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MIS RESEARCH: REFERENCE DISCIPLINES AND A CUMULATIVE TRADITION

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ABSTRACT

This paper discusses what is needed to make MIS into a coherent research field. It defines 3 main needs:

1. Clarification of reference disciplines.

2. Definition of the dependent variable.

3. Building a cumulative tradition.

It reviews the relationship of MIS to computer technology and to practice and assess the publishing inlets for MIS research.

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1. INTRODUCTION

At present, MIS research is a theme rather than a substantive field. Luckily, since computers are important and knowledge of how to use them limited, academics have been given a line of credit to draw on, and can expect that universities will eagerly continue to hire assistant professors in MIS even while they bemoan the poverty of their seniors' research.

Perhaps MIS _is_ only a theme. Perhaps, like Organizational Behavior and Business Policy, it is a convenient umbrella term for a hybrid, applied field which is more easily defined in terms of the MBA curriculum than research. Perhaps MIS will eventually be absorbed into other more clearly-defined disciplines, such as accounting.

This paper assumes that MIS _can_ become a coherent "classical" area. For this to happen, a range of issues must be resolved. The ones below are the main topic of the paper:

1. What are the _reference disciplines_ for MIS? What fields -- if any -- provide the model for good MIS research? By default, in the current academic climate, microeconomics and computer science are the assumed references: microeconomics as the classical analytic discipline and computer science as the technical one. These seem too constrictive for a hybrid, application-based field which is organizational and managerial in focus.

2. What is the _dependent variable_? Until we have a coherent defintion of "information" we have nothing to measure. Surrogates for improved information, such as user satisfaction or terminal hours of usage, will continue to mislead us and evade the issue of a theory of information for MIS.

3. How do we build a _cumulative tradition_? Unless we build on each other's work, a field can never emerge, however good individual fragments may be.

4. What is _the relationship of MIS research to computer technology_? For MIS to be an independent field, it must obviously go beyond fads and reactions

to new hardware and offer something that remains meaningful as technology changes.

5. What is the relationship to practice? By implication, MIS is concerned with the application of computers, but research should provide something different than do consultants.

6. Where should we try to publish? There is no major MIS journal, so that we are always trying to force-fit our work to suit some other field's style, axioms and themes.

The specific answers to these questions are less important than the introspection they stimulate.

Partly because MIS has been rushed along by a combination of successful innovations in technology and, until recently, only partially successful efforts to harness them, there has been remarkably little such introspection. The answers to the six questions above implicit in current research activities seem to be:

1. The reference discipline is computer science, or experimental social psychology or, alas, the Intercollegiate Case Clearing House. There is no clear theoretical base and no match between theory and method. The main methodologies seem to be naive experiments, narrative cases, and atheoretical questionnaires plus atheoretical regression or factor analysis.

2. The dependent variable is defined in terms of surrogates, surrogates, surrogates. There is no theoretical base for MIS, which is why so much of our work is full of "frameworks", untestable assertions and surveys.

3. There is virtually no cumulative tradition. With few exceptions, there has been no continued follow-up on interesting lines of inquiry. Many senior MIS academics have been inactive or have shifted to a new area. The applied computer field is too interesting. Researchers in the area of, say, algorithms for bi-value martin-gales in financial retrogression wake up on Monday, stretch and get down to the next paper on A.B.M.F.R.; MIS researchers get diverted almost daily, by new research ideas, gee-whiz applications, consulting, etc. The senior researchers desert the field and far too many of the rising stars quit to found a company.

4. The relationship to technology and relationship to practice are improving. We are no longer quite so dominated by fads, although the jump on the Office of the Future bandwagon is distressingly familiar. A real dilemma for MIS research is that there is such a demand from practitioners; one may build a lengthy publication list without having to try very hard or face the Russian roulette of referee's reports.

