nor does it involve researchers undertaking roles as practitioners. Rather, improvements are made simply by sharing existing research knowledge that relates to a fashion. This would involve a change of behavior in researchers, in the sense that it becomes more important to track emerging new fashions and proactively inform the practitioner public about the meaning of the fashions in relation to past research results.

Such measures as engaged research and fashion-setting participation are only short-term measures. In the long term, it may be that Information Systems practice is defective in its inability to more rapidly translate research results into practice. The flow of knowledge along the food-chain mentioned earlier may take decades. For such knowledge flow to move faster requires practitioners who can read research results, understand them, and apply them in practical settings. This requires certain intellectual skills.

It is unlikely that such a highly skilled corps of information systems practitioners could be built quickly. This is a generational challenge, starting with the recruitment of capable students, and rearranging course curricula to include original research readings that are relevant to the topic. Students would develop skills that would enable them to access and apply research relevant to their practical needs. Such advanced expectations from Information Systems students is already prevalent in some programs [Baskerville, 2009].

Another advantage of such long-term strategies is that these would lead to a substantial advancement in the professions of Information Systems. Practice would evolve into the hands of more advanced professionals, with newer intellectual skills to deal with the increasingly complex problems of information technology use. Similar notions have been advanced in management as a call for “evidence-based management” where decisions are informed by all available evidence (including research results) [Pfeffer and Sutton, 2006]. These principles are taken from evidence-based medicine, which is perhaps a professional ideal for the role of clinical application of knowledge generated in research.

It is important to recognize that the issues of the inability of practice to apply research results fall back into the laps of many researchers, but in a different form. While the recognition of needs to improve practice do not necessarily require changing the research agenda or the rigor in research reports, there are other implications. Many researchers also teach in university programs. Preparing a new generation of practitioners requires more attention to a higher level of learning. It will require campaigning for more capable entering students. It will require changes in course designs. It will also require more careful participation in professional meetings of practitioners in order to relate research and fashions. For such university faculty members, the observations above suggest that the problem shifts from the researcher role to the teacher and service roles.

However, the observations above no longer pinpoint research as the issue. Information Systems researchers are no longer enjoined to choose topics from among the short-term problems of today’s practitioners. More basic, fundamental, generalizable (and publishable) topics, along with solid social scientific research methods remain sensible. The issue, which concerns researchers, teachers, students, and practitioners, is improving practice to enable better application of the available, solid research results.

### **Grandon Gill**

Informing science is a transdiscipline that synthesizes research related to informing processes from a variety of disciplines, including MIS, computer science, library science, education, philosophy, and liberal arts. What we mean



by “informing processes” is communications that produce intended changes in the mental models of a recipient, generally referred to as the *client*.

The organizing framework for informing science is the *informing system*. Informing systems, in turn, are constructed of elements that collectively support the informing process. In its simplest form, such a system would consist of an *informer* (sender), a *message* or collection of messages to be conveyed, a *channel* through which messages are distributed, and a *client*, the intended recipient of the messages. As illustrated in Figure 1, informing science is particularly focused on understanding the fit among these components. In informing systems where such fit is poor, the ability to inform is severely degraded.

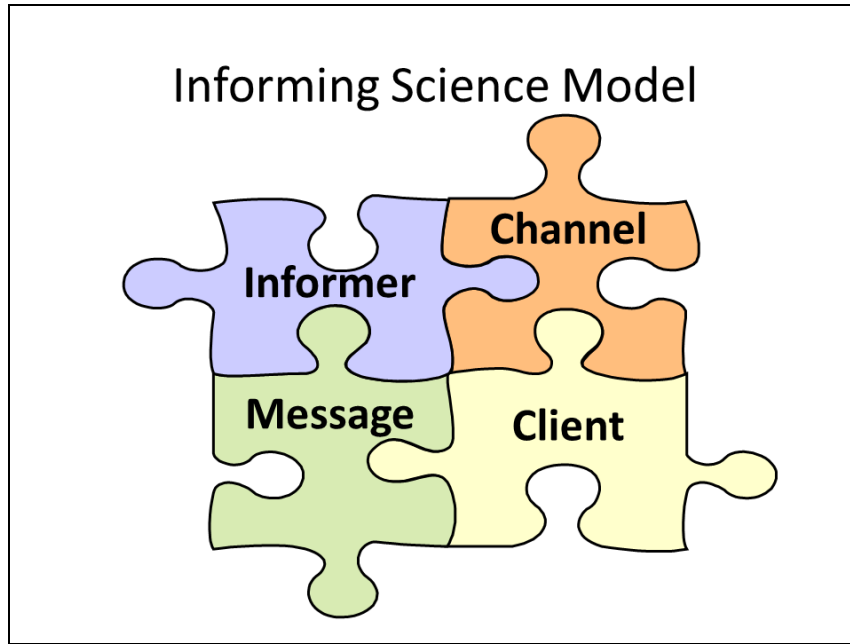


Figure 1. Elements of a Simple Informing System

### Theory

In considering the MIS research agenda, the application of informing science theory involves examining each of the system components. In this section, we focus on the message, client, and channel pieces of the system. Because the “informer” in this analysis is us—MIS researchers—the consideration of that component will be left to the recommendations section, where the question of how we might achieve better fit is addressed.

