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Selecting Research Topics: Personal Experiences and Speculations For the Future

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SELECTING RESEARCH TOPICS: PERSONAL EXPERIENCES AND SPECULATIONS FOR THE FUTURE

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ABSTRACT

In the rapidly changing field of information systems, every researcher faces important choices about what research topics to explore and how to pursue that research. This paper addresses these questions by summarizing a panel discussion at the 2001 Decision Sciences Institute (DSI) annual meeting. The first part of this paper provides a framework explaining factors that can be used in selecting research topics. The following parts explain how our own past choices of research topics reflect the factors in the framework. In the final section, applies the framework to speculate about promising research topics for the future.

KEYWORDS: research methods, research design, is research issues, IS research agenda

I. INTRODUCTION

A panel discussion the Decision Sciences Institute (DSI) annual meeting in San Francisco on November 18, 2001 was to focus on the topic "Future Research Directions in Information Systems." Unfortunately one panelist was not able to attend, but the other two panelists discovered an overlap in their prepared comments. Instead of just identifying research topics, Alan Dennis came to the panel prepared to explain how he typically identifies research topics and factors he considers when designing research projects. Based on his comments about desired characteristics for research topics, he identified a number of research areas that he believed were quite promising. Steve Alter came to the panel prepared to explain how his recent research had focused on a broad set of topics that deserve much more research. As the session unfolded it became apparent that Alan's comments provided a framework within which Steve could easily explain why he had chosen the direction and research approach he used.

This paper builds on the panel discussion by emphasizing how our ideas about selecting research topics can be applied to explain past decisions of both authors and to help suggest potentially valuable research directions for the future. First, we explain one approach to identifying and selecting research ideas to consider (Section II). Next, Steve applies these factors – post hoc – to summarize how he defined and pursued the main thrust of his research over the last decade (Section III). Alan follows with a parallel post hoc analysis showing how his past choices of a number of different research topics reflected these factors (Section IV). In Section V, we use the framework to speculate about promising research topics for the future.

II. A FRAMEWORK FOR SELECTING RESEARCH TOPICS

As faculty, we tend to teach our students a formal "rational model" of science in which research activity is driven by a solid understanding of prior work. Under this approach, research topics emerge from a careful analysis of prior research and theory. We believe that the rational model is a good model, but it is not the only model.

Over the years, we worked on a number of research topics including decision support systems and group support systems. But, in looking back over our respective bodies of work, it is clear than many of our decisions to pursue an overall research stream or specific topics were driven by factors not included in the rational model.

Perhaps, the best explanatory model for our past individual decisions is the garbage can model [Cohen and March, 1972; Dennis and Valacich, 2001; Martin, 1982]. The garbage can model of decision making, when applied to the research context, would argue that decisions about research topics are often made as decision opportunities present themselves rather than following an overarching planned strategy. Decisions are driven by the some of the same factors as in the rational model (i.e., previous research and theory), but are also affected by other factors, such as current practice, predictions of future practice, prior personal experiences, resources available to conduct the research, and research occurring in other disciplines.

SELECTING IDEAS TO PURSUE

Table 1 presents some of the factors we usually consider when we look for research topics to consider. In most cases, however, the main issue is not finding ideas, but separating genuinely promising research topics from research topics that might be not be as important or might turn into dead ends. In this section we focus on selecting ideas to pursue. More information on finding research ideas can be found in Martin [1982] and Dennis and Valacich [2001].

Table 1. Factors in Selecting Research Topics

Factors in Finding Ideas to Consider	Factors in Selecting Ideas to Pursue
• Previous Research	• Study Fundamental Issues
• Current Practice	• Simplify Complex Theories
• Future Practice	• Study Anomalies
• Personal Experience	• Create News Value
• Other Disciplines	• Fit with Current and Future Research
• Resources	

Study Fundamental Issues

Significant research should focus on fundamental issues. These issues are important today and are likely to be important tomorrow. It is a pity to do a research project today only to find that it focuses on a topic that is not important two years from now. For this reason it is important not to do research on the limitations of the current version of a particular software package or the features of the current version of a particular hardware technology. The research should always be about fundamental topics such as human limitations, new types of computer-enabled capabilities, distributed business processes, or human-computer communication.

Simplify Complex Theories

Most research builds on past research. At minimum, past research simply provides a baseline for explorations, but most research takes previous research and extends and improves it. The result is a gradual building of theory and empirical evidence that slowly but surely adds

richness and depth to our understanding of a phenomenon. Each study contributes by adding a new concept or relationship. Groundbreaking research does not add another piece to the puzzle; it removes pieces. For example, in the 1980s a number of researchers were looking at whether graphical or tabular displays of information were better and under what circumstances. Then Iris Vessey [1991] re-conceptualized the entire issue around cognitive fit and suddenly everything made sense. The real question was not whether graphs or tables are better, but rather, how we could best match data presentation to task needs. As a result of that insight, the entire thrust of research about whether graphs or tables were better changed.