5. Publishing outlets: a major, discouraging problem. The outlets receptive to MIS themes and tolerant of non-classical (non-OR, non-computer science, non-microeconomics) approaches lack prestige, and, often readership (MIS Quarterly is not even in the library of major MIS universities). The MIS department of Management Science seems unsympathetic to work whose reference discipline is not microeconomics. There are too many new journals that vary in quality and reinforce the fragmentation of the field, (Information and Management, Human Systems Management, Social Issues in Computing).

2. REFERENCE DISCIPLINES

For trying to sort our reactions to one's own and one's peers' research, the concept of a "Reference Discipline" (R.D.) is enlightening. An R.D. is an established field to which one looks to get an idea of what good MIS research would look like, if one could ever do it. Tenure committees think in terms of R.D.'s. A main weakness of MIS is that we have no clear criteria for evaluating our research. We look muddled, messy and fraudulent to people in information economics and Operations Research. They ask us what MIS is and we have no convincing answer. They then assume either that the R.D. is computer science or that their own field should be it; information economics at least has theorems and axioms and OR has analytic rigor.

Since MIS is a fusion of behavioral, technical and managerial issues, there is no obvious or single reference discipline. It seems fair to state, however:

1. Microeconomics, OR and computer science are not always suitable; they are highly convergent and require precision. MIS research at present must often be divergent and broad in scope and will have to work towards rather than from theory.

2. By studying one's R.D. one improves one's MIS research. For example, many of us are interested in the relationship between cognitive style and the use of information. The R.D. here is obviously cognitive psychology. It is essential then to look at that field in detail, not necessarily adopting its theories and methods but at least assessing what they imply for our own work. The naivety of many MIS experiments reflects a woeful lack of knowledge of experimental psychology.

Similarily, behavioral researchers in MIS have made useful efforts to study political issues in implementation. In general the research has been narrative and interpretative. The phenomena of interest fall into the inconvenient category of n=1.

Political science is the obvious reference discipline here. It has a relevant set of methods and theory and, more importantly, provides an approach to making case studies more than narratives. This is not to say that political science is packed with outstanding research that we should imitate; much of it is mediocre and vacuous, and it too has unsolved conceptual and methodological problems. As a reference discipline it clarifies what a research strategy should be for MIS - and at least establishes some criteria for evaluating the quality of MIS efforts in this area.

These criteria are different from those relevant to other "behavioral" research in MIS such as that on cognitive style, the impact of computers on organizational control processes, or the sociology of computer work. Too often, we look for "the" definition of MIS at one extreme or classify it in terms of very broad labels such as "behavioral", "technical" or "organizational". For MIS to be both coherent and expansive, we have to be sensitive to legitimate differences in theme and approach to research without ignoring the need to establish measures of quality.

The question of R.D.'s is worth a lengthy discussion. For this paper, there is room for only a few assertions:

1. Those who try to do exeriments, surveys and "behavioral" research had better define the R.D. as a way of introducing some quality control.

2. Information economics, though largely impractical, trivial, and at times fatuously overassertive, may be an excellent R.D. for those concerned with a theory for MIS, since it demonstrates how to approach the issue of defining information and presents an analytic strategy for developing and applying theory.

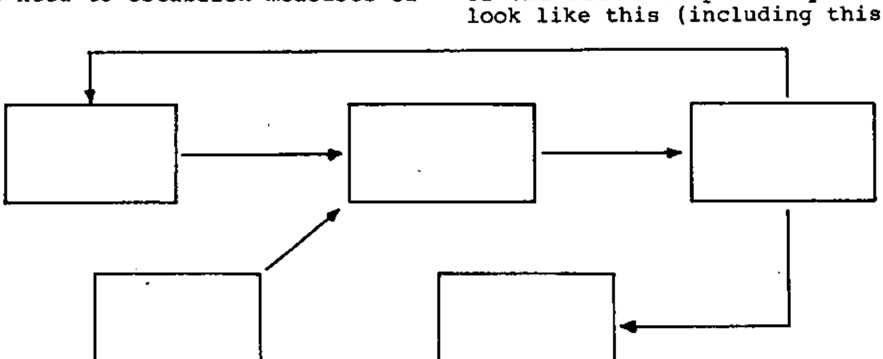
3. If a piece of MIS research is poor, judged in terms of its R.D., then it is poor.

