Social Aspects of Computing

Rob Kling Editor

Can the Field of MIS be Disciplined?

Preoccupations about the present and future evolution of MIS as a scientific field seem to be gaining popularity among researchers. The authors contend that most models used by the investigators of the MIS field have been based on an inappropriate monistic view of science.

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A number of active researchers of Management Information Systems (MIS) have recently expressed preoccupations about the actual state and future evolution of MIS as a scientific field. Preoccupations of this type have been around for quite a while, but they seem to have gained in popularity and acuteness in the last few years as witnessed by the frequency of exchanges related to this topic whether it be through papers, colloquia, or private and public conversations. Some of those expressing concern assert that MIS researchers too often work on non-pertinent [5, 22] or unrelated topics ("gadget of the week") while others [26, 53] question the research methods. Some will propose frameworks [24, 35, 42] that should bring unity to a field they see as characterized by too much dispersion. Others [16] will react to an apparent proliferation of frameworks and contend that "we have enough conceptual frameworks. It is time to test, enhance, and embellish these frameworks with empirical research results." Keen [25], the opening speaker at the First International Conference on Information Systems, asks for no less than a clarification of reference disciplines, a definition of the dependent variable, the building of a cumulative tradition and the solution to corollary problems such as the relationship of MIS to technology, the relationship between MIS research and practice and the establishment of publication outlets. Others, explicitly adopting Kuhn's model of the development of science [31], announce the advent [18, 19] or ask for the establishment of [52] or for efforts leading to a clear definition [51] of a paradigm for MIS.

This list could be extended to contain concerns about the establishment of journals [8] or the organization of colloquia [37, 41] or other such manifestations. It is probably sufficient, however, to show the pervasiveness among some of the most active members of the MIS field of the interest for the actual state and future of MIS as a scientific field.

A major driving force underlying the concerns of these authors is a preoccupation with the idea of progress and maturation of the MIS field. Indeed, their comments are aimed at one or both of the two following goals: first, to point at what they perceive as obstacles to progress within the field and to propose means to eliminate them; second, to suggest actions deemed appropriate for accelerating the pace of progress in order for the field to mature more rapidly. In both cases this implies a legitimate need to understand and evaluate the present state and foresee the future of the MIS field. Indeed, members of any scientific field, and particularly those belonging to fields struggling for recognition such as MIS, have to worry about the social and scientific status of their discipline.

It must be realized that anyone attempting to assess the state of a particular scientific discipline must necessarily proceed with the implicit or explicit help of a model as to what a scientific discipline is and how it should develop. This model first leads one to focus on those factors in the situation under investigation that are deemed significant and, second, helps pass judgment, positive or negative, on these factors in order to suggest appropriate courses of action. So, the model one uses to assess the present state of a discipline and present possible solutions is of crucial importance and it should therefore be carefully selected and explicitly stated. Furthermore, if the investigator, as it is usually the case, feels the necessity of comparisons with other disciplines, this model should be general enough and somehow be able to take into account and reflect intrinsic differences between the investigated disciplines

which can be as diverse as physics, computer science, management theory, MIS, sociology and others.

It is our contention that most models used until now by MIS investigators have been based on an inappropriate monistic view of science. This is a direct consequence of an explicit or implicit adoption of the kuhnian model of scientific development based on the notion of paradigm and a narrow definition of the concepts of scientific progress and maturation which results in recommendations for the future development of MIS that are not, in our view, always appropriate.

Using Kuhn's well-known model as an illustration, this paper shows how a monistic conception of the development of science is too restrictive to help understand the present state of MIS. Later, we will use a model adapted from Whitley's work on the sociology of knowledge [57] as an example of a more general view of the development of scientific fields, and show how such a model can be used with great benefit to investigate our scientific field and to draw certain conclusions.

INADEQUACY OF A MONISTIC VIEW OF MIS: THE KUHNIAN MODEL

The concept of paradigm is the key to get to the essence of Kuhn's model of scientific development. This model has been summarized by Chalmers [11, p. 90] as: *pre-science–normal science–crisis-revolution–new normal science–new crisis.* A paradigm is said to reign during periods of normal science and is brought under severe challenge during periods of crisis-revolution. The pre-science phase is characterized by the absence of a paradigm. The way Kuhn defines and delineates the term 'paradigm' is thus central for a good understanding of his model of the transformation of a scientific discipline.