Study Anomalies

Data, real world examples, and past research findings that don't fit existing theories or seem inconsistent are often excellent indicators of the importance of research. After all, if the previous understandings and findings just don't fit together, something is missing and perhaps it can be found if the correct question is asked. Kuhn [1962] argues that the existing paradigm persists until enough people start finding enough places where it just doesn't fit. Gradually someone comes up with a new approach or new insight that explains the past anomalies. By focusing on anomalies, one is more likely to develop an important new insight that changes the status quo.

Create News Value

Some things are just more interesting than others, and the same is true for research. As an area evolves, the number of people who remain interested gradually decreases. While the 24th paper in a particular research area may make an important contribution, fewer people will be interested. All things being equal, a new research topic will have greater "news value" to more people. Which would you rather read, another DSS or GSS paper or a paper on the effects of pervasive computing on managerial life?

CONCLUDING REMARKS ON THE FRAMEWORK

Life is too short and there are too many opportunities. That is why it is important to choose research topics that could be the basis of a series of research topics, not just an isolated project that has no follow-on. It is always easier to do your second study in an area than your first, and the third is easier than your second, because research builds on itself. In selecting projects, always look to build a stream of synergistic projects.

III. HOW I SELECTED AND PURSUED THE MAIN THRUST OF MY RESEARCH (STEVE ALTER)

In hindsight, the ideas in Section II about finding and selecting research topics fit well with my choices of research topics.

RESOURCES AND CURRENT PRACTICE

In my graduate school experience, advisors were a critical resource. I was fortunate to be a graduate student at MIT when Peter Keen and Michael Scott Morton were key players in the early work on decision support systems (DSS). I was always curious about current practice and real world applications of computerized systems and therefore decided to devote my thesis to trying to understand key issues related to creating and using DSS in organizations. This exploratory research occurred at the time when the possibility of DSS (versus Management Information Systems (MIS) and Transaction Processing Systems (TPS)) was a new idea. It included writing eight case studies and opportunistically extending the sample to 56 cases. This effort eventually resulted in a book on DSS and a number of articles published while I was on the faculty of the University of Southern California.

But then I moved to San Francisco for family reasons and joined a graduate school friend who was starting Consilium, a small consulting company that morphed into a manufacturing software company, went public in 1989, and was acquired by Applied Materials in 1998. Working on the management team of a start-up for eight years was exhilarating, but start-ups tend to have

major ups and downs, and shortly after one of the downs I decided to return to the more manageable lifestyle of academia and joined the faculty of the University of San Francisco (USF).

Returning to academia left me with the decision of what research topics to pursue. While at Consilium I maintained some contact with academia by teaching several courses at the University of California at Berkeley and publishing occasional articles related to DSS, but I came to doubt whether DSS were fundamentally different from other types of information systems. In particular, if DSS were fundamentally different, why had I almost never used the term DSS when discussing Consilium's software, which performed complex transaction processing but also provided data and models that supported decision making? While at USC years earlier I had worked with a team developing interactive planning models that raised interesting theoretical and practical issues, but the research center that had supported the modeling research had disbanded. I wasn't sure what direction to take. The following attempts to show how the decision factors in Table 1 help in explaining the course I took.

PERSONAL EXPERIENCE

While with Consilium, I had the impression that some customers bought our software without understanding that its value would be realized only if their firms used it to improve the way they did important work such as planning production, controlling operations, and identifying and responding to manufacturing defects. The problem seemed not to be about understanding the numerous features and details of the software because no one seemed to complain about our reasonably clear product concepts, consistent screen design, good documentation, and reasonably good demos. On the other hand, it sometimes seemed to me (without proof) that some of our customers and even some of our staff members did not see the relationship between software features and work practices.

During my first year of teaching at USF I also came to believe that the information systems texts I used both at USF and at USC a decade earlier would not have helped either our clients or our staff attain a better understanding of the connection between software capabilities and work practices. My personal experience implied that this elusive connection might be an area for valuable research, but I wasn't sure what to call it or how to proceed.

In the interim, I mentioned my dissatisfaction with existing textbooks to a sales representative from Addison-Wesley, which had published my DSS book. One thing led to another and the first edition of my information systems textbook was published in late 1991.