4. If it does not have a clear R.D., it is likely to be confused and ill-executed.

5. The reference discipline is only a reference. Research that is firmly grounded in a given field may not contribute to our understanding of Management, Information, Systems, or Management Information Systems, however high its quality. Some of the work done in MIS departments on database and software tools belongs in computer science.

Perhaps the clearest value of this focus on R.D.'s is in handling doctoral students. Since there is no consensual core to MIS research, they are often puzzled as to how to structure their preparation. One can guide them best by focusing less on the content of their dissertation than on the background discipline(s) that help clarify the why and how of their work rather than the what.

One useful consequence of thinking in terms of an R.D. is that it discourages one from falling into the framework-of-the-month trap. Many MIS frameworks look like this (including this author's):



(By calling them a contingency theory, one makes them untestable.) The boxes are arbitrary and the arrows largely atheoretical. The hypothesis presented here is: the firmer an MIS researcher's work is rooted in a coherent R.D., the less likely he or she is to issue a new contingency theory/framework.

This is not to deny the value of contingency theories, only to point out that they must be indeed theories and link clearly into a set of axioms,definitions and methods. Without an intellectual grounding in a coherent tradition of science, they are too easy to generate, too hard to challenge and too difficult to apply.

3. THE DEPENDENT VARIABLE

MIS research has no theoretical base at present since it has no consensual definition of information. When we discuss the impacts of changes in information systems on organizational or individual processes, we have little to measure. The dependent variable is at best a surrogate: "user satisfaction" or "hours of usage". We have no concept of information comparable with that of information economics, information theory or even accounting. Consequently, we lack axioms and theorems.

Unfortunately, the practitioners most receptive to our research are not interested in a theory of information and information use. After all, there is a vast range of practical, interesting issues to be explored, in which "information" is equivalent to "computer-based" something, and "success" equivalent to "use".

Is there any academic field whose basic definitions are so vague? Concepts of Office Automation, Decision Support Systems, MIS, information, success, etc., etc., lack any coherence. As rallying cries, undefined terms are acceptable, but when we come to measurement, the lack of a theoretical base and a set of axioms and theorems has woeful consequences.

MIS is -- must be -- an applied field. In a sense, the practitioners are right to emphasize action over philosophy. That said, the search for the dependent variable and an articulation of the I in MIS seem to be a critical issue for our research. Vague theorizing is unlikely to help much; it results in yet more "frameworks". Of course, many of us cannot contribute -- should not be expected to do so -- to the theoretical base for MIS. We must, all, however, encourage our very best doctoral students to tackle the issue and make it clear to them that a theoretical contribution to MIS will be valued for its own sake and that the question of application need not be directly answered.

To provide such a contribution, the student will need a thorough training in relevant reference disciplines. At M.I.T.'s Center for Information Systems Research, we have defined our research portfolio as the study of the Effective Design, Delivery and Usage of Information Systems in Organizations. This point is relevant here, since it indicates the importance of reference disciplines and indeed of theory:

1. The word "effective" forces attention to the dependent variable: what is effectiveness and how is it measured? This question throws us outside the technical discipline of computer science. We are concerned with a literal economy of information and the reference disciplines may be microeconomics, social psychology, or any field which deals with assessments of performance or valuation of information.

2. The phrase "in organizations" seems central for MIS, as opposed to, say, Human Information Processing. In itself it points to both specific reference disciplines and a concept of information very different from that of much of information theory and information economics:

   a. "information" and organizational design are closely linked, so

   b. "information: may be best defined in terms of a process of exchange and negotiation, flows and nodes, cognitive transformations, and not as a physical commodity.

