Communications of the Association for Information Systems

Volume 5 Article 12

January 2001

Diversity and Scientific Progress in the Information Systems Discipline

Ali Farhoomand
The University of Hong Kong, ali@business.hku.hk

Don H. Drury
McGill University, drury@management.mcgill.ca

Communications of the Association for Information Systems
Volume 5, Article 12
May 2001

DIVERSITY AND SCIENTIFIC PROGRESS IN THE INFORMATION SYSTEMS DISCIPLINE

Ali F. Farhoomand
School of Business
The University of Hong Kong

Don H. Drury
Faculty of Management
McGill University

ali@business.hku.hk

PROFESSIONAL

DIVERSITY AND SCIENTIFIC PROGRESS IN THE INFORMATION SYSTEMS DISCIPLINE

Ali F. Farhoomand
School of Business
University of Hong Kong

Don H. Drury
Faculty of Management
McGill University

ali@business.hku.hk

ABSTRACT

The Information Systems (IS) discipline is over a third of a century old. It is a multidisciplinary field of study that covers areas related to the management, deployment, and use of information technology. In response to this extended reach and the growing needs and requirements of its stakeholders, the IS community successfully solidified its foundations through institutionalization and professionalization. However, in light of a complex patronage structure, undisciplined diversity, and unbounded eclecticism in scholarly activities, the progress of IS as a scientific discipline has been attenuated. Drawing lessons from the field of psychology, this paper calls for solidifying the disciplinary matrix of IS. It argues that scientific progress of IS can be advanced further through the development of cumulative and exemplary theories aimed at significant problems. Such a cumulative approach to research tradition and knowledge unification would help demarcate the boundaries of the IS domain not in terms of its subject matters, but by the theories it develops to solve the problems within its domain.

KEYWORDS: Information systems discipline, diversity, progress, philosophy of science.

I. INTRODUCTION

Scientific progress in IS - or lack thereof - received ample attention in the course of the discipline's evolution. In a provocative and highly cited article, Keen [1980] called for development of a cumulative research tradition in the IS field. Since then many researchers examined various aspects of IS scientific evolution. Culnan [1986], Culnan and Swanson [1986], Culnan [1987] and Holsapple et al. [1993] discussed the underlying foundations of the discipline. Structural development and scientific progress of the field were covered by Farhoomand [1987], Weber [1987], Banville and Landry [1989], Teng and Galletta [1991], Alavi and Carlson [1992], Cheon et al. [1993], Hirschhiem et al. [1996], Mingers and Stowell [1997], and Weber [1997]. In an engaging discourse, Benbasat and Weber [1996] and Robey [1996] introspected about the ramifications of research diversity in IS. More recently, Farhoomand and Drury [1999] pointed out that questions concerning the scientific status of IS are instrumental in demarcating the boundaries of the field and ultimately shaping its foundations as a viable scientific discipline. Orlikowski and Iacono [2001] argued that IS researchers have not taken information technology (IT) as seriously as its effects, context, and capabilities, and subsequently called for development of specific theories about IT artifacts.

The objective of this paper is to provide an epistemological insight into the nature of scientific progress to engage the academic community in a debate regarding the ways in which the field can further entrench its position as a scientific discipline. In Section II, we discuss the meaning of scientific progress. Then through a historiographical examination of psychology we highlight some of the epistemological and methodological challenges awaiting the IS community (Section III). Finally, we present a synthesis of the preceding sections to stimulate an epistemological discourse about the current and future scientific status of IS.

II. WHAT IS SCIENTIFIC PROGRESS?

> *We must explain why science - our surest example of sound knowledge - progresses as it does, and we first must find out how, in fact, it does progress.* [Kuhn, 1970b, p. 20]

Students of the philosophy of science soon learn that one of the most influential works in the field is Thomas Kuhn's classic *The Structure of Scientific Revolution* [Kuhn, 1962, 1970b]. Kuhn's influence stems primarily from his idea that science grows through discontinuous, paradigmatic shifts. The scientific community recognizes anomalies in the field, becomes increasingly disenchanted with the existing framework, searches for new alternatives, and finally accepts one of the competing schools of thought as the dominating paradigm. In other words, scientific discovery starts with the awareness of anomaly - the observational and conceptual recognition by the scientific community that expectations based on prior paradigms have been violated - continues with the exploration of the area of anomaly, and closes only when the paradigm theory is adjusted so that the anomalous becomes the expected [Kuhn, 1970b, pp. 52-53].