FUNDAMENTAL ISSUES

Eventually I defined my goal as follows: "to develop a systems analysis method that a typical business professional could use to think about information systems at whatever level of detail made sense in that person's situation." In other words, the fundamental issue was the lack of an organized approach that was appropriate for typical business professionals. Presumably this method would emphasize something other than the data flow diagrams, entity relationship diagrams, and other techniques found in systems analysis and design texts for IS majors. Furthermore, the lack of an organized approach was probably related to the lack of a reasonably clear, widely accepted set of fundamental concepts for the information system field. If a set of understandable fundamental concepts existed, business professionals and IT professionals could probably use these concepts to understand and analyze systems in organizations.

I didn't know of any "research method" for developing a systems analysis method, but believed strongly that this was an important area and that if such a method could be developed it would lead to a significant stream of research involving various aspects of how business professionals typically think about information systems, how they might do this more efficiently and effectively, and how they might collaborate more effectively with IT professionals.

PREVIOUS RESEARCH

When I started thinking about doing research in this area I went to the USF library and looked at many of the books related to systems or systems analysis. On a trip to the East Coast I spent two days at the Library of Congress looking for books or articles that might provide ideas and direction. Visiting a library to search for IS-related articles and books might seem a bit

primitive today, but a decade ago no one had heard of online journals or Internet search engines. I don't remember whether the material I found provided many ideas, but I must have reached the conclusion that whatever I found was not clear, complete, and organized enough to make research in this area unnecessary. Ironically, some of socio-technical research that I might have seen at that time mentioned the term "work system," a term I later (re)invented as the central concept in the systems analysis approach that evolved.[1]

RESOURCES AND RESEARCH APPROACH

A promotional book tour in conjunction with the first edition of my textbook provided an important opportunity to work on the initial version of the systems analysis approach. At most of the campuses I gave a presentation to a group of professors. Instead of talking about the features and benefits of the book, I decided to use the book tour as a way to develop an initial version of the systems analysis technique. My presentation would be about what I viewed as the fundamental concepts that business professionals could use when thinking about information systems for themselves. In effect, the audiences for the numerous iterations of the presentation would be a resource that might help me clarify the ideas. I formalized the question as follows:

> *Assume that I have to give a one-hour presentation to a group of business professionals who will later attend an important meeting about a particular information system in their business. Like many business professionals they understand their business situation and may be familiar with office tools such as word processors and spreadsheets, but they have never received training about information systems. My presentation should increase their insight about whatever information system will be discussed in their meeting. Unfortunately, I face three unreasonable restrictions in preparing my presentation:*

> - *I cannot know what job or business background the business professionals have.*
> - *I cannot know anything about the information system they are discussing.*
> - *I cannot know the agenda of their meeting. They may be reviewing an existing system, evaluating a proposal from a software vendor, or creating a new system.*

If I could accept these unreasonable challenges and still say something useful I would be on my way to having a systems analysis technique they might be able to use. I cobbled together an initial presentation and gradually improved it by presenting it 22 times at universities and several research institutions. I watched the audience reaction to each presentation, recorded questions and disagreements, and tried to clarify the ideas. When the ideas seemed clear enough, I wrote a working paper summarizing what I had learned.

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[1] Only in 2001, after the Google.com search engine started to search PDF files, did I begin to find references to "work systems" that helped me track down references to this term in Mumford and Weir [1979], Davis and Taylor [1979], and Trist [1981]. From what I have found thus far, these researchers used the term at a more aggregated level than the definition I eventually used. For example, Trist [1981] said that "primary work systems (the first of three levels of analysis, the others of which are "whole organization systems" and "macrosocial systems") … are the systems which carry out the set of activities involved in an identifiable and bounded subsystem of a whole organization - such as a line department or service unit." [p. 11]… "The primary work system ….may include more than one face-to-face group along with others in matrix and network clusters." … "In a primary work system an individual is apt to have several group memberships." [p. 35] In contrast, the definition of work system that I currently use is: "A work system is a system in which human participants and/or machines perform a business process using information, technology, and other resources to produce products and/or services for internal or external customers."   Typical business organizations have work systems for obtaining materials from suppliers, producing and delivering end products, finding customers, creating financial reports, hiring employees, coordinating work across departments, and many other functions.

MBA and EMBA students in required information system courses were a key resource that helped me develop the ideas further. In over a dozen semester-by-semester iterations, I used successive versions of a systems analysis questionnaire as an integral part of required information system courses. Students in each course wrote group papers that:

- described an IT-enabled system in an organization,
- identified problems or opportunities,
- analyzed various aspects of the system, and
- justified a set of recommendations.

At each point during these iterations the papers involved substantial effort by the student teams, who typically viewed their work as an integral and worthwhile part of the learning in the course. I graded the papers consistent with the way I would grade any student paper and typically provided extensive annotations and comments about how the paper might have been better.