Unfortunately, the term paradigm has been used with many different significations throughout Kuhn's writings. Toulmin [50, p. 96–130] presents "five distinguishable phases" in Kuhn's use of this term between 1957 and 1970. These phases are seen as being closely related to the changes in Kuhn's conception of *revolution* as it became more and more evident that he really meant *evolution.* During this period, Kuhn went from seeing paradigms as dogmas and changes of paradigms as equivalent to religious conversion (revolution), to paradigms as exemplars and changes of paradigms as logically construed endeavors (evolution). As to the numerous meanings of the term paradigm, Margaret Masterman [36] produces a non-exhaustive list of citations from the first 1962 edition of *The Structure of Scientific Revolutions* indicating 21 connotations. Kuhn's followers' interpretations have resulted in the diffusion of additional meanings. So much so, in fact, that De Mey [15, p. 104–105] classifies some of them in three groups labeled through "their activity as *paradigm-hunting, paradigm-detection* and *paradigm-dissection.*" The paradigm-hunters are those "enthusiastic Kuhn followers who hope to remedy a deplorable state in their field by providing a paradigm or by promoting the search for it." Paradigm-detectors track down groups of interacting scientists through bibliometric or sociometric methods. Paradigm-dissectors see this concept as central to the cognition process and analyze it for its contribution to the understanding of this process.

Sharing what can be seen as the most widespread definition (even though it is not found as such in Kuhn's writings), the MIS researchers referenced in the introduction seem to use the term paradigm as meaning that members of a scientific discipline endowed with a paradigm always know precisely the relevant research topics in their discipline, the appropriate research methods and the proper interpretation of results. Therefore, a paradigm should dually indicate problems and methods not belonging to a discipline. An example of such a definition is provided by Ritzer [47, p. 7] for whom "A paradigm . . . serves to define what should be studied, what questions should be asked, and what rules should be followed in interpreting the answers obtained. The paradigm is the broadest unit of consensus within a science and serves to differentiate one scientific community (or *subcommunity*) from another."

This restrictive view of science explains why philosophers of science generally agree that Kuhn's vision of science is monistic: it allows only one dominant view to reign during periods of "normal science." As stated by Kuhn himself: "The new paradigm implies a new and more rigid definition of the field. Those unwilling or unable to accommodate their work to it must proceed in isolation or attach themselves to some other group" [31, p. 19]. But then, it is generally agreed that our discipline is made up of what has been called subfields. Mary J. Culnan, using an analysis of co-citations of the most often referenced MIS authors, has identified nine of these subfields in one study [12] and five in a subsequent one [13].

This brings the first considerations on the dangers in the claim for a paradigm for MIS if this notion of paradigm is to be understood in the original kuhnian perspective. These refer mainly to the identification of the community of owners of a paradigm, to the means of establishing a paradigm and to the scientific discipline used as an implicit standard of comparison in Kuhn's model.

If one is to build a paradigm, then the scientific community to which it will apply has to be identified. But then, who are the members of MIS? Traditionally, MIS has attracted scientists from *a priori* apparently weakly related disciplines such as computer science, decision theory, management theory, economics, psychology, and others. Each researcher would bring along concepts and methods from his background discipline and, often, would continue doing research which is closely related to it. This could explain why Culnan was able to identify so many subfields.

It might be argued that this dispersion has been a major factor in the rapid growth, be it only for the number of MIS researchers, experienced by the discipline in the last two decades. If a paradigm is to specify, as expected, the way in which research is to be conducted as well as how results are to be interpreted, then it seems dubious that we could end up with a paradigm that could include the different approaches

currently found in the MIS field. A large number of MIS members would face the choice of either leaving or converting (in its religious sense) to the emergent paradigm. Nobody could say for sure, at the time of this split, which branch(es) will eventually be considered as MIS, the one created by the paradigm or the more or less formal groups created by those leaving.

structive jungle warfare .... It is important that steps be taken to disentangle the management theory jungle. In a field where the many blunders of an unscientifically based managerial art can be so costly to society, delays cannot be tolerated" [30, p. 14]. This sounds much like what one now hears in MIS. Herbert Simon's answer was a call to patience and it stressed the fertil-

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Those claiming a paradigm for MIS seem to think that it could be created by a group of persons (which one would it be in MIS?) through simple force of will and adherence to a strict set of rules. Most scientists are concerned with contributing to their discipline, not with building paradigms. As stated by Chalmers [11, p. 93]: "Because of the way he is trained, and needs to be trained if he is to work efficiently, a typical normal scientist will be unaware of and unable to articulate the precise nature of the paradigm in which he works." Stegmüller [49], whose understanding of the term paradigm as exemplars was recognized as "captur[ing] precisely my original intent" by Kuhn [32], expresses similar ideas: "Normal scientists never examine their paradigm critically, in particular the paradigmatic theory. They simply use the theory uncritically as an instrument for puzzle solving" (p. 141).