Kuhn cuts an incisive argument when he deals with the problem of scientific progress.

> *To a very great extent the term 'science' is reserved for fields that do progress in obvious ways. Nowhere does this show more clearly in the recurrent debates about whether one or another of the contemporary social sciences is really a science.* [Kuhn, 1970b, p. 160]

Kuhn makes it clear that science and progress are inextricably connected. As Chalmers [1999] has noted, the purpose of normal science is to solve puzzles or problems that are within the paradigm. For the members of a mature scientific community who normally work within a single paradigm or from a closely related set, the result of successful creative work is progress. During the pre-paradigm periods, when more than one school of thought competes for domination, evidence of progress, except within the schools, is hard to find. It is only during periods of a normal science that progress seems both obvious and assured. Progress of the normal-scientific community is far easier to see in the absence at most times of competing schools that question each other's aims and standards (p. 163).

As Trouvé [1992] pointed out, scientific evolution is characterized by interplay between atomization and unification of knowledge. On the one hand, lack of consensus during normal science periods leads to atomize knowledge, generating a multiplicity of contents and diversity of the scientific community. On the other hand, consensus leads to solutions to problems, the continuity and unity of knowledge, and the collective dimension of knowledge. A certain degree of atomicity (dispersion) and of unity is a constitutive feature of scientific knowledge. The former relates to the microscopic structural level (breadth) of scientific progress, while the latter relates to its macroscopic structural level (depth). Atomicity and unity are interdependent and exist simultaneously. As such, both are necessary for advancement of knowledge. Atomization of knowledge describes an increase in diversity and flexibility, and unification of knowledge a decrease in diversity and flexibility of knowledge. In other words, the average contribution of individuals to the growth of knowledge decreases with increased diversity and is positively related to convergence.

Popper [1975] maintained that a new theory must always be able to explain fully the success of its predecessors. Similarly Lakatos [1970] regards a set of theories as progressive only when each later member in the series entails all the corroborated content of its predecessors. Kuhn [1970b] also sees progress, measured in terms of the number of problems solved, to be cumulative. "No creative school recognizes a category of work that is, on the one hand, a creative success, but it is not, on the other hand, a creative achievement of the group" (p. 162). By virtue of their shared training and experience, the scientific community is the most efficient instrument to set the rules of the game for unequivocal judgements of scientific advancement.

Paradoxically, few scientists will be persuaded to adopt a viewpoint that would challenge what was previously solved. Making prior achievements problematical undermines professional security. A new paradigm will be embraced by scientists only when it seems to resolve some outstanding and generally recognized problem and at the same time promises to preserve a relatively large part of the problem-solving ability of its predecessors. New paradigms narrow the scope of the community's professional concerns, increase the extent of specialization, and attenuate its communication with other groups. Using a Darwinian analogy, Kuhn maintains that:

> *The resolution of revolutions is the selection by conflict within the scientific community of the fittest way to practice future science… Successive stages in that developmental process are marked by an increase in articulation and specialization. And the entire process may have occurred, as we now suppose biological evolution did, without benefit of a set goal* (p. 172).

Kuhn assesses science's existence and its success in terms of evolution from the community's state of knowledge at any given time, irrespective of a set goal. Science makes progress by generating theories that are successively closer to the truth. Without the benefits of a permanent set goal, progress is measured in light of the aims of the agents (scientists) who accept or reject a certain theory to attain significant truth in terms of charting divisions and recognizing explanatory tendencies in nature [Kitcher, 1993, p. 156].

Pfeffer [1993], in his review of barriers to the advancement of organizational science, outlines a set of markers for progress for all disciplines. These criteria reflect scientific legitimacy and institutional legitimacy of a discipline. The scientific criteria are primarily concerned with building a focused, collaborative and cumulative research tradition, while the institutional criteria relate to such factors as level of resources allocated to a discipline and the level of power exercised by its members. Banville and Landry [1989], on the other hand, do not see progress as cumulation of research knowledge. In their coverage of the nature of the IS field, they maintain (p. 59), "… On the matter of progress… it is pragmatic success that cumulates in science, not necessarily the amount of knowledge." However, Banville and Landry fail to provide any clear indicators of progress.