The process of providing feedback for students also generated ideas I could incorporate into the ongoing improvement of the systems analysis method. Some of the shortcomings in the papers involved carelessness or poor writing style, but other shortcomings seemed to be based on confusion and or lack of awareness of important issues. For example, many papers barely mentioned measures of performance even in justifying recommendations (despite the fact that many of these students had covered management accounting in another course). Accordingly, subsequent versions of the questionnaire were more specific about requiring measures of performance as part of the analysis. Similarly, many papers seemed confused about what system was being discussed, and in particular, whether the system was just an information system or whether it should also include physical activities. (For example, is the physical movement of packages part of the system an IS student should analyze when studying FedEx, or is the system just the processing of information about the packages?) Accordingly, subsequent versions of the questionnaire required that the recommendation be divided into three separate recommendations related to (1) changes in the work system based on changes in the information system, (2) changes in the work system that are totally independent of changes in the information system, and (3) changes in the information system that might make the information system more efficient (e.g., upgrade technology to make it more maintainable) but would probably have little impact on the typical operation of the work system.

SURPRISES, INSIGHTS, AND "NEWS VALUE"

This research effort generated a number of useful results including the work system framework, a principle-based systems analysis method, and the work system life cycle model. These ideas appear in my information systems textbook [Alter, 2002a] and in a series of papers in *CAIS* [Alter, 1999a, 2000, 2001a, 2001b; Alter et al 2001] and *CIO Insight* [Alter, 2002b]. Here are some of the main surprises and insights that emerged thus far:

1. From a business professional's viewpoint, viewing "the system" as the system of doing a particular type of work (called a "work system" (WS)) is more natural and useful than viewing "the system" as the information system that supports the system of doing work. For example, assume a business professional is thinking about a system for hiring new employees. This is a system of doing the work of hiring, not just an information system that processes information about job requisitions and job applicants. This question about the identity of "the system" explains some of the common confusion about how to respond to a software vendor's attempt to sell an HR system or manufacturing system. From a business viewpoint the vendor is not selling "the system." Rather, the vendor is selling software that is used in an information system that supports a work system. The software's capabilities and technical attributes are obviously important, but the software will have no impact until it is incorporated into the way significant work is done in the organization.

2. Information systems may support work systems through many types of relationships. The IS and WS may be separate; the IS may be a small part of the WS; the IS may encompass most of the WS; a given IS may constitute part of the several WSs; a large IS may serve many different WSs. [Alter et al., 2001, Figure 1] If the overlap between the IS and WS is minimal, it

may make sense to talk about the impact of the information system on the work system. On the other hand, if they overlap substantially talking about the impact of the IS on the WS makes no more sense than talking about the impact of your skeleton on your body.

3. When business professionals speak of system success they typically focus on the work system, not the information system. By representing work systems and information systems as partially overlapping, Figure 1 shows why the success of information systems (at least from a business viewpoint rather than an IT project viewpoint) is often related to aspects of the work system that may or may not involve the information system. Consequently, success measures that focus solely on the information system's impact are designed to ignore aspects of real world situations that are relevant and often easily understood. Although this difference may not detract from carefully controlled research on IS success, it poses a dilemma that might be described as the "Siamese twins problem" [Alter, 1999b]. Decades of advances in real time computing created greater degrees of overlap between the work system and the information system that supports it. In this environment, it may be self-defeating to focus on one twin and exclude the other from careful consideration.
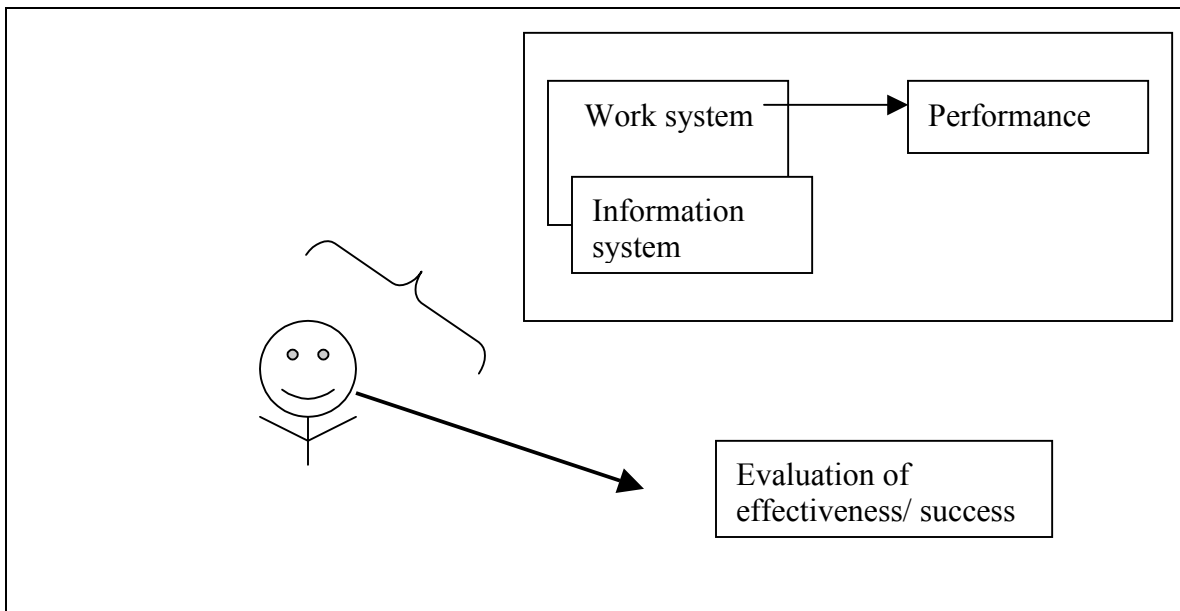

