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In "The Structure of Scientific Revolutions," Kuhn (1970) identifies two mechanisms which govern the process of scientific progress: (i) *Normal Science* -- a continuous, cumulative process which builds on and refines a central paradigm; and (ii) *Scientific Revolutions*, which are periods of turbulent and discrete paradigm shift. While some of Kuhn's critics viewed his reference to the development of normal science as somewhat derogatory and researchers often emphasize the revolutionary mode in the development of science (especially in reference to their own work), Kuhn himself makes his appreciation of normal science clear and unequivocal. In Kuhn's view, normal science is a most efficient mode of achieving scientific progress because the scientist is adhering to a set of values and standards that are accepted by the profession, because he can avoid endless arguments about fundamental premises which are embodied in the prevailing paradigm, and because he can concentrate on obtaining results for focussed and well-defined research projects. Scientific revolutions are important since they enable the consolidation of better paradigms for continuing the progress of normal science.

Kuhn's analysis puts a special emphasis on the mechanisms that create a scientific profession. A key mechanism in the development of MIS is the International Conference on Information Systems (ICIS), which has set the stage for the evolution of an MIS research community. The concerns raised in the first ICIS, exemplified by Keen's (1980) statement that "At present, MIS research is a theme rather than a substantive field" (p. 9), suggest that MIS had not reached the state of a normal science as defined by Kuhn. These concerns played an important role in the subsequent development of MIS and in the reliance on relevant reference disciplines.

Kuhn's description of physical optics before Newton seems particularly relevant. Kuhn (1970) writes:

> Those men were scientists. Yet, the net result of their activity was something less than science. Being able to take no common body of belief for granted, each writer felt forced to build his field anew from its foundations. In doing so, his choice of supporting observation and experiment was relatively free, for there was no standard set of methods or of phenomena that every optical writer felt forced to employ and explain. Under these circumstances, the dialogue of the resulting books was often directed as much to the members of other schools. [p. 13]

Further, argues Kuhn, fact-gathering which is not guided by a central paradigm

> ...is a far more nearly random activity than the one that subsequent scientific development makes familiar....The resulting pool of facts contains those accessible to casual observation and experiment, together with some of the more esoteric data retrievable from established crafts....Only very occasionally do facts collected with so little guidance from pre-established theory speak with sufficient clarity. [pp. 15-16]

These statements are echoed by numerous comments documented in the 1980 ICIS *Proceedings*, from Keen's complaint on the "framework-of-the-month," ad-hoc theorizing and the lack of a cumulative research tradition through Turner's criticism of research designs to Dickson, Benbasat and King's (1980) statement that "We seem to randomly generate research projects with the outcome that we have a scattering of results which presents a severe problem of pattern recognition" (p. 4) and their call for more rigorous standards and theories in the area. So, in 1980, MIS was not a "normal science" by Kuhn's definition: either it was not a science at all or perhaps it was an abnormal science.

The clear consensus of the 1980 ICIS participants was that MIS should seek guidance from the established reference disciplines. This is consistent with Kuhn's suggestion that coherent paradigms are necessary for scientific progress; Kuhn actually asserts that new fields may create their paradigms by building on more established ones. But there is a key difference between Kuhn's views and the approach actually adopted by the MIS community. Kuhn sees scientific progress as sequential process, characterized by long periods over which the profession adheres to a single paradigm with rare (but important) discrete jumps to new paradigms that subsume the old ones. In contrast, MIS has developed with a strong emphasis on parallel paradigms. The multi-disciplinary scope of MIS research is one of the cornerstones of the area. This parallelism is best demonstrated by the very structure of the first ICIS (as well as subsequent ones) which recognized that MIS is inherently inter-disciplinary and that is it important to build on a variety of reference disciplines that would support MIS as a research theme.