We at CISR are finding that the central issue for integrating our individual efforts and training our doctoral students is to pin down the meaning of effectiveness. The relevent literature is scattered over several fields, especially microeconomics, accounting and organization theory.

Most doctoral students in MIS do not get a basic training in the fields that are relevant to "the effective design, delivery and use..." Few MIS academics could give it to them. In many ways, designing such a course can clarify both the theoretical issues and the relevant reference disciplines. In practice, however, the few Ph.D. courses for MIS cover only the theory that directly links to technique and practice. We thus overtrain students in the Carnegie concepts of decision making, because the application of MIS supports the decision process, not because the Carnegie school focuses on concepts of information (although of course it includes them). Where there is any focus on organizations, effectiveness and measurement of the dependent variable, there need be little discussion of technology and applications. In the means-end link for MIS, the end is improved something and the means are computer and information systems. The search for the dependent variable relates to ends, not means. Few, if any of us, were trained in MIS. We come from other disciplines. because of the focus on means -- computers and techniques -- we have had little incentive to clarify the end.

More importantly, too many MIS researchers are not adequate MIS scholars. They do not know much of the literature relevant for basic training of doctoral students. If every MIS researcher needs to know the fundamentals of computer technology surely he or she must similarly have a sense of the literature on the dependent variable. To be an expert in one's own area of research involves detailed understanding of a reference discipline. To be an expert in MIS, that understanding has to be linked to the dependent variable. The core of MIS relates to this.

A call for a "good" theory of information is meaningless. It would be foolish for MIS to try to become a "science". For a start, those who talk about science when discussing management disciplines generally have a vague image of 19th century physics; tidy phenomena, precise definitions, controlled experiments, replication and causal relationships. If MIS is about design, delivery and use in organizations, it will always be a little messy, and very applied. The question then is "which science?" This translates to "which reference discipline(s)?" This in turn translated to "which definition of information?" Once we answer that latter question, we have a "scientific" base for MIS. Until we do so, the field will be entirely driven by applications and technology and any theory is likely to be mere theorizing.

4. A CUMULATIVE TRADITION

Name the top ten people in MIS research. Name the ten most important articles in MIS over the past five years. Name the five or ten main themes in current research. In most disciplines, responses to these requests trip quickly off the tongue. They do not for MIS.

The problem is not necessarily one of quality (though let us be honest and admit it may be). There is no cumulative approach in MIS. Whereas in an established field like microeconomics, one begins a paper by saying "the x problem has been discussed by A, B, C, D, and E", in MIS we spend the first page explaining and justifying our topic. At tenure time, we have to do the same thing, except that 12 pages are needed. Many people in other fields are genuinely puzzled by us. They ask "what do you people in MIS do?" When we tell them they are even more puzzled, because our explanations do not indicate a core set of issues, ideas and methods.

A cumulative tradition is one where:

1. Researchers build on each other's and their own previous work;

2. Definitions, topics and concepts are shared;

3. Senior researchers view their main role as shaping the field;

4. Each journal in the field has a clear focus;

5. There is some definition of orthodoxy, while unorthodoxy is not discouraged.

For a variety of reasons, such a tradition has not emerged in MIS:

1. The field has been driven by changes in technology, rather than by issues of management, information and systems that are independent of specific technologies;

2. There are too many interesting new research opportunities;

3. It requires a huge effort to get something publishable in a hostile prestigious journal, but zero effort to do so for a journal mainly read by practitioners: whenever there is a new hot topic, a good MIS researcher has something interesting and relevant to say;

4. Few of the academics who rose to the top of MIS in the early 70's have stayed with the field or their original topic;

5. There are many temptations for some of the people of most potential to leave universities;

6. Several excellent consulting companies have been started by rising MIS academics who often continue to shape MIS concepts and practice as much as they did as "researchers". The lack of

a mesh between MIS research and the journals discourages, penalizes and sidetracks us too often. The idea that "good" work will be accepted is not true when:

a. We in MIS do not have a clear idea of what "good" means;

b. The evaluators view good in terms of well-defined themes and methods (e.g., OR);

c. There is no continuity in the field, so that "good" has to be defined and assessed anew for every piece of work.