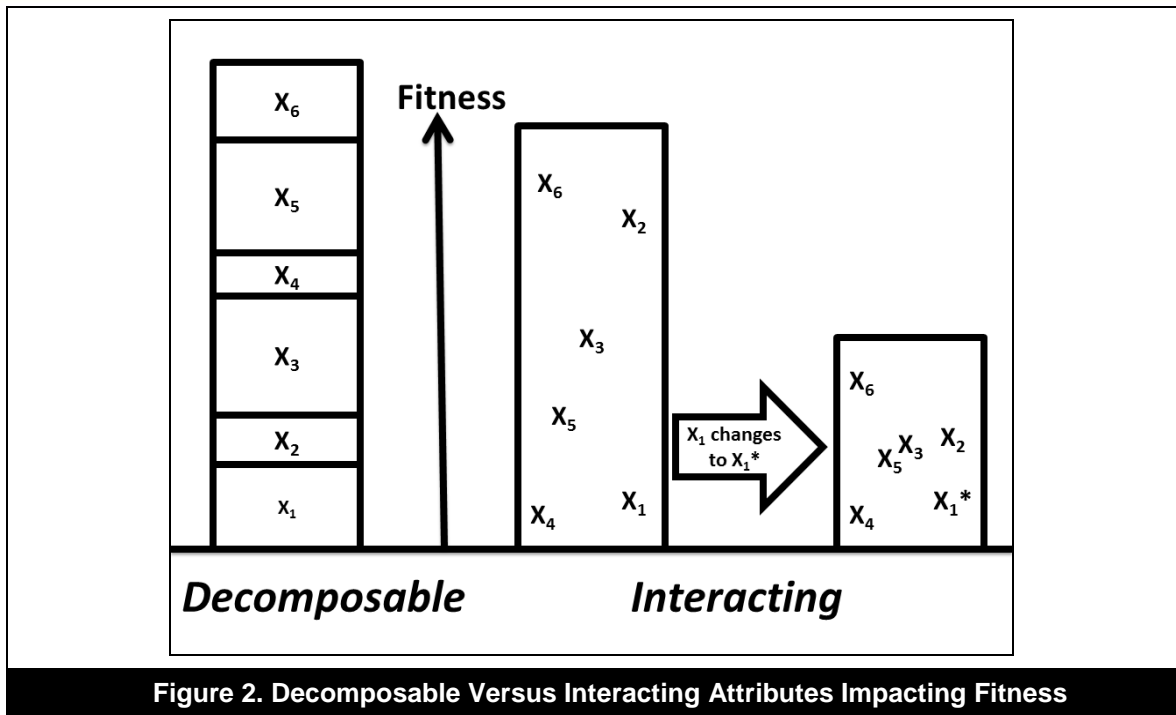
### The Message: Research on a Rugged Landscape

With the exception of certain research subdomains—most notably design science [Hevner et al., 2004]—MIS research is dominated by the goal of developing theory or theories that provide an understanding of the underlying truths that characterize the environments and processes that we study. These environments overlap considerably with the informing systems studied by informing science and, even where an Information System cannot properly be described as an informing system, it still has much in common: to function effectively it requires fit among a variety of components that include individuals (e.g., users, operators), technology artifacts, organizations, broader environments, and specific goals, just to name a few.

In informing science, when we encounter such systems we may apply a model developed in evolutionary biology [e.g. Kauffman, 1993], that of a fitness landscape. A fitness landscape is a mapping between a set of attributes (i.e., independent variables) and some desired outcome (i.e., dependent variable), referred to as *fitness*. Of particular interest to us is the fitness landscape characteristic referred to as ruggedness (closely related to complexity).

Such ruggedness is most often a consequence of interactions between attributes. As illustrated in Figure 2, attributes may influence fitness in a manner that is decomposable—each attribute exerts an influence independent of other attributes—or interacting, meaning that the influence of one variable cannot be determined independent of the values of other variables. The greater the role played by interactions, the greater the ruggedness.





**Figure 2. Decomposable Versus Interacting Attributes Impacting Fitness**

The ruggedness of a landscape being studied has a major impact on the form of the theory necessary to describe the landscape. Consider a landscape where fitness is determined by 20 binary (0 or 1) variables, the type of landscape commonly explored by Kauffman [1993]. Where the landscape is fully decomposable, a theory-of-fitness can be described completely by a simple equation of the form:

$$\text{Fitness} = c_0 + c_1x_1 + c_2x_2 + \dots + c_{20}x_{20}$$

Thus, our theory can be conveyed compactly with just twenty-one values ( $c_0$  through  $c_{20}$ ). Although we may be loath to admit it, much of our theory-testing in MIS relies on tools that implicitly assume such decomposability, such as multiple regression analysis and structural equation modeling.