III. SCIENTIFIC EVOLUTION: THE CASE OF PSYCHOLOGY

The rise of scientism, personal ambitions, and professionalism of higher education - and thus institutional pressures - at the turn of the century all had important impacts on the evolution of psychology as an academic discipline [Wilson, 1990]. The emergence of new journals and the subsequent establishment of the American Psychological Association (APA) in 1892 "symbolized the advance toward professionalization" [Camfield, 1985, p. 67]. As the number of doctorates in psychology exceeded the number of laboratories in the 1890s, many psychologists turned to practical application of psychology and to problems of education. Hand-in-hand with these developments, many leading psychologists continued to use different approaches to their work, adding fuel to the debate about the lack of unanimity regarding the scientific characteristics of the field. The ultimate focus of these controversies was "whether psychology could, or should, become an exact science, and whether it was to be devoted primarily to theoretical or to practical problem solving" [O'Donnell, 1985, p. 131].

Institutional aspects of professionalization, including a growing number of new journals, doctorates, and even the APA itself did not lead to confidence among psychologists as professionals right away. It was not until early 1900s that psychologists exhibited a relentless concern for full-scale development and for the stature of psychology as a science and profession. But efforts to achieve scientific stature were frustrated for many years due to psychologists' inability "to reach agreement among themselves as to definition of their field and its phenomena, or with regard to proper methods of investigation" [Camfield, 1985, p. 73]. Under attack not only by philosophers but also by some scientists (e.g., biologists), psychologists slowly realized that they needed to narrow their field of inquiry and explanation to win credibility as a science. Yerkes [1910] cited the lack of a generally acceptable body of presuppositions and postulates as one of the main reasons behind the unsatisfactory state of psychology. "What becomes clear in Yerkes's critique of the discipline is the lack of unanimity among the practitioners of psychology as to the state of the science, its characteristics if it were a science, or even the extent it could or should be a science" [Wilson, 1990, p. 116].

The early years of 1900s saw the advent of new schools of thought, the first of which was structuralism, introduced by one of the Americans trained in Germany, E.B. Titchener.[1] John Watson soon attacked this school of thought in his behaviorist revolt in 1913, which subsequently led to the general conceptualization of psychology's purpose and scope. The behaviorists protested against structuralists, both methodologically and conceptually, arguing that the only scientifically observable phenomena are behavioral responses.[2] Behaviorism led to legitimization of psychology as a field with practical utilization, and grounded in an experimental science. Even though behaviorism made a significant contribution to the growth of psychology, it was soon found to be restrictive because it excluded diverse phenomena and mechanisms, such as neurophysiology and cognitive processes [Wagner and Owen, 1992]. The turn of the last century also saw the advent of functionalism, a school of thought that also regarded structuralism as restrictive. Functionalism is regarded by some to be the overarching paradigm in psychology today [Whitley, 1992].

It is important to note that the development of psychology as a scientific discipline during its early periods was hindered by eclecticism. Not only was organizational identity needed to ward off threats from other disciplines, but also more importantly, there was and perhaps still is a need for theoretical consistency [O'Donnell, 1985]. Even today, after a 120-year history, there are still great concerns about the disciplinary status of psychology. The diversity of

---

[1] According to structuralists, the subject matter of psychology is limited to the structure and organisation of human consciousness. As such, the goal of psychology is to identify the elements of the mind.

[2] Behaviorists maintain that the shift from the study of consciousness to the study of behavior can be largely explained by viewing psychological thought within the context of a patterned system of social and intellectual relationships [O'Donnell, 1985, p. xi].

interests coupled with academic and professional affiliations exacerbated the situation to a point where research psychologist members of the APA created a new society, the American Psychological Society, in 1988 [Holden, 1988]. In the absence of a mature paradigm, some argue that a proliferation of conceptually unrelated topics led to the failure of the discipline to achieve its primary mission of explaining the mind. Miller [1985, p. 40-45] unabatedly referred to the status of psychology as an "intellectual zoo," fearing that it will be subsumed by its more established sister disciplines, sociology and biology. Similarly, Koch [1985, p. 938] pronounced pessimistically that "the disciplinary status of psychology… is, in one word, in doubt." Some show concerns that psychology will be Balkanized into a new family of disconnected disciplines, such as cognitive science and neuroscience. Others, in contrast, advocate such a move, calling for the conception of the "psychological studies," in which psychology "is not a single or coherent discipline but, rather, a collectivity of studies of varied cast, some of which may qualify as science while most *do not*" [Ibid, p. 942].