A paradigm can only be observed *hic et nunc*; it is a result of the action of the forces at work in a scientific field. In the kuhnian model, a paradigm appears after the pre-paradigm phase or after a crisis-revolution; a call to arms therefore seems absolutely useless as a paradigm will emerge only if certain conditions are met. Even when adhering to such a model, one should always remember that paradigms are largely a matter of implicit social consensus and that their emergence requires time and the combination of many favorable factors which can, at best, be facilitated. The direct consequence is that one should not distract researchers from their daily activities and ask them to try to set up a set of rules to be called a paradigm; rather, one could observe how these researchers proceed, elaborate a model and propose it as a paradigm. We argue here that Kuhn's framework cannot, at the present time and in a foreseeable future, represent such a model for MIS.

In this regard we refer to a comparable debate that took place in the field of management theory at a symposium held more than two decades ago. Similarities between management theory and MIS are worth noting: the members of both fields have varying backgrounds, work on apparently unrelated topics with different research methods and their results are often interpreted in divergent ways. These similarities justify a comparison of the history of this recent yet older field of management theory.

At this symposium Harold Koontz proposed ways and means to get his discipline out of a "confused and de-

ity of the multitude of points of view for a young discipline. He first stated that "confusion, by another name, is progress to which we have not yet become accustomed" (p. 82); in other words, we should not be so sure that the relative calm of a paradigmatic period is a lot better than the turmoil of a crisis. Simon later added "Science, like all creative activity, is exploration, gambling, and adventure. It does not lend itself very well to neat blueprints, detailed road maps, and central planning. Perhaps that's why it's fun" (p. 85). This conception is radically different from the rather monolithic view of normal science built into the kuhnian model and deserves consideration. Equally interesting was Robert Dubin's statement at the same symposium: "I happen to be an intellectual free enterpriser. I would like to put in a plea for free enterprise in perhaps one area where it still can exist; namely, in the affairs of the mind, in the affairs of the intellect" (p. 106).

History seems to repeat itself. Once more we find ourselves confronted with the problem of the delicate balance between a high degree of organization in a field and consequent possible sclerosis on the one hand, and free enterprise will and consequent risk of futile dispersion on the other hand. It is a contention of this article that the imposition of a paradigm in MIS, if at all possible, would be a risky move towards the former.

We also argue that MIS researchers should not long for a paradigm as it rests upon assumptions of the kuhnian model of science which imparts value to knowledge on the basis of the conformity of its methods and results to an explicit standard: physics. Physics is but one science and its largely recognized value stems more from the fact that, at least in a popular view, it is applied to so-called *hard* objects with very adequate methods that have been perfected by sharp minds over a number of centuries. The large social consensus on the status of physics as the *Queen of Sciences* brings scientists to compare the state of their field to that of *The Model* and it is no wonder, the basis of comparison being biased, that they end up feeling very uncomfortable. This should not be so. Chalmers clearly states the position held in this article on this matter [11]: "Philosophers do not have resources that enable them to legislate on the criteria that must be satisfied if an area of knowledge is to be deemed acceptable or scientific. Each area of knowledge can be analyzed for what it is.... Each area of knowledge is to be judged on its

merits by investigating its aims and the extent to which it is able to fulfill them. Further, judgments concerning aims will themselves be relative to the social situation" (p. 166).

In conclusion, the idea of establishing a paradigm for MIS, along the lines of the popular conception of Kuhn's model, if at all practicable, would not bring about the effects expected by the very proponents of this idea. On the contrary, the most probable result would be a break-up of the field into rather hermetic factions and the consequent loss of the creativity generated by exchanges about research topics and research methods. Furthermore, one can doubt that MIS paradigm-hunters would themselves be ready to accept all the consequences of an implementation of the full kuhnian model commensurate with their view of the paradigm. In fact, they seem to long for a period of perpetual *normal science*, thus forgetting the cyclical nature of a process of transformation that would take them regularly through the pains of crisis-revolution.