At the other extreme, assume that all twenty variables interact. In that case, the fitness function cannot be decomposed from its most general form:

$$\text{Fitness} = f(x_1, x_2, \dots, x_{20})$$

The consequence is that our theory will now require 220 different values be established—one for each possible combination of  $x_1, x_2, \dots, x_{20}$ . Such a “theory” would be impossibly large to convey.

The other major implication of ruggedness is the existence of many peaks. Using Kauffman’s [1993] model, for example, a maximally interacting landscape—referred to as a *chaotic landscape*—with twenty attributes will have around 50,000 local peaks. Moreover, entities on that landscape will tend to migrate toward these peaks in adapting to increase their fitness. A good easy-to-visualize example of such an outcome is a cookbook. Conceptually, such a book represents an attempt to partially describe the mapping from ingredients/cooking techniques to completed outcomes. Each recipe within the book describes a local fitness peak—at least in the eyes of its creator. After all, what author would include a recipe knowing that it could be improved by one or more simple incremental modifications?

Why we might expect MIS research should mainly revolve around rugged fitness landscapes is explored elsewhere [e.g., Gill, 2010]. In brief, Kauffman [1993] offers a theoretical rationale by proposing that adapting systems naturally gravitate toward a rugged boundary between order and chaos. Ordered systems—where all entities gravitate toward a single peak—are too brittle in the face of environmental change. Chaotic landscapes, on the other hand, have too many peaks with low fitness. From a pragmatic perspective, the MIS discipline’s desire to study “relevant” topics nudges us toward studying variables whose outcome is desirable, such as system use, quality, and security; in other words, variables that correspond to fitness. In addition, there are a variety of markers that suggest the presence of a rugged landscape. These include:

- Evidence that entities can achieve success (i.e., fitness) in a variety of different ways; this is consistent with the presence of diverse local peaks.
- Observations of situations where incremental changes to individual attributes appear to exert an unexpectedly large impact on fitness; this is typical of interacting variables in a rugged environment.
- Exceptions are observed to nearly every general theory or proposition; the rules governing behavior on different local peaks can vary dramatically.
- Resistance to change is often encountered; entities on peaks will naturally be suspicious of incremental recommendations that would seem likely to move them to lower fitness.
- Research frequently fails to replicate; observations selected from one set of peaks can produce dramatically different results from those selected from a different set of peaks.
- Punctuated equilibrium dynamics often exist; this is a frequently observed characteristic of complex adaptive systems.

From an informing standpoint, what this model asserts is that complex environments will produce a general theory that is likely too large to be communicated and the behavior at or around peaks is likely to be of greater interest to clients than general descriptions of the landscape.

#### The Client: Expert Practitioners

As MIS faculty members, we have many possible clients—our students being those that we most routinely inform. When it comes to our “cutting-edge” research, however, our *ultimate* client needs to be the practitioner expert. To be sure, we need to inform each other (i.e., other MIS researchers), but if there is no long-term goal of informing practice, then our research could best be described as playing the same role as friction in a mechanical system: it consumes resources without giving anything back to the owners of the system. Sadly, when costs become a serious issue, high-friction components tend to be at the top of the list for elimination or replacement.

In considering the practitioner client, it is very important to take into account that the client is also an expert. In a practical sense, that means: (a) the client has already attempted to navigate to the top of a local fitness peak, and (b) the client has a pre-existing mental model of the environment in which he or she is operating. Both of these factors have important consequences for informing.

Consider, for example, the challenge facing the researcher intent upon informing the expert client with a simple proposition, such as a finding resulting from a successful hypothesis test. Based on the expert’s past experience and current mental models, the proposition confronts a continuum of pre-existing knowledge, ranging from “already known to be true” to “already known to be false.” In between the two extremes, we have a range where the expert is uncertain or has no opinion.

The practical challenge to informing presented by pre-existing knowledge is illustrated in Figure 3. Where the proposition is already believed to be true, there is no informing, since the client’s mental model remains unchanged. Where the proposition is already believed to be false, the medium we prefer to employ for our informing activities—publications, whether they be academic or trade—is insufficiently resonant to overcome such pre-existing knowledge, which is often supported by the expert’s experience on his or her local peak. It is only in the area where uncertainty is high that the potential to inform is likely to be high. For propositions that are material in their impact, however, prior knowledge hovering in the uncertainty range will likely be far less common for expert clients than for novices or students. For this reason, we expect it will be very hard to inform experts with knowledge framed in the form of general propositions. Actually, that should not be too surprising. General propositions rarely capture the full truth of a rugged environment.

Viewed in broader terms, the combination of a rugged landscape and pre-existing expert knowledge drives a serious divergence between what is intended to be true (i.e., theory) and what is intended to be useful (i.e., the conceptual schemes used by experts). This divergence is illustrated in Figure 4 by the complexity arrows pulling apart the truth and useful ellipses within the general space of all models. What is generally true is too large and convoluted to be communicated to practice; it is, therefore, not useful. What is useful is too local and heuristic-based to be generally true.