IV. DISCUSSION

Questioning the foundations of knowledge and their divisions into manageable, comprehensible parts, is part of the development of intellectual processes. The relations between and within fields of endeavor are as important to progress as the subject matter itself. The process of questioning roots, diversity of approaches, and stability of paradigms should be taken as a healthy activity from which structure emerges, is accepted, is questioned, is reinvented and reaches new levels of understanding. As demonstrated in Section III, fields other than IS have tried to address these issues to reach new levels. This introspection is now applied to the development of IS as a field of inquiry.

IS seems to have built the institutional aspects of professionalization successfully, as evidenced by, among other things, the recent amalgamation of the Association for Information Systems and the International Conference on IS in 2000. The field enjoys having several respected academic journals, a professional association of about 2000 members, numerous universities with doctorate degrees in the field, a close link with a burgeoning industry, and a healthy demand for its graduates [ITAA, 2001]. If the developments of the past few decades are any indication, IS should continue to entrench itself institutionally even further in the future.

As we argued in Farhoomand and Drury [1999], the IS field is at the heart of one of the last century's most significant accomplishment, with far-reaching and complex impacts. While worrying about scientific progress in the field incessantly is not warranted, we can learn from experiences of other disciplines. In the preceding section we saw that eclecticism was a major reason behind fragmentation in psychology. Although the arguments put forth by the proponents of diversity are intellectually appealing, in our view such diversity in the long run comes at the high cost of divisive fragmentation, continued reliance on reference disciplines, low level of conceptual coherence, and low or no barriers to entry [Fitzgerald and Adam, 1997]. Rather than helplessly accepting the unbounded eclecticism by treating IS as a "fragmented adhocracy," we concur with Benbasat and Weber (1996) and call for a moratorium on *theoretical* diversity. In this context, we make an important distinction between diversity in method and diversity in the theories used by researchers. A great deal of energy was expended on the debate regarding the supremacy of one methodology over another, something, which as Weber [1997] noted, is a straw-man debate. In contrast, we believe that theoretical diversity has overarching implication for IS progress. Trouvé [1992] points out that diversity is accompanied by flexibility in scientific discovery, and it may lead to the emergence of competing schools of thought that question each other's aims and standards. However, undisciplined theoretical diversity and eclecticism rupture the mosaic necessary to bond the scientific community and may eventually lead to balkanization of the discipline. Again, lessons learned from psychology and organizational science are illuminating.

Symptomatic of eclecticism in the IS discipline is the recurring charge that academic research in the field bears only slight relevance and is of little interest to the broader practitioner community. This matter was discussed at length in a recent special volume of the Communications of the Association for Information Systems [CAIS, 2001]. In spite of diverse opinions on this subject, the prevailing view is that we are often issue-driven, chasing practice or engaging in research that is neither here nor there in the eyes of industry. In addressing this circumstance, the eclectic background of academics comprising the research community, and their divergent perspectives, were cited as a contributing cause [Bhattacherjee, 2001]. IS research is conducted from the point of view of management practice, econometrics, social theory, and a bevy of other areas – all of which are commonly associated with various "reference disciplines." Consequently, most IS research is only relevant to a narrow constituency.

Questions of basic vs. applied research, relevant vs. rigorous and other such issues are unproductive when viewed plainly as dichotomous propositions. Mason [2001] argues for a two-dimensional view of research based on motivation, which leads to the use of good science in the pursuit of new understanding and practical solutions. This mindset allows for scientifically rigorous research to be conducted in addressing particular needs, with results that prove useful for practical application while expanding our body of knowledge. There is intrinsic value in scientific inquiry for its own sake, and there are some examples of far-reaching innovations stemming from IS research – but it is often years before any resulting applications are developed, and relevance is then assessed after the fact.