Figure 1. Success of What --- The Work System or the Information System that Supports It?

4. Even a cursory understanding of a work system involves the eight elements shown in the work system framework in Figure 2 [Alter, 2002a].   A business professional (or IT professional) trying to understand or analyze a work system needs to know something about these eight elements. Focusing solely on information needs or computerized information may support an idealized view of the business process but does not suffice in understanding the situation either from a business or IT professional's viewpoint.

5. General principles that apply to the elements of a work system can be used as the basis of a systems analysis method. Examples of these principles include "please the customer", "do the work efficiently", and "serve the participants." Whether or not the analysis starts with a pre-defined problem or opportunity, the principles can be used to identify problems in an existing work system and to guide the evaluation of possible changes or improvements in any part of a work system.
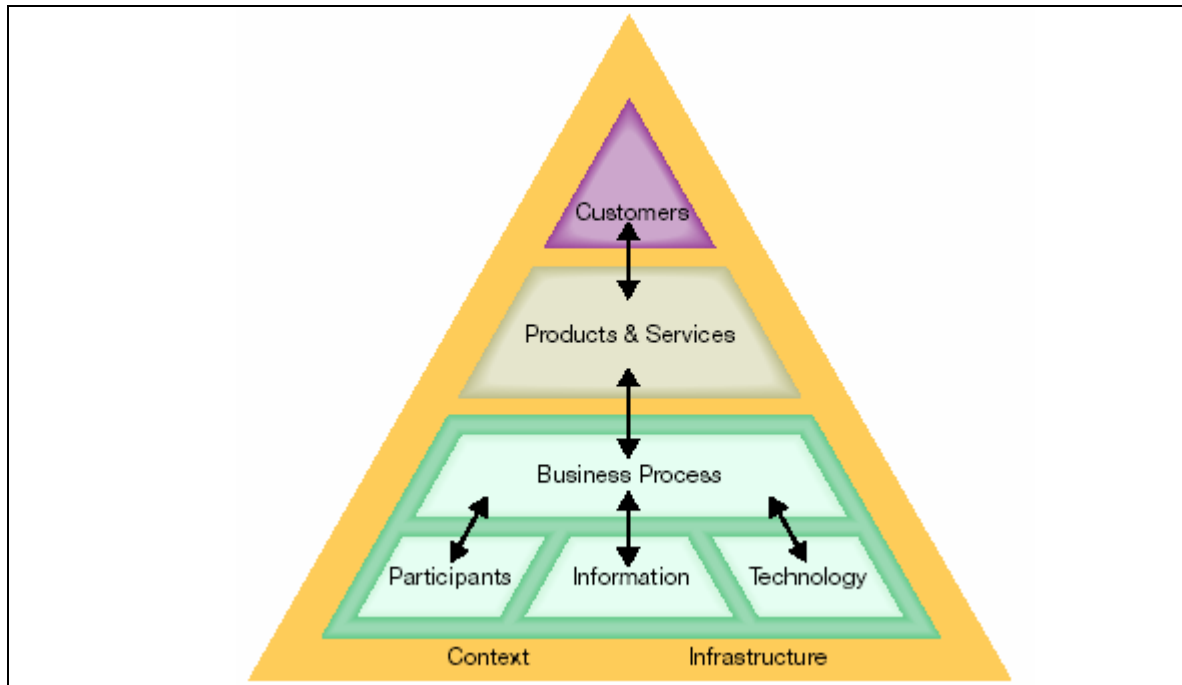
Figure 2. The Work System Framework