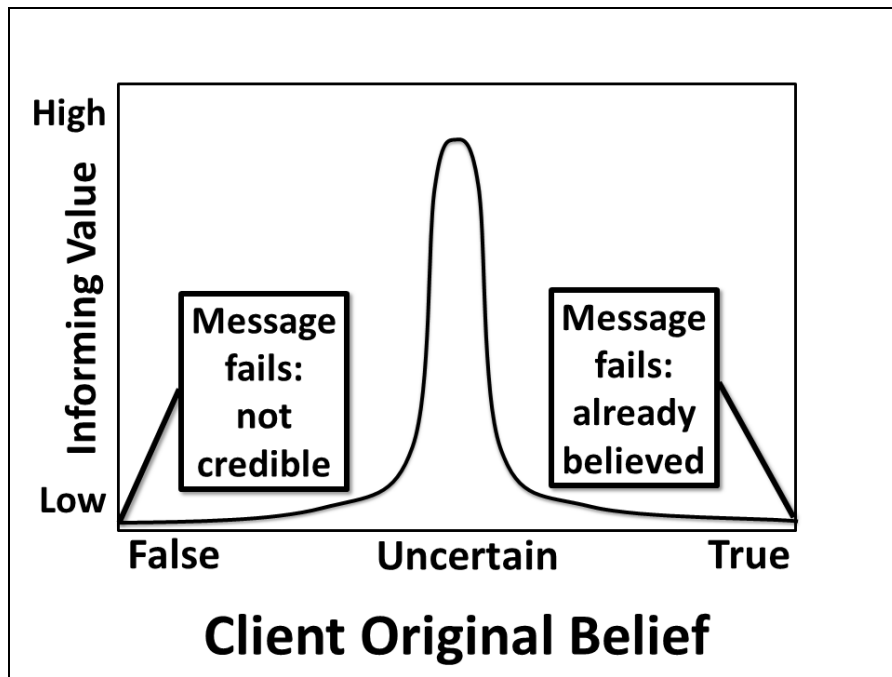


Figure 3. Informing Potential for Propositions Based upon Expert's Prior Knowledge

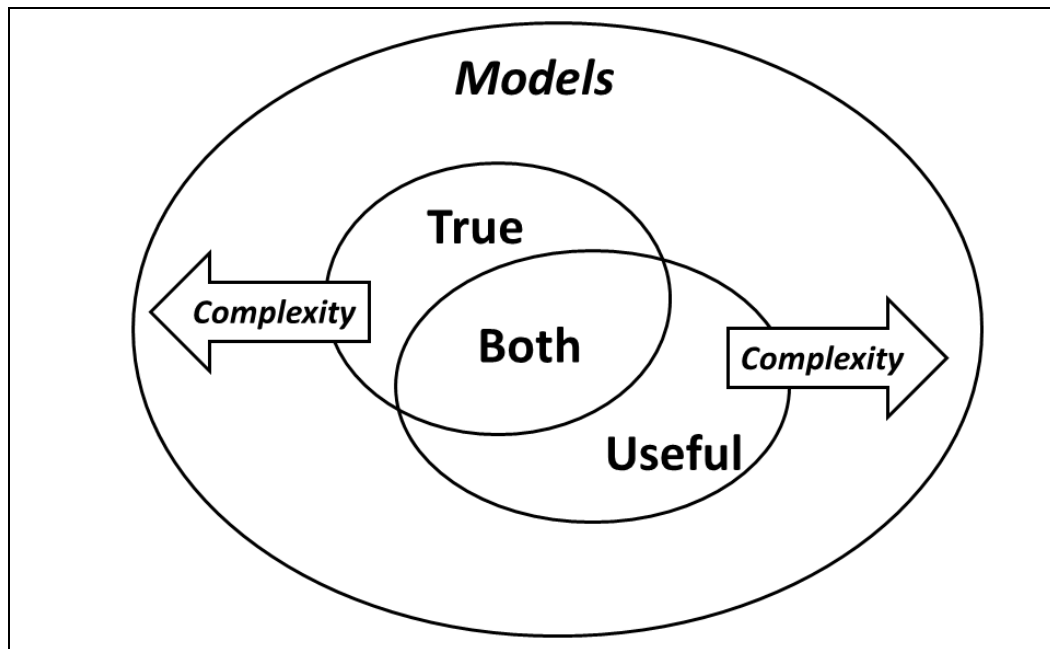


Figure 4. Divergence Between True and Useful Models as Complexity Grows

This paradox is at its worst when we focus our research on understanding the routine activities of MIS practice. It is these activities where the expert is most likely to be confident in the validity of his or her local peak. Where the environment has just experienced a significant discontinuity—such as the black swans and grey swans described by Taleb [2007]—then the likelihood of experiencing expert uncertainty is likely to be higher.