There is as yet no clear MIS research network. It is emerging and this conference is an explicit step towards building it. It seems absolutely essential that the main aim for the MIS network be to establish a cumulative tradition. This means:

1. As individuals, we have a responsibility to try and link our work, most obviously by staying with our main themes, clarifying our reference disciplines and, wherever possible, starting from where someone else left off;

2. As a network, we must produce a published statement of what we feel the field is that corresponds in spirit though not in detail to the MIS curriculum Cougar and others published in ACM in 1972. It should be something that ensures that the evaluation criteria for tenure and publication are ones _we_ choose. At present, they are someone else's, usually in a "hard" analytic field. We have a political problem in trying to justify application-based, broad, exploratory work at a time when many of the leading schools are becoming intolerant, narrow and chasing after "science". We need a political statement, so that when asked, "what is MIS and why is the research so bad?", we can at least say "this is what people in it view it as, and _these are the criteria they feel should be used to evaluate it._" The proceedings of this conference is the first such statement of our field.

We have to recognize that it will be hard to make MIS "respectable". The current climate in academia is not receptive to the hybrid fields (e.g., Business Policy, Organizaional Behavior) and political power is in the hands of the tough guys in many universities. MIS is interested in affecting the practice of management; that is somewhat unfashionable. In many universities researchers in finance, accounting and management science have pushed themselves to an extreme of isolation from practice that may make it hard ever to recover diversity and excitement.

For many of us in MIS, respectability is not worth buying at such a price.

5. THE RELATIONSHIP OF MIS TO TECHNOLOGY

No one should be involved in MIS research who is not a craftsman in some aspect of computer technology and techniques. Occasionally, real behavioral scientists wander in to MIS and apply their knowledge to it. Generally, this fails to have any impact, because they do not understand the technology. While no one can know the computer field in detail, an MIS researcher has to have a craftsman's feel for the technology and sense what is easy and what is hard, what the core technology is, what emerging trends imply, etc.

That said, the craftsman's sense is necessary but insufficient. The _mainstream_ of MIS seems no longer _technical_ but organizational, managerial and behavioral. Virtually every research topic in this fragmented field touches on non-technical concerns; the MIS issues for telecommunications, microcomputers, office automation, and database are psychological, organizational, political and social.

The idea of viewing a hybrid field like MIS in relation to reference disciplines can be extended and helps clarify the necessary relationship between MIS technology. Computer science, artificial intelligence and electrical engineering are important reference disciplines. The reference discipline, however, should link _into_ the core MIS field but not _be_ the _core_. This is as true for computer science as for cognitive psychology. It is pointless to do mediocre psychological research under the MIS banner instead of doing good work in a real psychology department. Similarly, a computer scientist should do work in computer science. There has to be a special reason for entering the MIS field. Work in MIS which could just as easily be done in the reference area should be.

The definition of MIS research as concerned with the effective design, delivery, and use of information systems in organizations leaves plenty of room for technology, behavioral science, economics, accounting, etc. The test we need to make for any piece of research is does it relate to "effective(ness)... in organizations?" Too much of the work on aspects of database, programming techniques, artificial intelligence and operating systems is not MIS based on this test. Equally, much of it is. In either instance, "good" work requires a solid training and competence in the reference discipline; MIS must not be a refuge for those who cannot make it in the other field. "Good" MIS work goes beyond the reference area.

