EDITORIAL
Information Technology Research in Accounting

In the Spring 1990 issue of *JIS*, I outlined the editorial guidelines for the journal with some specific attention being paid to the relationship of various cognate domains to the fields of both accounting and management information systems. Four areas in particular were discussed: (1) management science, (2) behavioral and organization science, (3) economics, and (4) computer science. Readers can note that the first three of these are domains whose research paradigms and successes have greatly affected the "non-systems" areas of accounting (like taxation, financial reporting, management decision making, and auditing). Their research traditions, therefore, are most commonly understood by our accounting colleagues. Such is not yet the case for the fourth domain of computer science, and work in this area remains sadly behind in terms of its shared understanding among those we work with in accounting departments. Unfortunately, one of the most obvious manifestations of that failing is a blanket denunciation sometimes heard of information technology workmanship in accounting as more tinkering or development than scholarship or research. Such a state of misunderstanding is often a cause, I believe, of skilled people eschewing work in this arena in favor of research programs that feature more traditional and safer projects. This migration sometimes occurs in spite of the fact that a particular scholar's interests, background, and (most dysfunctionally) teaching responsibilities might have ideally equipped him or her for high quality scholarship in information technology areas.

When one **looks** at computer science, it is hard to understand how its research traditions and its solid record of scientific progress over thirty years can fail to impress accounting researchers who should logically be eager to adapt its methods to the tasks of building better accounting systems. Why hasn't it happened? The reason in my opinion is precisely because those researchers **haven't looked**. They are complete neophytes and content to stay that way — a state of affairs that is understandable when one looks at the nature of the two disciplines. Computer science departments have theoretical (especially mathematical) underpinnings, but they are often associated most strongly with engineering research traditions that emphasize the practice of building better things or building things better, an inherently normative mind-set strongly dissonant with the overwhelmingly positive mores of the modern accounting publication establishment. I personally don't consider this mainstream accounting fixation with its own ideas particularly damaging to the cause of information technology research in accounting, unless it reaches the point where the blanket denunciations mentioned above stop work. Unfortunately, I think we may be perilously close to that point in academic accounting. If we ever get there, the tasks of building better accounting systems will eventually be left exclusively to non-accounting researchers or to forward thinking practitioners.

The situation as I have described it in the accounting research establishment reminds me strongly of a misconception that artificial intelligence theorists used to labor under many years ago. Once upon a time in AI, scientists pursued the notion of intelligence exclusively as generalized problem solving ability or abstract expertise. This was intelligence that could be applied in any domain to unravel complexity and move quickly toward a solution to a particular question or difficulty. The underlying theme in such a pursuit was that intelligence was a general phenomena that transfers effectively from one area of problem solving to others. More specifically, we could expect that an expert physician would be an expert chess player, an expert bridge player, an expert stock market analyst, etc., because the high level of intellect needed in certain types of diagnostic medical matters could be applied with similar results to picking move strategies or successful portfolios. This theme has a strong intuitive appeal as many of us, I am sure, would be surprised to find a renowned physician behaving quite stupidly in some kind of an intelligent endeavor, especially if it appeared to us that the doctor in question was quite confident of his or her ability to produce successful answers to problems. The ascendancy of knowledge-rich systems in more recent AI research has signaled a retreat (although not a complete one) from this position. Expertise is now seen to accrue much more to specific domain knowledge or to a specific collection of past experiences in the particular domain than to general problem-solving abilities. Good chess players are not necessarily brilliant intellects; they are people who study compendiums of chess moves and who play chess a lot.

So how do these theories of generalized intelligence and domain-specific expertise relate to accounting researchers and to information technology research? There is a dysfunctional tendency in academic accounting, I believe, to attribute generalized expertise to renowned researchers. For example, if a "JEOPARDY" tournament (or some similar non-accounting intellectual endeavor) were to be held at the annual AAA meeting, I suspect that there would be a substantial number of people who would assume automatically that the quarter-finalist group of 15 people would overlap heavily with the editorial review boards of the leading journals. In actuality, this would not be the case; however, the aura of the "best and brightest" would affect the expectations of many. More specific to the domain of *JIS*, there is no reason to heed disproportionately the counsel of capital markets or agency theory experts when one is pondering a research program in information technology. These people might be able to advise you on the strategic game-playing implications of choosing that research subfield ("Nobody who does that stuff succeeds"), but they cannot render a valid opinion on its substantive efficacy. They usually don't know enough computer science to understand (more simply, they just don't get it). It seems fortunate that most editors and tenure reviewers realize this and rely more on the judgments of others than on their own intuition, but there are still many who will not hesitate to demean work they don't understand. In that sense, they are like doctors who go to an office poker party expecting to clean out the nurses, clerks, and clinicians. The trouble in academics is that there is no embarrassing end to such delusions, although I suspect we could approximate such an awakening by ruse if we could induce these people to review blindly good computer science work. It would be interesting, for example, to see how many judgments of "no substantial scientific merit" would be given by well-known accounting researchers to pieces like Codd's seminal 1970 paper in *Communications of the ACM.*

In summary, my message with regard to information technology research in accounting is that we shouldn't let the opinions of those outside of the AIS research community disproportionally affect our agenda. Building better systems or building systems better are inherently normative activities, and we shouldn't apologize if that is where we believe our interests and natural research advantages lie. There are certainly valid reasons for shying away from information technology research in accounting (the learning curves are too steep, there are still too many tinkerers in the field who give it a bad name, the systems require disproportionate amounts of resources for single researchers or small teams, etc.). However, when I think about the difference between what is needed and what is being done, I attribute much of that gap to discouraging opinions of systems research voiced by people who are really not qualified to make such judgments.

W.E. McCarthy. 1990. Journal of Information Systems (Fall 1990), pp. iii-iv.