The Kuhnian Model as Revisited by Kuhn

Kuhn has reacted to his critics [33] and his ideas on the concept of paradigm have evolved after the publication of [31]. In [32] he writes about the problems caused by the confusion around the concept of paradigm and stresses the importance of the notion of scientific community: "Whatever paradigms may be, they are possessed by any scientific community, including the schools of the so-called pre-paradigm period. My failure to see that point clearly has helped make a paradigm seem a quasi-mystical entity or property which, like charisma, transforms those infected by it. There is a transformation, but it is not induced by the acquisition of a paradigm." (footnote, p. 295). This allusion to some magical properties possessed by a paradigm and its refutation by Kuhn himself should seem a sufficient answer to those asking for a paradigm in MIS on the basis that it will necessarily transform our field for the better. In the same text, Kuhn even proposed to drop the concept of paradigm and replace it with a new notion, that of a "disciplinary matrix" constituted by 1) symbolic generalizations, 2) common beliefs and models, 3) shared values, 4) exemplars and other elements (said to exist but not presented in [31]).

Even though the periods of normal science are presented by [31] as parts of a "cumulative enterprise," they also have been clearly characterized as being made up of *mopping-up operations* and *puzzle-solving* that produce no *major novelties.* A paradigm is thus not necessarily the moving force that will propel a scientific discipline on the way to the test of critical hypotheses or the construction of new theories that will incorporate anomalies in observed phenomena. To the contrary, Kuhn states clearly that, during a normal science era, the typical normal scientist observes only what his paradigm tells him to observe and most of the observations that do not fit this very tight schema either go on unnoticed or are put aside as irrelevant or, better, for the sake of "progress," as something that cannot be explained yet. To use Karl Weick's expression [53], "believing is seeing" during those periods which are characterized, in retrospect, by no great leap in scientific progress.

Progress is at best relative during periods of normal science, but it is said to be a ". . . universal concomitant of scientific revolutions" [31, p. 166]. The great moments of the history of science seem to be, both in the general public's mind and all along Kuhn's book, the great revolutions brought by Newton, Lavoisier, Einstein, Darwin, and others. These great moments could easily be considered the only moments of real progress and the preceding periods of normal science could be called *quiet science.* The reign of a paradigm might mean progress, but it refers to a very restricted type of progress.

Thus, it is not clear at all whether progress occurs most during periods of normal science (paradigmatic periods) or during periods of crisis-revolution in the kuhnian model. To make matters worse, Kuhn discusses the difficulty of establishing an operational definition of scientific progress and concedes an inevitable circularity between what he considers progress and what he sees as science. To use Stegmüller's [49, p. 156] expression, in any struggle over what science should be, "the victors are by definition the progressives."

In summary, asking for the establishment of a paradigm in the name of progress for a scientific discipline rests on shaky grounds. There could be as much progress in the non-paradigmatic periods. As stated by Simon, science may progress (whatever that word means) better without "neat blueprints, detailed road maps and central planning."

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Ambiguity of Kuhn's Conception of Progress

The creation of a paradigm is often claimed for the sake of progress in a discipline. However, an attentive reading of [31] raises questions about Kuhn's vision of progress. For him, and as stated by Stegmüller [49, p. 137], there are two "forms of pursuing science: . . . normal science and extraordinary or revolutionary science."

The Importance of the Concept of Scientific Community

Kuhn [31, p. 176] recognized the importance of the concept of scientific community when he wrote that "If

this book were being rewritten, it would . . . open with a discussion of the community structure of science, a topic that has recently become a significant subject of sociological research and that historians of science are also beginning to take seriously." To him, "A scientific community consists . . . of the practitioners of a scientific specialty" and is a factual observation, not an *a priori* determination. These scientific communities are good candidates as the basic units of analysis: ". . . there is excellent reason to suppose that the scientific enterprise is distributed among and carried forward by communities of this sort" [32, 296–297]. Operationally, these communities are created along both a social and a cognitive dimension. They can be observed through group membership, membership in professional societies, journals read, attendance at summer institutes and special conferences, preprint distribution lists, "and above all to formal and informal communications and networks, including the linkages among citations."