Kuhn would suggest that this is but a transitory phase of the paradigm period:

> Before it [the transition from the pre- to the post-paradigm period] occurs, a number of schools compete for the domination of a given field. Afterward, in the wake of some notable scientific achievement, the number of schools is greatly reduced, ordinarily to one, and a more efficient mode of scientific practice begins. [p. 178]

Further, according to Kuhn, even researchers in the same narrow research area use different vocabularies before and after the paradigm change, and their whole world views will be different:

> Practicing in different worlds, the two groups of scientists see different things when they look from the same point in the same direction...they see different things, and they see them in different relations one to the other...before they can hope to communicate fully, one group or the other group must experience the conversion that we have been calling a paradigm shift. [p. 150]

Recalling that Kuhn is talking about scientists practicing in the same narrow field, this would certainly imply that the MIS researcher who uses microeconomics as a reference discipline cannot communicate effectively with the behavioral scientist, wouldn't it? One of the objectives of this panel is to subject Kuhn's hypothesis -- as it pertains to the MIS domain -- to an empirical test. A substantive question which arises is whether the parallel-reference-discipline mode of MIS research is sustainable. Another question is whether it is desirable: do the benefits of cross-fertilization exceed the costs of communication?

What is a reference discipline? It is a mechanism which allows applied fields to become normal sciences by building on an existing paradigm. Some people resist this idea, arguing that it imposes unnecessary constraints and hampers creativity. But it is important to recall from our previous discussion that such constraints are the operational essence of normal science. In fact, the MIS researcher will necessarily have to exercise greater creativity and caution than the "pure" followers of the reference paradigm, reshaping it to accommodate relevant MIS issues. The result is a sub-paradigm which builds on the original one but modifies and refines it in meaningful ways to make it relevant to MIS. The reference discipline provides a core of shared values, research methodologies, techniques and quality-control mechanisms that the MIS researcher can draw on. The craftsmanship of the research will be judged by

the quality criteria of the reference discipline, whereas its ultimate value will depend on its contribution to the MIS domain.

Balancing the relevance (or importance) to the application domain against the quality of execution, evaluated using the criteria of the reference discipline, is a constant source of strain due to the monitoring and quality-control role of that discipline. This role should not be taken lightly (especially by those who are not yet tenured). Most business schools engage in a process of mutual monitoring across areas. A non-MIS researcher who evaluates an MIS piece may not be able to appreciate the importance and relevance of the issue studied and its contribution to MIS, but will quickly detect a sloppy application of the research methodology. The result is that the craftsmanship of execution -- measured through the criteria of the reference discipline -- becomes a critical necessary condition for the development of an area such as MIS. The established areas challenge the new kid on the block to prove its quality and relevance using their own criteria rather than those developed within MIS.

The panel assembled here represents an important subset of reference disciplines relevant to MIS research. The tradeoff between depth and breadth called for a limit of three panelists. Each panel member will first discuss the importance and contribution of his or her reference discipline to MIS research. After these position statements, a common research theme will be introduced to the three panelists and each will indicate how this theme would be addressed from the viewpoint of the specific reference discipline. Then, panel members will challenge their competitors' approaches to the problem, followed by an open discussion. Hopefully, the winner will be MIS rather than any of the competing reference disciplines.

The common theme to be addressed by all three panelists will be "improving software development productivity." It is clear from the foregoing discussion that each reference discipline's viewpoint will necessarily lead to different research questions (and certainly different answers) despite the fact that they are all looking at the same issue. This is why the topic has not been refined to the level of a specific research question -- a task which must be left to the specific paradigms. Each panelist will outline the research questions which could (and should) be addressed by his or her paradigm, the approach to studying them and the expected contribution of the results. The discussion should not be a review of completed research, but a research agenda which will highlight what the reference discipline has to offer to MIS through this concrete example. To help focus the discussion on the roles of the competing disciplines, the importance of the chosen issue to MIS research and practice can be taken for granted -- it is widely accepted, as evidenced by the frequent references to the "software bottleneck." As one measure of the value of improving software-development productivity, Gurbaxani and Mendelson (1987) estimate that the net present value of a gradual increase in software-development productivity from 5% to 10% over a 25-year period to user organizations in the United States is $335 billion, and to hardware vendors -- $177 billion. What fraction of this value can each of the competing reference disciplines be entitled to (if any)?

The panel participants are Gad Ariav from the Graduate School of Business at New York University, who will present the role of Information Technology research; Gerardine DeSanctis from the Carlson School of Management at the University of Minnesota, who will present the role of the behavioral sciences in MIS research; and Jeffrey Moore from the Graduate School of Business at Stanford University, who will discuss the role of Economics in Information Systems research.

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