6. THE RELATIONSHIP BETWEEN MIS RESEARCH AND PRACTICE

Mis faculty are all in business schools. So, too, are management scientists and accountants; some of these, however, assume they are in different company. To want to be "scientists" may be a praiseworthy goal, but if this involves an explicit disdain for "application", "business", or "practice", one wonders about the sense or ethics of the researcher being at a business school. A lot of prestigious research done in business shcools has no possible link with the practice of management. It seems outrageous that applied MIS work should be a homeless orphan at a time when Management Science publishes articles on optimal doubling rules in backgammon.

There is a clear conflict between respectability in academia and relevance to the practice of management. If we take the M in MIS seriously, we may never be respectable. MIS may end up as an applied teaching field, not a research one. This is basically a political issue.

The personal links between academics and practitioners are stranger in MIS than almost any other discipline. We have a high consulting-to-university time ratio. More importantly, a lot of our knowledge comes from field experiences, whether case studies, consulting or conferences which include a good balance between practitioners and researchers. In some instances, practice is way ahead of research (e.g., in Decision Support Systems, telecommunications, and structured design and anlysis techniques). Most of us probably find the relationship with the Real World healthy, interesting, and informative.

It is also, however, blurred. At times it is not clear whether a given academic in MIS is a researcher or a consultant (or even a programmer). A solution to this (enjoyable) dilemma is to try to ensure that all one's work involves research-with-practice. The research should point towards practice. The practice should point towards research. For example, teaching very senior executives (practice) can explicitly help clarify the strategic aspects of MIS (research). We have tended to focus on tactical issues in systems development and define the "user" too low in the organization and to focus on hardware/software/implementation issues centered around specific projects. The lack of a strategic focus on, for example, the dynamic link between the business plan and technology plan or the dominating importance of telecommunications for MIS strategy, largely reflects the isolation of assistant professors trained as electrical engineers from senior managers. This limits MIS research immensely. Where would finance be as a field if it had defined the user as the supervisor in accounts receivable and the practitioner as the junior staff analyst?

Even though MIS has to clarify its theoretical base and focus on reference disciplines, the world of practice is central not peripheral. We need to keep very direct links with practitioners. Research-with-practice is key. What this really means can be determined only if we have a clear definition and cumulative tradition. At present, the world of respectable research is too divorced from practice to help shape MIS, and the research issues arising from practice are unstated.

7. WHERE TO PUBLISH

Here again, we have a political problem -- or set of problems. They may be unresolvable. They are stated, badly, below:

1. Interfaces, Sloan Management Review and similar journals in which some of the better applied MIS work has been published count negatively in the publish-and-perish tenure game.

2. While ACM is reasonably receptive, it focuses on a limited set of reference disciplines.

3. MIS Quarterly has minimum prestige and circulation. To change this, we rather than the editors, have to do something. A check at several major universities showed that none of the libraries and few professors subscribe.

4. It is assumed by many in the MIS network that it is a waste of time to submit anything to the Management Science MIS department.

5. There are lots of other journals where MIS work can be published. Most of

these are new (Human Systems Management, Social Issues in Computing, Information and Management, Technology and Human Affairs). They are thus fragmented and not well-known and thus do not help build a cohesive, cumulative tradition. (Since the first draft of this paper was written, Technology and Human Affairs collapsed before its first issue).

Politically, two problems seem to be:

1. Should we try to make MIS Quarterly the central, prestigious outlet?
2. Can we influence the Management Science MIS Department.

8. CONCLUSIONS

This position paper is obviously only a set of private opinions. The core is the list of questions stated at the start:

1. What are the reference disciplines?
2. What is the dependent variable?
3. How do we build a cumulative tradition?
4. What is the relationship between MIS and computer technology?
5. What is the relationship with practice?
6. Where do we publish?

The answers given are clearly not the only possible ones. It is easy to be negative about the current state of MIS research and at the same time happy and excited in doing it. Research should be fun rather than a grind and one should believe in its relevance and value. It is not inconsistent to then admit that as an academic field, we look mediocre at best.