To recapitulate what has been said so far about Kuhn's model and its application to the field of MIS, it has been argued that, first, it is too restrictive in its application to bring a valuable contribution to our field, second, the advent of a paradigm does not necessarily guarantee progress in a field and, third, this model nevertheless contains the seeds for the elaboration of a more appropriate model which can be extracted from recent contributions to the philosophy of science. One of these contributors [10, p. 74–75] probably sums up best what has been said when he writes "Though Kuhn's influence has spread far beyond the confines of the philosophy of science, some nonphilosophers who are acquainted with his work have not been aware of criticisms of his position. This has the paradoxical (though hardly unusual given the information lags that exist among disciplines) consequence that at present Kuhn's prestige is greater outside the philosophy of science than within it."

Saying that the application of Kuhn's model to MIS would not be beneficial to our discipline does not imply that the quest that has taken us this far is futile. On the contrary, epistemological reflection is an essential and integral part of any scientific enterprise but it should be done with great care and with the best possible means. This rebuttal of the kuhnian model should not be extended to the reflections and discussions carried out by certain MIS researchers. Their task is legitimate but must be supported by more adequate models of the development of science.

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THE SCIENCES AS INTELLECTUAL FIELDS

It is now widely recognized that the production of scientific knowledge is an endeavor that is simultaneously cognitive and social. Although the cognitive dimension has been quite extensively studied since the first days of epistemology, the social dimension is a more recent preoccupation that has been picked up mainly by sociologists of knowledge. As sociologists are interested in societies, the question of specifying the basic unit of analysis of *societies of knowledge producers* cannot be avoided. The concepts of scientific discipline and community have been used by some while others would rather use the concept of field to capture the social object constituted by groups of scientists at work.

Böhme [7] concentrates on the informal organization of disciplines for which he uses the expression "communities of research"; argumentation is then seen as the main ingredient of the basic relation within these communities. Mulkay [40, p. 61–63] uses the terms "scientific community" and "intellectual community" on the same page. These are to be seen as the places where the meanings commonly used by groups of scientists in their scientific activities are constructed. A detailed description of this process of construction would be difficult as "these meanings . . . are inherently inconclusive, continually revised and partly dependent on the social context in which interpretation occurs." To Knorr-Cetina [29, p. 82–83] fields are the "relevant contextual organisation[s] of laboratory production . . . which in principle transcend the specialty networks . . . [and] may include the provost of the university, the research institute's administrative staff, functionaries of the National Science Foundation, government officials, members or representatives of industry, and the managing editor of a publishing house." These fields "not only criss-cross the borders of a specialty group, but also shrink and expand in response to the issues at stake"; they "appear to be the locus of a perceived struggle for the imposition, expansion and monopolisation of what are best called *resource-relationships*." The expression refers to allocation of jobs among scientists, distribution of research money, dissemination and use of research results . . . . For Whitley [57, p. 8], intellectual fields are ". . . the social contexts in which scientists develop distinctive competences and research skills so that they make sense of their own actions in terms of these collective identities, goals and practices as mediated by leaders of employment organizations and other major social influences." Finally, the definition of Audet [3], in our view, captures and comprehensively links all the main ingredients incorporated in the preceding definitions. To him, a field is a common ground on which field members compete to gain control of the definition of conditions and rules of knowledge production and validation and, at the same time, the system of their relations and relative positions.

According to this last definition, one would probably vainly ask for the constitution of a field around his own personal view of the appropriate research topics and methods, as his recommendations would necessarily become an object of debate. To Audet, the research topics and methods are themselves the result of the collective action of a field's contributors. In other words, a field cannot be created and evolve according to precisely pre-defined plans. Actions can be taken that would influence its constitution and evolution, but the field itself can only be identified and characterized by observing its contributors at work. This assertion is in line with Newell's view when he observes that "scientific fields emerge as the concerns of scientists congeal around various phenomena. Sciences are not defined, they are recognized" [15, p. 145]. Of course, the difficulty of identifying what constitutes a field is not without similarities with the difficulty of specifying the owners of a paradigm (is there a paradigm for the whole of physics or should it be restricted to nuclear physics?). Indeed, there remains the necessity of a recursive use of the concept since subfields can also be seen as fields on their own when they become the units of analysis. Clearly, the term *field* does not refer only to the traditional scientific disciplines such as biology, economics, sociology and so on, but can also be applied to artificial intelligence or molecular biology.