Dennis [2001] and Heart and Pliskin [2001] argue that to improve its overall contribution to society, the IS discipline should adopt a new model, akin to those of Medicine and Engineering, where academic efforts are applied toward advances in current practice. However, we find again that evolution of the discipline is ultimately hindered by identity confusion, a vast field of inquiry, and lack of consensus about the fundamental areas. To progress, the IS community must establish an agenda and identify the key problems to be addressed. We learned from psychology that the maturation of a scientific discipline is a lengthy process, but one that begins with a consideration of such fundamentals in light of the strengths and weaknesses of the field. This sentiment is echoed by IS academics who call for defining a collective research agenda [Amaravadi, 2001], clearer borders between IS and other disciplines [Ben-Menachem, 2001], marketing the core areas where IS excels [Chatterjee, 2001], and devising a set of basic research questions that lead to "more cohesive and integrated theories" [Rollier, 2001]. As companies enter the era of network interdependence, being subject to unpredictable interactions between various parts within and outside the organizational bounds, the need for distinct, integrative IS theories and concepts is becoming more evident than before. This issue was the subject of a debate at ICIS 2000 [Alter, 2001], where once again, questions of relevance, the focus of IS, and definition of core fundamentals were raised.

In establishing a cumulative tradition, we believe it is important to recognize and pay closer attention to those substantive theories that made a progressive contribution to our depth of knowledge and achieved a significant level of validation. Certain recent theoretical developments in narrowing down the breadth of IS research and establishing a focused and cumulative tradition are encouraging. For example, the application of the theory of reasoned action (TRA)[3] [Fishbein and Ajzen, 1975] in IS research is showing promising results in building a cumulative tradition. Through the extension of this theory, IS researchers were able to move towards the development of a coherent theory that could explain user behavior [Barki and Benbasat, 1995].

Since TRA was introduced in 1975, a stream of research appeared that aimed at testing the predictability of behavior based on analyses of intentions, as applied to a range of disciplines. The very general nature of TRA earned its approval as a basis for explaining almost every kind of human behavior. To this end, TRA was also applied to the study of IS as a tool in measuring overall satisfaction of users with IS products and services [Melone, 1990]; in assessing the predictability of user acceptance to new systems through measuring and explaining their intentions [Davis et al., 1989; Mathieson, 1991; Jackson et al., 1997]; in studying the importance of user participation in the formulation of a positive attitude towards new systems [Hartwick and Barki, 1994]; and in projecting that, since behavior is ultimately determined by beliefs, senior management may be persuaded to change their beliefs (through influencing the attitudinal and/or subjective norm determinants of intention) in favor of the adoption of new IS [Mykytyn and Harrison, 1993]. In general, the application of TRA to the study of IS concentrated on its usefulness in predicting and influencing user acceptance in advance of system introduction. For example, the technology acceptance model (TAM) developed by Davis [1986] and Davis et al. [1989] is specifically designed for studying computer usage behavior and provides a basis for analyzing the impact of external factors on internal attitudes and intentions.

In addition to TRA and its variants, a few other areas used a cumulative research tradition to develop theories that should help IS to solidify its theoretical depth. These concepts seem to have had significant impacts on cementing the foundations of the discipline and will ultimately play an essential role in the progress of IS as a discipline.

- *Systems theory* and its progeny, such as systems modeling techniques and soft systems theory, are showing promise in heightening our understanding of information systems and the way they *should* be designed, developed and implemented.
- The applications of *decision theory* to decision support systems and group support systems brought us to new levels of understanding about the way information systems *are* used by the end-users.
- *Structuration theory* applied to IS research enhanced our knowledge about the *relationships* between technology and organization. Further work in this area should help us explain the social implications of IS [Avgerou, 2000].

[3] TRA specifies the relationship between beliefs, attitudes and behaviors. The theory is founded on the proposition that an individual's behavior is determined by his intention in relation to that behavior

V. CONCLUSION

Progress is indistinguishable from science; for a science to exist it must progress [Kuhn, 1970a]. As Anderson [1983] and Weber [1997] pointed out sciences progress through a commitment to theory-driven programmatic research with a view to providing theoretical unity and coherence for a discipline. As such, collective efforts are needed to unify knowledge necessary for progress of IS as a scientific field of inquiry.

To achieve this goal, the complicated patronage structure of IS [King and Applegate, 1997] must be circumscribed. The diversity of research in IS has taken us to a point where it is increasingly difficult to distinguish IS from other disciplines [Benbasat and Weber, 1996]. This proposal, of course, is not a call for *disciplining* processes that govern and restrict the forms of knowledge that are accepted as legitimate [Foucault, 1982], and would, in turn, result in a "closed" discipline where the potential threats of becoming irrelevant are great [Robey, 1996]. Rather, it is a call for the consensual solidification of the disciplinary matrix [Kuhn, 1970b] of the field through the development of exemplary theories aimed at significant problems.