It may be useful to end this paper by briefly discussing the research strategy we are developing at CISR. Many of the ideas expressed here were stimulated by our efforts over the past years to face up to the issue of evaluating and integrating the work of a coterie of technical, managerial and behavioral researchers. The central question we asked was: is CISR just a funding umbrella for useful individual projects or is there a central direction that adds up to a coherent concept of MIS?

Our resulting definition of MIS is, as stated:

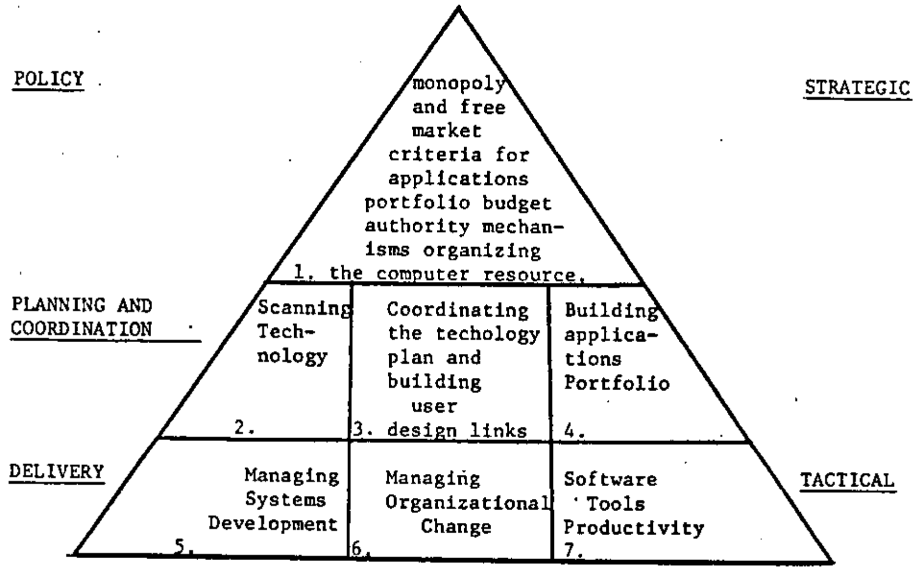
<u>the effective design, delivery and use of information systems in organizations</u>

The words "effective" and "organizations" are key. Like most practitioners and other MIS groups we have been operating with too informal a definition of "effectiveness": we know what it is when we see it. We have shifted a significant part of our attention to the dependent variable:

1. At the theoretical level, by focusing on what we can learn from the axioms and analytic methods of information economics and accounting.
2. Methodologically, by adding an experimental expertise we have hitherto lacked and by shifting the emphasis in our work on Decision Support Systems towards measurement of qualitative benefits.
3. Overall, by sharpening our concepts of productivity in MIS (not of MIS):
   a. Productivity of users.
   b. Productivity of tools.
   c. Productivity of systems development units and staff.

Increasingly, the focus on "effective... organizations" is shifting our emphasis from tactical issues of how to deliver specific systems to planning and policy. MIS has been a remarkably tactical field. Perhaps because in the early 1970's it was so difficult to get anything to work, studies of systems development and implementation mainly looked at single projects and technical research at program and coding and testing. As a result, we now have a sound understanding of the delivery issue, but organizations are struggling for guidance on:

1. Planning and coordination: how to:
   a. Scan emerging technologies from an applications perspective.
   b. Build a tradition of management in the information systems area.
   c. Set up mechanisms for selecting and integrating projects, to ensure a coherent portfolio instead of a set of independent go/no go choices
   d. Develop a systematic methodology for evaluating proposals that recognizes and measures nonquantitative "value-added" benefits.



2. Policy choices

   a. A monopoly versus a free market; regulating the market approach to using the computer resources, to balance integration with innovation and local needs

   b. Organizational design

   c. Defining the criteria for the applications portfolio: these are critical success factors (CFS).

   d. A methodology, rather than a set of buzz words, for resolving the trade-offs between centralization and decentralization of computing.

   e. Technical managerial and organizational issues relevant to common systems.