Fields as Reputational Systems

According to Whitley, the quite explicit aim of the members of intellectual fields is the production of new knowledge. This implies standards of a particular kind for organizing and controlling research, standards which are created by the members of the field through a particular structure called the *reputational system.* The evaluation of one's contributions by colleagues can be recognized as part of this reputational system. The importance of the reputational system is great as it controls the access to rewards, be it money for research projects, academic promotion, invitations as guest speaker at prestigious conferences and so on. The search for a positive reputation is achieved through the use of a formal public communication system which comprises colloquia, journals, seminars, etc. Scientific research is then seen as a form of work organization, a special craft system " . . . distinguished by its combination of continual novelty production . . . with strong collective co-ordination of task outcomes through access to rewards being controlled by reputations based on the utility of results for colleagues' research." [57, p. 33]. Whitley even uses the expression *colleague-competitors* to show how one must reach and maintain an equilibrium between innovation and conformity to the established knowledge and methods.

Presentation of Whitley's Model

The concept of field is intuitively appealing and surely seems relevant to scientists who are sensitive to the social aspects of their scientific world. Yet, it should be evident that an operational definition could never exhaust such a rich reality; a field is perpetually subject to change as a result of the actions of inventive and interested human beings. That is why any model that tries to explain the actual state and possible evolution of scientific fields will probably turn out to be inadequate with time or upon intensive usage. In this domain, models, as dominant as they may be at any particular time, eventually become obsolete and we agree with Pondy and Mitroff [45, p. 21] that "If we have begun to confuse the map with the territory, then it is time to change maps." It is within this map perspective that we present a classificatory model based on Whitley's work and it is also within this perspective that it should be received. But, even though it is based on a model that has been received as over-ambitious by Pinch [44] and of problematic applicability by Yearley [62], in our view and for the time being, it is a map that captures and expresses a lot of what we can observe within our field and across fields.

As already hinted, Whitley applies the methods of sociology, more specifically of the sociology of work organization, to the intellectual enterprise (the public sciences) carried on by scientists. He has proposed a classificatory scheme that, according to him, highlights important differences between the various intellectual fields. He also uses a set of contextual factors to explain how the evolution of these fields can be explained or influenced through certain actions of internal or external agents.

There is a certain analogy between Whitley's concept of intellectual field and Kuhn's scientific community. The different public sciences can be classified in both Kuhn's and Whitley's models. Whereas Kuhn emphasizes the necessary evolution toward normal science, we will see that for Whitley, and in accordance with Chalmers as quoted above, each field possesses its own characteristics and pattern of evolution and none of these fields is *a priori* more valuable than any other.

Classificatory Scheme

In his earlier works [54, 55], Whitley introduced the notions of cognitive and social institutionalization of scientific fields. While these two dimensions have become quite classic, they are not independent. Whitley has thus provided a new model in his most recent publications [56, 57]. This model uses four variables that express the cognitive and social dependence of members of a field through the way they produce knowledge and interpret each other's research results.

The strong link between some aspects of the social and cognitive dimensions of intellectual fields [62] justifies a simplification of Whitley's model with minimal loss of interpretative power. This reduced version uses only three variables as classification criteria and should thus be easier to understand and use. These three variables are:

- functional dependence;
- strategic dependence;
- strategic task uncertainty.

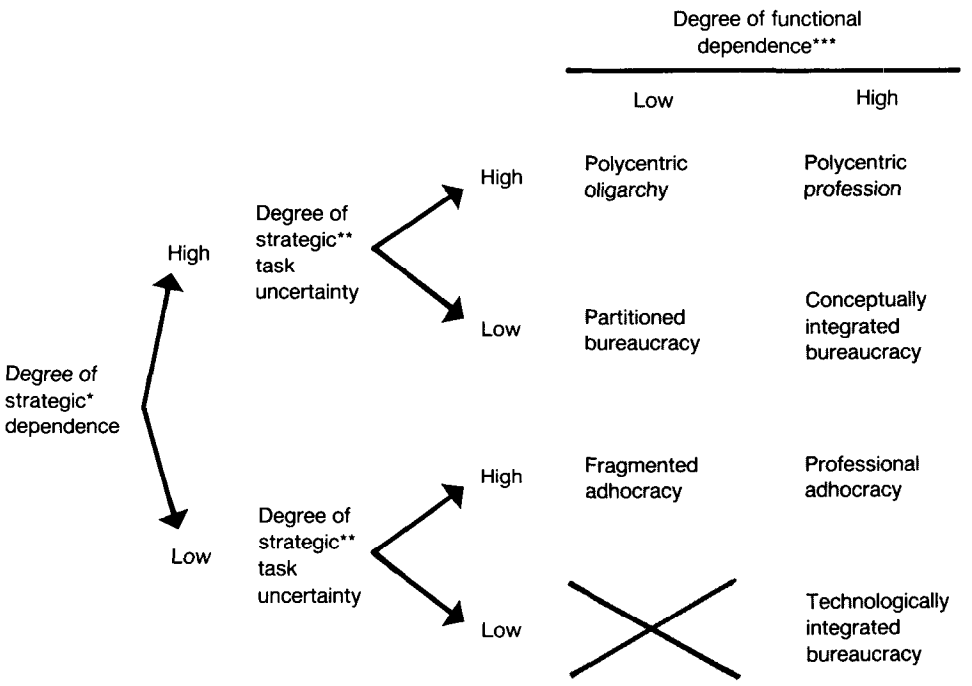
These variables are used to produce a typology of intellectual fields that is presented in Figure 1.