As Popper [1962, p. 67] noted, a discipline should be defined not by its subject matter, but by the theories it develops to solve the problems of its domain. In this article we adopted a realist, paradigmatic approach to the study of scientific progress to argue that science is cumulative and it is done with no set of fixed goals in mind. For IS to progress, we need to have competition among macro-structures that are shared by the scientific community, whose members in turn are the most efficient instruments to judge progress. These theories must account for the way all the branches of the field can fit together and how new branches emerging in the field can be integrated into the old ones [Trouvé, 1992]. In the absence of such theories, IS would remain a fragmented adhocracy, with few theories of its own that distinguish it as a distinctive field of study, possibly leading to perpetual identity crisis experienced by some other disciplines.

Editor's Note: This article was fully peer reviewed. It was received on January 21, 2001 and was published on May 10, 2001. It was with the authors for approximately 1 month for 1 revision.

ACKNOWLEDGEMENTS

This paper benefited from the comments and feedback we received from many people including Izak Benbasat, Eric James, Kalle Lyytinen, Dan Robey and Ron Weber. However, the usual disclaimer applies, and we take sole responsibility for the content of the paper.

REFERENCES

EDITOR'S NOTE: The following reference list contains hyperlinks to World Wide Web pages. Readers who have the ability to access the Web directly from their word processor or are reading the paper on the Web, can gain direct access to these linked references. Readers are warned, however, that

1. These links existed as of date of publication but are not guaranteed to be working thereafter.
2. The contents of Web pages may change over time. Where version information is provided in the References, different versions may not contain the information or the conclusions preferred.
3. The author(s) of the Web pages, not AIS, is (are) responsible for the accuracy of their content.
4. The authors of this article, not AIS, are responsible for the accuracy of the URL and version information.

Alter, S. L., P. Ein-Dor, M.l. Markus, J. Scott, and I. Vessey (2001) "Does the Trend Toward E-Business Call for Changes in the Fundamental Concepts of Information Systems? A Debate*", Communications of the AIS*, (5)10.

Amaravadi, C. S. (2001) "Improving Consumption", *Communications of the AIS*, (6)4.

Anderson, P. F. (1983) "Marketing, Scientific Progress, and Scientific Methods", *Journal of Marketing*, Fall, pp. 18-31.

Avegerou, C. (2000) "Information Systems: What Sort of Science Is It?", *Omega*, (28)5, pp. 567-579.

Banville, C. and M. Landry (1989) "Can the Field of MIS be Disciplined?", *Communications of the ACM*, (32)1, pp. 48-60.

Barki, H. and I. Benbasat (1994) "Contribution of the Theory of Reasoned Action to the Study of Information Systems: Foundations, Empirical Research and Extensions", *Proceedings, The 4th European Conference on Information Systems*, Lisbon, Portugal, pp. 5-15.

Ben-Menachem, M. (2001) "IT/IS Management and Research Direction", *Communications of the AIS*, (6)5.

Benbasat, I. and R. Weber (1996) "Research Commentary: Rethinking "Diversity" in Information Systems Research", *Information Systems Research*, (7)4, December, pp. 389-399.

Bhattacherjee, A. (2001) "Understanding and Evaluation Relevance in IS Research", *Communications of the AIS*, (6)6.

CAIS (2001) *Communications of the Association for Information Systems Special Research Relevance Volume,* (6) (March)

Camfield, T. (1985) "Professionalization of American Psychology" in O'Donnell, J. (ed.) (1985) *The Origins of Behaviorism: American Psychology, 1870-1920,* New York: New York University Press.

Chalmers, A. F. (1999) *What is This Thing Called Science*, Buckingham, UK: The Open University Press

Chatterjee, S. (2001) "Information Systems Research and Relevance", *Communications of the AIS*, (6)8.

Cheon, M. J., V. Grover, and R. Sabherwal (1993) "The Evolution of Empirical Research in IS, A Study in IS Maturity", *Information & Management*, (24), pp. 107-119.

Culnan, M. (1986) "The Intellectual Development of Management Information Systems 1972-1982: A Co-Citation Analysis", *Management Science*, (32)2, pp. 156-172.