#### The Channel: Diffusion Is Integral to Applied Research

The final element of the research informing system is the channels being employed to convey our research message. Our channel of choice is, of course, the refereed publication. The diffusion literature [e.g., Rogers, 2003], however, suggests that such one-way, mass-media channels are likely to be almost entirely ineffective in conveying

complex ideas to a population with its own pre-existing mental models. In informing science, the two dimensional diagram presented as Figure 5 is sometimes [e.g. Gill, 2010] used to convey the context of channel(s) selection. The vertical dimension indicates the degree of structure present in the message to be conveyed. This ranges from highly structured (e.g., transferring a database record) to low structure (e.g., conveying an ambiguous feeling). As complexity and ruggedness grows, structure declines. The horizontal dimension reflects the informer's knowledge of pre-existing client mental models. The range goes from complete knowledge (e.g., we send a purchase order to an automated server) to very little knowledge (e.g., we are offering our opinion to a group of strangers from another country). As we move from the routine informing to the low structure informing regions, the appropriate channels change. At one extreme, routine informing messages can be broadcast with little or no danger of misinforming. At the other extreme, low structure informing typically requires high bandwidth bi-directional channels capable of conveying rich information of many types; a necessity for the client attempting to triangulate meaning.

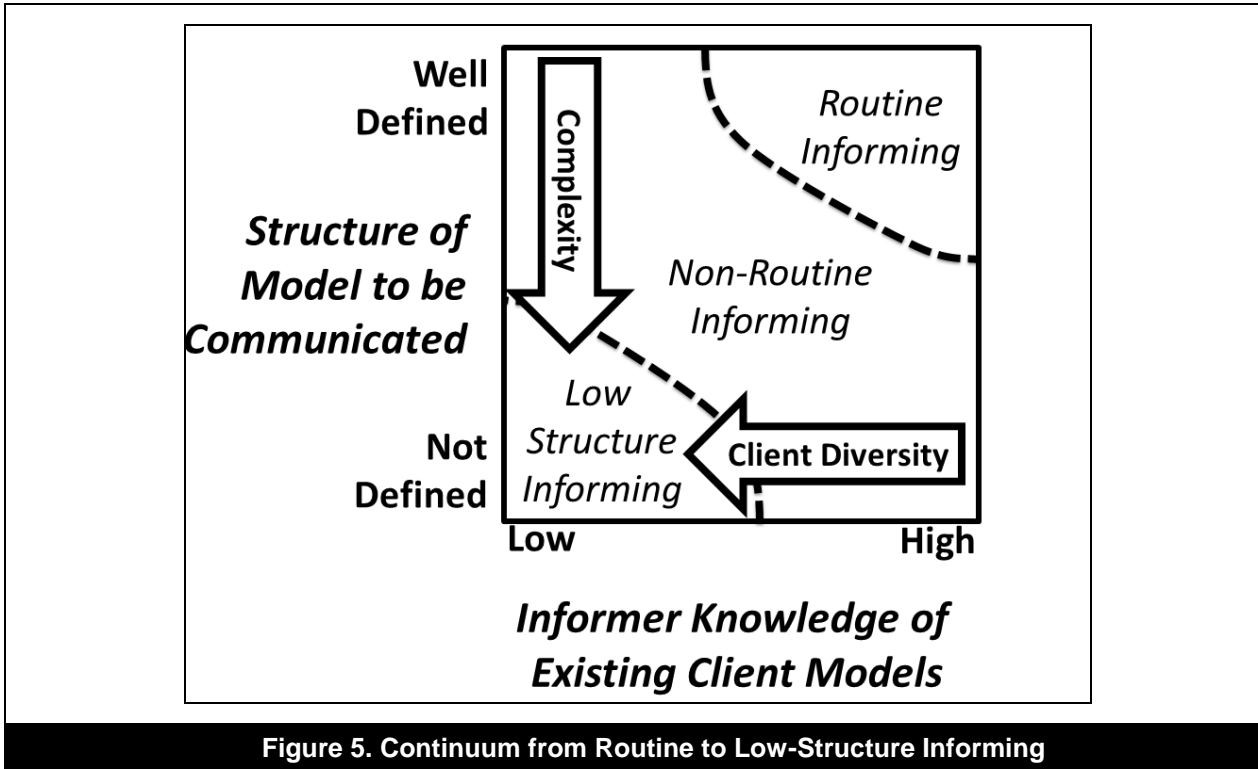


Figure 5. Continuum from Routine to Low-Structure Informing

Interestingly, we intuitively understand these issues in our own intra-disciplinary activities. Doctoral student informing involves vastly more interactive face-time with researchers than is offered to other categories of students. Conferences, where we interact with other faculty members to build interest and correct misunderstandings, play a central role in the ecology of our disciplinary informing system. Even the peer review process serves to ensure that authors and readers achieve some degree of common understanding. Where we have failed to learn these lessons is in our activities to inform practice.