Functional dependence, as used here, refers to " . . . the extent to which researchers have to use the specific results, ideas, and procedures of fellow specialists in order to construct knowledge claims which are regarded as competent and useful contributions" [57, p. 88–123], and to "the extent to which work techniques are well understood and produce reliable results . . . " As functional dependence increases, one can observe " . . . greater specialization of research topics and tasks, standardization of work procedures, competence standards and communication structure, and co-ordination of task outcomes from different research sites for dealing with particular problems. The scope of problems tackled by individuals and research groups tends to decline as functional dependence grows." The degree of functional dependence could be said to be a measure of the dependence on standard topics and methods of knowledge production.

Strategic dependence refers to " . . . the extent to which researchers have to persuade colleagues of the significance and importance of their problem and approach to obtain a high reputation from them." Again, "Increases in strategic dependence are associated with greater concern over the relative importance of problems and approaches, and so intensify competition between groups for the domination of the field . . . high strategic dependence implies a strong need to co-ordinate and interrelate research strategies and goals with those of specialist colleagues in order to gain important reputations from them."

Strategic task uncertainty is defined in relation to the fact that " . . . the stability of problem formulations, and of hierarchies of problems according to their importance and significance, varies across fields . . . " Strategic task uncertainty is low when members of the field agree on a hierarchy of research problems, when there is a tight control over research goals and minimal local autonomy in the formulation of research problems and significance standards. Conversely, a high strategic task uncertainty is associated with the presence of loosely coupled schools of thought.

The different combinations of hi-lo values on the three variables produce eight possible configurations of intellectual fields. One of them is eliminated on the grounds that, according to Whitley, it is "unstable and unlikely to become firmly established" [57, p. 205–206]. We are then left with seven valid entries. This classifi-



*: Strategic dependence is a measure of the political dependence of the members within a field. It is concerned with coordination, control and conflict.
**: Strategic task uncertainty is a measure of the conceptual coherence within a field. It is concerned with relative importance of the different problems and the presence of schools of thought.
***: Functional dependence is a measure of the technical and procedural coherence within a field. It is concerned with the standardization of research tools, procedures and results interpretation. Adapted from [57].

FIGURE 1. A Classification of Intellectual Fields

cation is not necessarily exhaustive and unique but highlights some of the major differences between the scientific fields and helps explain such differences. Again, it must be emphasized that these are unqualified differences; it is neither good nor bad for a discipline to qualify as high or low on any of the variables used for classification. A particular value on each criterion is to be seen only as a characteristic of the investigated field.

It is beyond the scope of this article to comment on each of these stable types and compare them. The names used by Whitley to designate them are, to a certain extent, self-explanatory and are further explained in [57] where the interested reader may find a general description of each of these types. Some examples of scientific field classifications by Whitley may nevertheless be useful here.

A polycentric oligarchy in which contributors produce diffuse, locally coordinated knowledge, is exemplified by German psychology before 1933 with its many schools of thought. A polycentric profession appears with the emergence of a few dominant schools as functional dependence increases; experimental physiology is proposed as an example.

Seen as a partitioned bureaucracy, a rarely observed configuration, is the field of business finance. Low functional dependence is evidenced by the relative schism between field members over the all-important efficient market hypothesis and empirical research findings that continuously contradict its validity. Low strategic task uncertainty and high strategic dependence can be seen as a result of the growing influence of analytical economics whose "intellectual goals and standards . . . have come to dominate the literature" [59, p. 6].