What we are calling the management system for using computers is shown below. (It is not a framework, merely a format for explanation and introspection).

Major components of our portfolio are listed below. The numbers refer to the relevant cell in the diagram above. Those that are starred are projects largely or entirely stimulated by our joint thinking about the portfolio as a whole and the need to sharpen definitions and fill in gaps:

1. Critical Success Factors - 1.

2. End user needs survey - 1,4,5,7.

3. Value analysis: evaluating the impacts of and criteria for designing decision support systems - 1,4*.

4. Measuring software quality - 7*.

5. The impact of APL end user programming in a free market - 1.

6. Budgeting and accounting for the computer resource - 1*.

7. Evaluating the impact of graphics - 2,4*. (These all relate to the dependent variable).

8. Structured design techniques - 5,7.

9. Political issues in implementation - 3,6.

10. Microprocessor networks - 2.

11. Organizational issues in office automation - 3,6.

Our definition of MIS was explicitly developed to facilitate a cumulative approach. The irritating old disdain of "technicians" for "behavioral science" has largely (partly?) disappeared at CISR. Almost all doctoral students are actively involved in work with a managerial, organizational or behavioral component and we include real, unrepentant behavioral scientists. That said, we are only gradually developing an adequate conceptual base and our methodological base is weak. We lack a solid experimental expertise and are working hard on clarifying how to do case-based research. In this last instance, the major steps forward came from recognizing that for implementation research, a good reference discipline is political science, in which structured case studies are a main methodology.

The detailed contents of our individual research projects remain differentiated; there is still a behavioral, a managerial and a technical camp. Given the diversity of MIS issues, of computer technology, of applications, and of organizaions, it would be impossible to identify a narrow set of topics that constitute MIS research. The key conclusion of this paper is that MIS is definable only in terms of:

1. A meaningful concept of information and effectiveness.

2. Reference disciplines.

3. A network that builds, legitimizes, and gives political clout to MIS research.

Without these, MIS research will be seen as weak, fragmented and unfocused, however narrow its topics. With them, we can continue to be broad in content but gain some degree of coherence and synthesis. Whatever we do in this conference, let us not argue about the specific content of MIS research. The six questions posed in this position paper constitute an agenda. If we can agree on the general answers to them, we can build an MIS research community.

9. ADDENDUM: BEYOND RESEARCH

Building a rich, meaningful field of study involves more than just "doing" research. American academia is remarkably anti-intellectual in many ways. It rewards narrow positivism and discourages breadth. If teaching drives out research, research drives out reading. Publications seem often valued for the abominable quality of thinking. To label academics as "conceptualizers" is to damn them (as if MIS had no need of concepts).

This paper is written by someone who is not an orthodox researcher. My reference discipline in the end is history. My role is more that of a scholar and, mutter it quietly, a conceptualizer. There is room for such an individual in MIS. There is a need for reflection on the field, its roots, relations with other disciplines and historical context.

There is need too, for those whose reference discipline is philosphy. MIS is an immature field and while obviously its maturation depends on good research, it must not isolate itself from the work of ideas for ideas' sake. Too many researchers in MIS (and even more in management science and economics) lack curiosity. They rarely review their own axioms or actively search for new sources of concepts and methods. They read little outside their immediate area of specialization and then only publications dated post-1977.

Every scientific field has a sense of history. It atrophies if it cuts itself off from curiosity, diversity and reflection. Many of our colleagues in other fields are frankly bored with what they do and a little worried that their work is partly making the emperor some new clothes. Most of us have chosen to work in MIS because it is enjoyable and relevant to some wider concerns. Let us make sure we keep a few philosophers, historians, general systems theorists and social activists within our network; even if only to write useful survey papers. Research is the profesional core of a discipline, but for it also to be the intellectual core, we need to think about research, not just do it.