**Recommendations**

Throughout this analysis, the MIS researcher has been cast in the role of “informer.” The particular challenge that we face is that if better fit is to be achieved among the components of the research informing system, it is we who will need to initiate the change. Furthermore, there are limits to what we can do. The environments we study are the cards that we have been dealt. If they are rugged, as has been asserted here, then we had better accept that reality and plan our research accordingly. We might be able to exert some little impact on the MIS practitioner client through our educational activities, but that is a very long-term prospect. Thus, it is the *channels that we employ* and the *nature of the research that we choose to conduct* where we are most likely to be able to initiate change. Five recommendations relating to these areas are now presented.

*1. Target the peak (uncertain) portion of the Figure 3 inverted-U*

Regardless of how we choose to inform practice, it will be easier if our areas of research correspond to the questions where practice has not previously formed strong opinions. This would represent a fundamental change in research philosophy. Today, in our search for generalizable truths, we tend to seek areas of the *research literature* where existing studies are absent or not convincing. What is proposed here is that we should be directing our efforts



toward the areas where practice is fuzzy, not areas where we need to flesh out the literature for the sake of completeness.

Under such a philosophy, empirical research might begin with focus groups or surveys of practice where research questions are floated as propositions and practitioners are asked to agree or disagree with them. Where the majority reaction is *either* strong agreement or strong disagreement (or both!), then the likelihood the research will impact practice through existing channels—whatever the outcome of the research—is negligible.

## 2. *Respect popular schemes*

It is not uncommon to find academic researchers expressing a certain disdain for consultants and their models. After all, consultants are not subject to peer review, most lack the training in formal rigor provided by doctoral education and, perhaps most distressing, they are in it for the money.

If we want to inform practice, we need to abandon such snobbery. Consultants employ very rich bi-directional channels and can, therefore, tune their models to the preferences of their clients. By ignoring these preferences, all we do is ensure that our own work seems foreign to our practitioner clients. If we do not take the time to understand what practitioners already know, why should they make the slightest effort to understand what we are offering?

Moreover, recalling Figure 4, we need to recognize that even where popular models are not entirely true, they survive because they are useful. In our paranoia about introducing Type I error (asserting something that is not true) into our research, we fail to recognize that a bad model is often better than no model at all. Not making a decision is, in itself, a decision. Often it is the worst one a practitioner can make.

## 3. *Develop resonant messages*

As suggested earlier by Figure 3, the odds of successfully informing an expert with a straightforward proposition are very low. Fortunately, we are not limited to informing through propositions. As noted in the previous item, once we understand our client's preferences for models, a great deal of informing can be accomplished by framing our message in a way that is consistent with these preferences. Of course, that will usually require that we drop the endless qualifications we offer to insure that we are not later embarrassed by subsequent research suggesting that we were wrong. But, if we overcome our irrational aversion to Type 1 error, the sting of being occasionally contradicted will be far less frightening.

There is another piece of good news. There is a message format that we, as humans, are particularly tuned to: the story. There is a large body of research that demonstrates the power of the story to inform [e.g., Heath and Heath, 2007], as well as some proposed explanations for this uncanny ability [e.g., Gill, 2010]. From an informing standpoint, if we conduct research that seems unlikely to produce an interesting story, then we have just thrown away the most powerful arrow in our quiver of techniques for informing practice.

## 4. *Cultivate rich bi-directional channels*

Over the long term, our goal must be to establish channels that are capable of conveying any message to practice—not just those messages about which the client is uncertain. There will certainly be times when our goal will be to correct misconceptions or deliver knowledge that is not to a client's liking; as academics who are not the paid servants of a particular organization, we are well-suited to that role. This is a valuable service but, at the present time, one that our existing channels simply cannot support. The solution then is to cultivate new channels capable of sustaining more powerful influence and to nurture existing channels that are similarly capable but are today underutilized or underappreciated.

A variety of such channels is described in detail elsewhere [Gill, 2010]. Some examples include:

- Action research
- Case writing
- Professional doctorates, including both those intended to make junior professionals better attuned to research and those intended to prepare senior professionals for a transition to academia
- Sabbaticals in practice for faculty members
- Faculty participation in professional service organizations
- Treating consulting activities as a form of research activity that corresponds to the external grant in the sciences. Implicit in this conception is that the researcher would provide the institution with a percentage of the activity's revenue as overhead—precisely as is the case with today's external grants.

What is critical, from an informing perspective, is not the tangible output of activities such as these. Rather, each of these activities—as well as others not listed here—represents a step towards constructing a network of rich bi-directional channels through which research findings can flow.

##### 5. View practitioner experts as partners rather than strictly as recipients or clients

In viewing practitioners, there is a natural tendency among some academics to imagine them as being similar to students. Feeling that way, the researcher may also feel the paternal obligation to “protect” them from themselves. Our oft-mentioned aversion to Type 1 error is a perfect example of this attitude. If we do not resist such a state of mind, we can find ourselves seriously misaligned with the experts’ own attitudes. In fact, if those experts are reasonably familiar with academics, they are likely to feel a similar paternal need to educate us.