Culnan, M. (1987) "Mapping the Intellectual Structure of MIS 1980-1985: A Co-Citation Analysis", *MIS Quarterly*, (13)2, pp. 221-229.

Culnan, M. and E. Swanson (1986) “Research in Management Information Systems 1980-1984: Points of Work and Reference”, *MIS Quarterly*, (10)3, pp. 289-301.

Davis, F. D. (1986) “A Technology Acceptance Model For Empirically Testing New End-User Information Systems: Theory and Results”, Doctoral dissertation, Sloan School of Management, Massachusetts Institute of Technology.

Davis, F. D., R. P. Bagozzi, and P. R. Warshaw (1989) “User Acceptance of Computer Technology: A Comparison of Two Theoretical Model”, *Management Science*, 35(8), pp. 982-1003.

Dennis, A. R. (2001) “Relevance in Information Systems Research”, *Communications of the AIS*, (6)10.

Farhoomand, A. F. (1987) “Scientific Progress Of Management Information Systems”, *Database*, Summer, pp. 48-56.

Farhoomand, A. F. and D. H. Drury (1999), "A Historiographical Examination of Information Systems", *Communications of the AIS*, Volume 1 Article 19.

Fishbein, M. and I. Ajzen (1975) Beliefs, Attitude, Intention, and Behavior: An Introduction to Theory and Research, Reading, MA: Addison Wesley.

Fitzgerald, B. and F. Adam (1997) “The Tale of the Diplodocus: Lessons for the IS Field”, Working paper, Cork, Ireland: Executive Systems Research Centre, Cork University College.

Foucault, M. (1982) “The Subject and Power, Afterword” in Dreyfus, H. L. and P. Rabinow (eds.) (1982) *Michel Foucault: Beyond Structuralism and Hermeneutics*, Chicago: Ill, University of Chicago Press.

Hartwick, J. and H. Barki (1994) “Explaining the Role of User Participation in Information System Use”, *Management Science*, 40(4), pp. 440-465.

Heart, T. and N. Pliskin (2001) “Making Academic IS Research More Relevant to Industry Via Open-Architecture Models for Career Paths in Academia”, *Communications of the AIS*, (6)12.

Hirschheim, R., H. K. Klein, and K. Lyytinen (1996) “Exploring the Intellectual Structures of Information Systems Development: A Social Action Theoretic Analysis”, *Accounting, Management and Information Technology*, (6)1/2, January-June, pp. 1-64.

Holden, C. (1988) “Research Psychologists Break with APA”, *Science*, (241), p. 1036.

Holsapple, C. W., L. E. Johnson, and J. Tanner (1993) “A Citation Analysis of Business Computing Research Journals”, *Information & Management*, (25), pp. 231-244.

ITAA (2001) “When Can You Start? Building Better Information Technology Skills and Careers”, *The Information Technology Association of America*, http://www.itaa.org/workforce/studies/01execsumm.htm, April 28.

Jackson, C. M., S. Chow, and R. A. Leitch (1997) “Toward an Understanding of the Behavioral Intention to Use an Information System”, *Decision Science,* (28)2, pp. 357-389.

Keen, P. G. (1980) “MIS Research: Reference Disciplines and Cumulative Traditions”, *Proceedings, First International Conference on Information Systems*, Philadelphia, Pennsylvania, December, pp. 9-18.

King, J. and L. Applegate (1997) “Crisis in the Case Study Crisis: Marginal Diminishing Returns to Scale in the Quantitative-Qualitative Research Debate”, Working Paper, Boston, MA: Harvard Business School.

Kitcher, P. (1993) *The Advancement of Science: Science Without Legend, Objectivity Without Illusions*, New York, NY: Oxford University Press.

Koch, S. (1985) “Afterward” in S. Koch and D. Leary (eds.) (1985) *A Century of Psychology as Science*, New York, NY: McGraw Hill, pp. 928-950.

Kuhn, T. S. (1970a), “Logic of Discovery or Psychology of Research?” in Lakatos, I. and A. Musgrave (eds.) *Criticism and the Growth of Knowledge,* Cambridge, UK: University Press, pp. 1-23.

Kuhn, T. S. (1970b) *The Structure of Scientific Revolutions*, 2nd edition, Chicago, Ill: The University of Chicago Press.