6. Information systems and projects can be characterized in terms of the eight elements shown in Figure 2 and are therefore special types of work systems. Consequently, vocabulary that applies to work systems in general also applies to information systems and projects. Although information systems and projects also have their own unique vocabulary, one might wonder whether a large majority of the concepts related to information systems and projects are actually concepts related to work systems and are best understood at that level. [Alter, 2001c]

7. Although the elements of a work system provide an intuitively understandable framework for summarizing almost any work system quickly, MBAs and EMBAs attempting to analyze a system-related problem or opportunity that has not been defined in advance for them often find it quite challenging to define exactly what work system they are trying to analyze. According to direct feedback from many students, this insight is one of the most important things that they learned from doing the systems analysis assignment. In terms of educational approaches related to systems in organizations and information systems specifically, it is much easier to talk about generalities and broad concepts than to try to apply those concepts to situations that were not filtered and sanitized to make the concepts fit easily. In other words, even students with a lot of business experience who seem comfortable using buzzwords such as value proposition, reengineering, e-business, and empowerment need a lot of practice in applying very basic system concepts.

8. A work system and any related information system and software each have a life cycle. A very general work system life cycle model (Figure 1 in Alter [2001b]) covers all three cases and may help in comparing the many project and life cycle models in the IS literature.

9. Success factors related to work systems in general should be inherited by special cases of work systems, such as information systems, projects, and supply chains. Inheritance explains why many of the typical success factors for information systems (e.g., management support, adequate resources, appropriate training, commitment, incentives aligned with goals of the system, adequate communication) are actually success factors for work systems in general. Consequently, research about success factors for expert systems will probably generate many of the same success factors as research about management information systems, CRM systems, and data warehouses.

IV. HOW I SELECTED AND PURSUED VARIOUS RESEARCH TOPICS (ALAN DENNIS)

As with Steve, in hindsight, the ideas about finding and selecting research topics fit well with my experiences. However, I would be hard pressed to claim that I used the ideas in foresight.

RESOURCES

When I arrived at the University of Arizona as a graduate student, the major research emphasis was on group support systems (GSS). A new large meeting facility was just completed and the department was looking for research and consulting projects to use it. I never really considered any other topic because there was such a major emphasis on GSS; the software and facilities just pulled me into the area.

PERSONAL EXPERIENCES AND AN ANOMALY

The first research project I deliberately undertook after arriving at Arizona, as opposed to those I was given because of my research assistantship assignment or course assignment, was driven by personal experience. At the time, most of the published research on GSS was being conducted with small groups of students in laboratory experiments. This research concluded that GSS didn't help much – even that it impaired group performance. This finding was in sharp contrast to my personal experiences working with large "real" groups from organizations in our two GSS facilities.

I was troubled by this anomaly between the published academic research and my personal experiences in the field, and resolved to confront it. First, I wrote a paper about the anomaly (Dennis, Nunamaker and Vogel, 1991) and then Joe Valacich and I decided to do a series of experiments on brainstorming because we realized that one of the major differences between the published laboratory studies and our experiences in the field was the nature of the task: decision making in the laboratory and idea generation in the field. We first focused on the effects of group size in brainstorming (Dennis, Valacich and Nunamaker, 1990), because that was the second major difference between the two: small groups in the laboratory and large groups in the field. We also went on to enlist the help of our friend Brent Gallupe and his colleagues – leaders in the laboratory side of GSS research – because we felt we could benefit from their experience (Gallupe, et al. 1992).

OTHER DISCIPLINES AND AN ANOMALY

A second major turning point in my research was also driven by an anomaly. After doing numerous studies on brainstorming, I decided to take a closer look at decision making. While the use of GSS clearly helped groups to generate more ideas, there was little evidence that its use was effective in helping groups make decisions. I wondered why. At this point, I stumbled across Stasser and Titus [1985], a psychology paper that looked at information exchange on "hidden profile" tasks and found that verbally interacting groups did not make effective decisions because they were not effective at sharing information. I now realized that the integration of information as well as the sharing of information could be important and this launched my next research stream looking at information exchange (Dennis [1996]).

SIMPLIFYING COMPLEX THEORIES

Much research in GSS is contradictory, with GSS use being found to improve performance, to impair performance, and to have no effect on performance. Prior work, including my own, tried to identify a host of factors that might explain the difference in performance, such as the task, the size of the group, the type of group, the type of GSS, and the use of a facilitator. While these factors may all be important, they do comprise a long list leading to a complex contingency theory.

One of my recent papers attempts to simplify this contingency theory approach by proposing a Fit-Appropriation Model (FAM). FAM argues that performance is based on the GSS's fit with the task, and the support the group receives in appropriating it (Dennis, Wixom,

and Vandenberg, 2001).   These two factors explain a significant amount of variance in performance, leading to a much simpler theory. While I would like to take credit for setting out to develop such a theory, I can't – and nor can my co-authors. We submitted a more traditional complex contingency theory paper and the reviewers challenged us to develop something new. It was this challenge that led to the re-conceptualization and a much simpler theory.