The only way to get around this dilemma is to view our informing relationship with practice as one of partnership, rather than one where they are strictly the client. There are many advantages to cultivating such a collaborative attitude. Partners share information with their partners more freely than with others. Partners develop ideas together, with the goal of making them resonant to both parties. Partners can inform each other with their preliminary thoughts and have confidence that each will protect the interests of the other.

At the present time, the MIS research discipline desperately needs partners who are motivated to protect its interests. We have become so obsessed with informing each other that we have no such partner at the moment [Gill and Bhattacharjee, 2009b]. If practice does not come to our defense, the best we can hope for is that enrollments will rise and that the flow of resources to us will, therefore, be further depleted only slightly. More likely, our research departments will find themselves ever more frequently absorbed by other business departments. MIS is too important to be ignored entirely. Should we continue to be unwilling to reach out to practice in an active way, we may lose the privilege of continuing to exist as an independent research discipline.

The good news is that other business disciplines are not far behind us in their irrelevance to practice [Gill, 2010]. Thus, if we are willing to forgo the status quo and adapt our activities to informing practice, there is a good chance that we could not only improve our situation, but could also blaze a trail for other departments to follow. It will be up to us to initiate the change, however. We cannot expect practice to follow our lead until we demonstrate that our understanding of their needs is worthy of such respect. We need to prove that we can, and will, do what it takes to inform them.

#### Neil Ramiller

My remarks during our panel session focused on how taking a neo-institutional perspective on the research agendas of the field could help in two ways. The first is in sorting out normative claims—like those of my co-panelists—about the “proper” approach to setting agendas. The second is in grasping the magnitude of the challenge involved in actually implementing a given approach.

In regard to taking a normative position myself, I conceded that I was not proposing and defending a position at quite the same level as my co-panelists, whose debate in the pages of the December 2009 issue of *MIS Quarterly* had provided the inspiration for the panel. That is, rather than take a stand for our field leading industry in fashion setting, or joining practitioners as informing partners, or taking some other approach, I reflected on how the merit in any such proactive strategy must confront established norms and rather profound inertial mechanisms. This pointed, in a sense, toward meta-strategic considerations in efforts to transform our agenda-setting processes.

#### Theory

My thoughts during the panel originated for the most part in an article I wrote with Burt Swanson and Ping Wang on the value of taking what we called an *institutional ecology* perspective on the matter of research agendas [Ramiller et al., 2008]. That perspective emerged around three observations, of which I summarized two for the purposes of the panel discussion.

The first observation was that our field’s research sometimes chases after whatever is current and, frequently, a craze in industry, without doing much service for durable scholarly knowledge. When this happens, there is reason to doubt whether we contribute much to industry knowledge either. Instead, we often simply go along with industry’s current belief.

When I was doing my dissertation research under Burt’s supervision, such crazes included CASE and data warehouse, among others. Today, we see ads for such things as “Chair in Cloud Computing for M-Commerce in Service Markets over OSS Platforms.” We should all wonder what that will sound like in five years—or even two years.



As an aside, recognizing that some area of innovation is a craze does not necessarily imply that it has no practical merit. The label *craze* just means that we're witnessing hyperbolic discourse and an exaggerated response. In this regard, hype can actually be functional if, indeed, hazardous, a point I have argued elsewhere [Ramiller, 2006].

Burt's, Ping's, and my second observation was that we also witness situations where significant attention and resources of the field get focused on topics of little or no interest to industry and of little import for scholarly knowledge more generally. (During the panel I abstained from giving examples, but did remark that everyone has a favorite, and that many of us share the same favorite.)

Our account of this pair of divergent observations is that both arise from researchers' responses to market demands that take shape within institutionally constituted sets of expectations.

In regard to fashion following, as members of an applied field, we believe we ought to speak to what industry is interested in. That principle or ethic doesn't necessarily lead us to jump on bandwagons. Often enough, however, we do.

In regard to the problem of research insularity, self-sustaining academic sub-communities can develop around certain research topics, given the right set of circumstances. Such circumstances commonly include sponsorship from powerful senior scholars who have the means to attract junior scholars in number to the cause. Given the friendly support of doctoral programs and some early favorable treatment in journals, such a sub-community can grow to command a lot of turf in terms of conference slots, journal pages, and faculty positions. All of this is fostered by the basic institutional machinery in academia that allocates opportunities and rewards.

In short, we concluded that there are both external and internal markets, each shaped by the institutional structure of expectations and rewards peculiar to a science that is both applied in ambitions and centered in the academy.

#### Recommendations

Again, during the panel I noted that I would not be offering recommendations as specific as those of my co-panelists, which had produced such a provocative contrast in how each thought scholars in our field ought to relate to practitioners. I granted that in our community having a meaningful relationship with practitioners—however that might be accomplished—was certainly a good thing. However, the point I hoped to make was that any proposal or (better yet) initiative to change how we set our research agendas would demand a realistic and critical awareness of the forces that are actually in place and currently shaping how we in fact choose what we do. To do something different will require a confrontation with those forces.