Lakatos, I. (1970) "Falsification and the Methodology of Scientific Research Programmes" in Lakatos and Musgrave (eds.) (1970) *Criticism and the Growth of Knowledge*, Cambridge, UK: University Press, pp. 91-196.

Mason, R. M. (2001) "Not Either/Or: Research in Pasteur's Quadrant", *Communications of the AIS*, (6)16.

Melone, N. P. (1990) "A Theoretical Assessment of the User-Satisfaction Construct in Information Systems Research", *Management Science*, (36)1, pp. 76-91.

Miller, G. A. (1985) "The Constitutive Problem of Psychology" in Koch, S. and D. Leary (eds.) (1985) *A Century of Psychology as Science*, New York, NY: McGraw Hill, pp. 40-45.

Mingers, J. and F. Stowell (1997) *Information Systems: An Emerging Discipline?* London, UK: McGraw Hill.

Mykytyn, P. P. Jr. and D. A. Harrison (1993) "The Application of the Theory of Reasoned Action to Senior Management and Strategic Information Systems", *Information Resources Management Journal*, Spring, pp. 15-26.

O'Donnell, J. (ed.) (1985) *The Origins of Behaviorism: American Psychology, 1870-1920,* New York, NY: New York University Press.

Orlikowski, W. and C. S. Iacono (2001) "Desperately Seeking the 'IT' in IT Research: A Call to Theorizing the IT Artifact", *Information Systems Research*, 12(2).

Pfeffer, J. (1993) "Barriers of the Advance of Organizational Science: Paradigm Development as a Dependent Variable", *Academy of Management Review*, (18)4, pp. 599-620.

Popper, K. (1962) *Conjectures and Refutations*, New York, NY: Harper.

Popper, K. (1975) "The Rationality of Scientific Revolutions" in Harre, R. (ed.) (1975) *Problems of Scientific Revolution: Progress and Obstacles to Progress in the Sciences*, Oxford, UK: Clarendon Press, pp. 72-101.

Robey, D. (1996) “Research Commentary: Diversity in Information Systems Research: Threats, Promise, and Responsibility”, *Information Systems Research*, (7)4, December, pp. 400-408.

Rollier, B. (2001) “Information Systems Research: Reversing the Orientation”, *Communications of the AIS*, (6)22.

Teng, J. T. C. and D. F. Galleta (1991) “MIS Research Directions: A Survey of Researchers View”, *Database*, (22), Winter/Spring, pp. 53-62.

Trouvé, J. M. (1992) “The Evolution of Science and the ‘Tree of Knowledge’” in Lamb, D. (ed.) (1992) *New Horizons in the Philosophy of Science*, Avebury,UK: Avebury, pp. 87-114.

Wagner M. and D. A. Owen (1992) “Introduction: Modern Psychology and Early Functionalism” in Owen, D. A. and M. Wagner (eds.) (1992) *Progress in Modern Psychology*, Wesport, Connecticut: Praeger, pp. 3-16.

Weber, R. (1997) *Ontological Foundation of Information Systems*, Australia: Cooper & Lybrand.

Weber, R. (1987) “Toward a Theory of Artifacts: A Paradigmatic Base For Information Systems Research”, *Journal of Information Systems*, Spring.

Wilson, D. J. (1990) *Science, Community, and the Transformation of American Philosophy, 1860-1930*, Chicago, Ill: The University of Chicago Press.

ABOUT THE AUTHORS

Ali F. Farhoomand is Associate Professor and Director of Centre for Asian Business Cases (CABC) at The University of Hong Kong (http://www.business.hku.hk/research.centres/cabc). He is the author of over two dozens refereed articles in journals such as *Communications of the ACM, Communications of the AIS, MIS Quarterly,* and *IEEE Trans. on Engineering Management*. In addition he developed 40 teaching cases in electronic commerce, which are distributed by CABC and Harvard Business School Press. His book, Global e-Commerce, was recently published by Prentice Hall (2001).

Don H. Drury is Professor of Accounting and Information Systems at McGill University in Montreal, Canada. His Ph.D. is from Northwestern University. He published papers in *MIS Quarterly, Communications of the ACM, Management Science, Information & Management* and numerous other journals in management information systems. He is the author of four monographs on planning, costing, and control issues in information technology and is consultant to government and business organizations.

Communications of the Association for Information Systems

ISSN: 1529-3181