FIT WITH CURRENT AND FUTURE RESEARCH

In all the research I have done, I almost never started a research project without understanding how to do at least two studies on the topic. It is simply not worth the effort to do just one study, because the second study in an area can reuse many of the materials developed for the first study. Therefore, I produced a long series of papers on electronic brainstorming, a long series of papers on GSS-supported decision making, several papers on GSS-supported strategic planning, and several papers on GSS-supported systems analysis and design, to name a few.

V. SPECULATIONS ON PROMISING RESEARCH TOPICS

Many topics are promising for future research. In this section we use the framework developed in Section II and speculate on some especially promising ideas. Each of the following topics is very broad and can be addressed many different ways using different methodologies. A few brief comments and only a few references will be provided for each topic despite the fact that many deserve lengthy discussions and extensive literature searches. The topics discussed are:

- systems development,
- information and the environment
- future practice
- holistic understanding

SYSTEMS DEVELOPMENT

One of the fundamental issues in MIS is the development of new information systems; it is what sets us apart from all other disciplines. One key research issue is how business and IT professionals analyze systems individually and in collaboration, and how they might analyze and design systems more effectively. The development of the work system framework and related systems analysis approach stemmed from a belief that most business professionals do not use an organized method for thinking about systems in organizations, as may be indicated by the appallingly high proportion of systems that fail or are never implemented. The evolution of the work system approach to date is based on subjective assessment of which changes might improve the effectiveness of over a dozen iterations of a questionnaire. The current version of these ideas could certainly be developed further, and might be re-framed for greater effectiveness in a variety of specific situations.

Here are some of the possible directions for research involving the work system approach and/or any other approach related to how business professionals describe or analyze systems:

- How do business professionals perceive, describe, and analyze systems in organizations (whether or not IT is involved)?
- To what extent does the lack of an organized way to think about systems in organizations actually affect the likelihood of success for systems and projects in organizations?
- What are the relative advantages and disadvantages of different systems analysis techniques that business professionals could plausibly use? For example, what are the relative merits of the work system approach, Checkland's soft systems methodology

(Checkland [1981], Checkland and Scholes [1990]) and any other organized way to think about systems in organizations?

•   How well do business and IS professionals work together and what might be done to facilitate their collaboration? (e.g., Boland [1978] and Beath and Orlikowski [1994]).

•   How well do techniques such as JAD and RAD really work? (e.g., see Davidson [1999]) What preparation or other interventions would make these techniques more effective?

•   In Section III, we claim that IS and IS projects should "inherit" success factors of work systems in general and projects in general. To what extent is this assertion borne out by past research?

•   What happens in "messy" projects? What diagnostics provide early warning of impending disaster? What can be done to deescalate troubled projects? (Keil and Montealegre [2000])

•   What types of assumptions about projects often prove wrong as the projects unfold, and often lead to requirements creep and schedule and budget overages?

•   What factors and dynamics contribute to unrealistic expectations on the part of both business and IS professionals? What might be done to make those expectations more reasonable?

MUTUAL IMPACTS BETWEEN AN INFORMATION SYSTEM AND ITS ENVIRONMENT

A number of research topics are related to each link in the work system framework in Figure 2.

• *Technology – business process*: Orlikowski and Iacono [2001] call for greater attention to the IT artifact in IS research and suggest five potential research areas related to the IT artifact. Because the purpose of many IT investments is to change the way a business process operates, it would be worthwhile to compare situations in which these investments did and did not change business process operation significantly.

• *Information- business process*: It sometimes seems that information system designers assume that information in a database will be correct if reasonable validity checks are built into data entry procedures. What is the error rate in data in different types of databases? (Strong, Lee, and Wang [1997]) What types of data errors occur and what percentage of these might be prevented by better data validation? What is the impact of data accuracy on business process performance variables such as consistency, productivity, and cycle time, and how do circumstances determine the effects of data errors?

• *Participants – business process*: The IS field is often concerned about the impact of technology on users. (e.g., the IS success model [DeLone and McLean, 1992]). The work system framework says that the business process affects the participants directly and that the impacts of technology on work system participants occur through the business process. In other words, the business process has a stronger and more direct effect on the satisfaction and self-efficacy of work system participants, who secondarily happen to be technology users during some part of their work. What are the different special cases, and to what extent is it true that the business process affects participants much more than the technology per se?