Institutional theory tells us that change is hard—and sometimes even unlikely. It also tells us that we need to uncover the structural conditions that exert power to maintain the status quo. Just as significant, institutional theory illuminates how we are sometimes our own worst enemies, when it comes to creating change, because of how we reproduce the existing order based on what we take for granted.

In short, rather than anything as direct or bold as calling for academics to lead practitioners (Baskerville) or to inform practitioners (Gill), I proposed that we need to inform ourselves. In particular, we need to inform ourselves about how our institutionalized responses to exoteric and esoteric markets for research help to reproduce those very markets. Appreciating the grip which this cycle of reproduction has on our practices as scholars is crucial to any kind of proposal to change how we set our research agendas.

As a part of such reflexive inquiry, we also need to examine how we allow ourselves to be led. Among other things, this demands scrutinizing the practices of the institutional entrepreneurs in our midst, those thought leaders who manage to convince us that certain topics require our attention, and journal space, and academic lines. There is method to institutional entrepreneurship, about which many more members of our academic community would do well to be knowledgeable—if only to be ready to challenge entrepreneurs who may over-sell a particular research agenda, to the detriment of our attending to other possibilities.

#### Comments on My Co-Panelist's Positions

Because my "meta-strategic" perspective was not directly opposed to the positions of my co-panelists, I did not find it sensible to contrast my position with theirs. I did remark that their disagreement was, in itself, perhaps a debate between complements.

In regard to Baskerville's proposal that scholars lead industry in fashion-setting, the phenomena of innovation fashions do indeed matter in practice because of the role they play in creating salience, fostering interpretation work,



and getting people and organizations moving. Gill's informing perspective, then, strikes me as an entirely useful way to think about how to move from the broader vision encoded in fashionable innovation discourse to its mindful realization in real and varied contexts of practice [Swanson and Ramiller, 2004].

In short—and here I responded to Gill's response to Baskerville's response to Gill's original article—information systems are both fashions and diffused innovations. It takes a kind of multi-level thinking to grasp the inter-relationship involved, but once you do, you can see that this is necessarily so.

I concluded with a couple of points of concern about the arguments of my co-panelists that could make realizing the complementarity of their positions difficult.

When it comes to academics leading in fashion-setting, the institutional perspective would raise doubts about accomplishing what amounts to a fundamental transformation in roles and expectations. For example, will teaching our students how to read and appreciate scholarly research really be enough to change industry's inclination to mostly ignore us? And will simply presenting our research, as is, to a more properly conditioned and better educated practitioner audience then put us out front in defining IT innovation fashions?

In regard to informing science, I noted the appeal of its call for practical engagement. However, its apparent distaste for theory left me wondering: What do we academics have to uniquely contribute without theory? I concur with Gill's observation about the "combinatorial" problem in theorizing, in which variables become massed upon variables as scholars try to generalize across cases. However, this is characteristic of a particular species of theorizing, specifically variance theory [Markus and Robey, 1988].

When Gill then talks about scholars creating "resonant stories," I enthusiastically agree. But I would go on to point out that these stories are, in fact, theories! [See Ramiller and Pentland, 2009.]. And here I see the strongest point of complementarity between my co-panelists' positions, at least potentially. Both are really in the story-telling business—or they ought to be. And both are in the theory business, Baskerville mainly from the top down or outside in, Gill from the bottom up or inside out. Where they meet in the middle we will find the more sharply focused questions about setting our research agendas: What are the stories we should create for practitioners, and how can we best tell those stories?

#### IV. DISCUSSION AND CONCLUSIONS

This article has set out three distinct perspectives concerning the setting of our research agendas in Information Systems. These perspectives were presented and debated during a panel session at the 2010 International Conference on Information Systems (ICIS).

In summary, Richard Baskerville, arguing from a management fashion perspective, said that we need better educated and informed practitioners. The "problem" might not be with researchers, but with Information Systems practitioners. We need "scholar-leaders" who are prepared to engage with and help transform practice. Grandon Gill, arguing from an informing science perspective, said that educating practitioners to be more informed about research is unlikely to solve the problem. The problem is with IS researchers themselves. Our theories are too simple to capture the realities of a complex environment, and our publication channels are not well-suited to informing practice. We need to reform these publication channels. Neil Ramiller, arguing from an institutional ecology perspective, said that we first of all need to inform ourselves. We need to be more reflexive about our own institutional responses and practices (many of which we take for granted). From a meta-strategic perspective, both Baskerville's and Gill's positions can be seen a complementary and not necessarily opposed.

Where we all agree is that there is a need to debate about how we go about setting our research agendas and how these agendas should be related to practice. We hope that this article will contribute to the establishment of such a debate. Whether the three theoretical perspectives proposed are the best or only ones that can be taken remains to be seen.

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