• *Business process – products and services*: Performance variables for a business process (activity rate, productivity, consistency, cycle time, etc.) can be viewed as different from performance variables for products and services (cost to the customer, quality perceived by the customer, responsiveness, etc.). To what extent do business and IS professionals recognize this distinction? How strong are the correlations between specific process and product performance variables? How does this affect the understanding of systems in organizations?

• *Products and services – customers*: Measurement of customer expectations and customer satisfaction for IS departments has received a lot of attention (e.g. Kettinger and Lee [1994], Watson, Pitt, and Kavan [1998]) Is there a deeper linkage between customer satisfaction and the organizational significance of the products and services produced by IS departments?

• *Context – remainder of work system*: To what extent do business and IS professionals discuss context issues while analyzing and designing information systems? What types of context issues are deliberately ignored because they are too sensitive or embarrassing to

mention? (Argyris [1990], Scott-Morgan [1994]) How often do those issues come back to haunt projects?

> • *Infrastructure – remainder of the work system*: How do business and IS professionals make decisions about what capabilities to move out of individual work systems and into shared infrastructure? How well is technical, informational, and human infrastructure utilized?

FUTURE PRACTICE

As IS researchers, we often study current or past practices. Only rarely do we try to lead practice, for the simple reason that understanding an emerging technology requires a level of technology access that we often lack.

Nonetheless, sometimes we can predict the future and attempt to get in on the early stages. Some technologies are fairly safe bets for influencing future practice (e.g., mobile wireless, pervasive computing) while some IT-based business practices are also fairly safe bets (e.g., e-learning, virtual teams, privacy and security). The more we can embrace the future rather than the past, the more interesting our work becomes. Some specific ideas include:

> • *E-Learning*: Many companies are moving into the e-learning environment. Some embraced individual-based methods, much like the old stand-alone "correspondence-course" model, while others adopted the "shared-course" model of group discussions. How effective, efficient, and satisfying are each of these approaches, compared to traditional residential style programs and compared to no learning program at all?

> • *Virtual teams*: Although room-based GSS systems were not widely adopted in industry, systems to support virtual teams have been. How does work change when groups work virtually? How can we build more effective tools to support virtual teams? How can teams better integrate and use different media such as telephone and face-to-face meetings in a virtual world?

> • *Mobile Wireless*: How will the world change when mobile wireless is effective and cheap? Will we wear our computers? Will voice input become common?

> • *Privacy*: Much has been written about the demand for privacy on the Internet. The European Union enacted strict privacy laws. Special interest groups lobby the U.S. Congress. Yet for many people, privacy is a non-issue; many simply do not care if their Internet surfing and purchases are recorded because there is some potential benefit from such monitoring and personalization (e.g., Amazon.com's recommendations). Is privacy a real issue or just a special concern to some? Why are some people concerned and others not? Is there a market for privacy?

HOLISTIC UNDERSTANDING

IS research might be divided into three levels based on the extent to which the research treats systems in organizations as holistic entities rather than sets of separable components that can be studied individually. The most integrated level deals with systems as holistic systems in operation. An intermediate level deals with one or several components of a system in operation The least integrated level deals with one or several components completely outside the context of a system (e.g., developing knowledge about system participants by studying psychology outside the context of systems or developing knowledge about business processes by theorizing about alternative representations of work flows)

The first approach, dealing with systems as holistic systems in operation, is more difficult than the others because many disparate variables must be considered or reconciled in order to tell a cogent and useful story, no less provide insight about how that story is related to general principles that might be applied elsewhere. On the other hand, to the extent to which the core of the IS field really is about IT-intensive systems in organizations, the long term usefulness of IS research depends at least in part on developing better ways to go beyond focusing on their individual components and to understand systems as systems.

VI. CONCLUSION

This article presented a framework for selecting research topics, illustrated how the choices we made could be explained using the framework, and then used the framework to speculate on future research topic selection. Although we believe these topics are useful, many other topics are useful. For example, just as we completed this article, Baskerville and Myers (2002) published a paper about IS as a reference discipline that presented a table of concepts, theories, processes, and applications adapted from Davis (2000). Their list overlaps with ours in a few areas but also mentions many potentially fruitful topics and approaches that we did not mention. Obviously there is no dearth of possible IS research topics. We hope this article and the framework we present will help readers think about and select research topics in a purposeful and methodical way.

Editor's Note: The authors report that they contributed equally to this article and their names are listed alphabetically. CAIS has taken the unusual step of listing their names alphabetically on the same line rather than one after the other, which is the usual case. This article was submitted on February 23, 2002 and was published on March 22,2002.

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LIST OF ACRONYMS

DSS	Decision Support Systems
EMBA	Executive MBA
GSS	Group Support Systems
MIS	Management Information Systems
TPS	Transaction Processing Systems
USC	University of Southern California
USF	University of San Francisco
WS	Work system

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