Modern physics, on the other hand, is classified as a conceptually integrated bureaucracy: low on strategic task uncertainty and high on functional and strategic dependence; these are the characteristics of Kuhn's normal science. Management studies (or administrative sciences) is viewed by Whitley as constituting a fragmented adhocracy: high on strategic task uncertainty and low on functional and strategic dependence. This is a result of the relative failure of the attempt to apply the scientific methods to management problems. Administrative sciences are now "to be seen as a largely academic enterprise with few connections to managerial actions and one which is highly internally differentiated . . . into separate ideas and approaches." [58, p. 775].

Artificial intelligence is to be seen as a professional adhocracy, i.e., as producing highly specific and empirically focussed knowledge with "a variety of problem formulations and conceptual approaches linked to particular skills" [57, p. 161–162]. Finally, modern chemistry, with its well-established industrial and academic research technology, very well illustrates a case of technologically integrated bureaucracy as its members concentrate on their particular problems, using quite standardized methods and tools and not showing a great concern about "general contribution to the field as a whole."

The Contextual Factors Influencing the Evolution of a Field

It is interesting to know the means through which the characteristics of any field can be influenced. For example, according to Whitley, the centralization of funds or jobs increases strategic dependence and decentralization does the reverse. Such a decrease in strategic dependence was the case in bio-medical research in the United States, especially for cancer and heart diseases, when governments started pouring more and more money into this research area thus creating additional sources of research funds. Knowledge of the effect on our field's characteristics of external or internal decisions and of collective or individual actions should help us understand the implications of propositions such as the claim for the establishment of MIS journals or clarification of reference disciplines.

Whitley has identified three sets of contextual factors affecting the structures of scientific fields [57, p. 220] that constitute the more dynamic part of Whitley's model. They are:

1) Degree of reputational autonomy from competing intellectual organizations and the wider social structure in setting standards: performance standards, significance standards and descriptive terms and concepts.

2) Degree of concentration of control over access to the means of knowledge production and validation: extent to which control over jobs, facilities, funds, and journal space, is dominated by a small number of employment units and research sites (horizontal concentration) and to which it is unequally shared between employees within those units (vertical concentration).

3) Structure of reputational audiences: variety of audiences available to get a reputation and extent to which such audiences are ranked in terms of prestige and importance.

A concrete illustration of the workings of these contextual factors is the use of mathematics or statistical methods which reduces task uncertainty, restricts audiences and gives access to prestigious audiences. In the same way, centralization of control over publication outlets can, at least until new outlets are created, increase the degree of functional and strategic dependence.

THE CASE OF MIS

We first have to ask if MIS qualifies as a scientific field. The following facts support a positive answer to this question. Many universities offer MIS programs at both the undergraduate and the graduate levels. These programs are managed by MIS departments that receive and use MIS research funds. Specific publication outlets have emerged such as *MIS Quarterly* and *Information and Management* while papers published in other prestigious journals are identified as MIS papers. Prestigious conferences such as the International Conference on Information Systems are held on a regular basis (the eighth

was held in 1987 in Pittsburgh) and MIS sections are created in more general conferences such as the Administrative Sciences Association of Canada yearly conferences. The availability of a directory of the academic members of North America [38] containing entries on 1,696 members, 447 schools and 469 academic programs stands as an additional indication that MIS qualifies as a field. Based on these characteristics, we feel justified to assume that MIS is a field.

MIS as a Fragmented Adhocracy

We will now attempt to classify MIS within the model previously presented. We will not try to prove that MIS is of type such and such; our intention is to bring forward indications as to the type that best fits our perception of our field. To us, knowing the exact situation of MIS within the model is not in itself as important as the results of the exercise of trying to figure out where our field stands on each of the pertinent variables and how this can be argued. The process of classification can thus be used as a basis for further discussion on the actual state of MIS.

It will be argued here that MIS is a fragmented adhocracy; it shows low degrees of strategic and functional dependences and a high degree of strategic task uncertainty. Whitley [58] has established the main characteristics of a fragmented adhocracy as the following: Research is rather personal and weakly coordinated in the field as a whole; a researcher can gain a reputation by contributing in a way that is largely specific to a group of colleagues or a research site. The field is largely open to an educated public and amateurs can affect the field's standards; barriers to entry in the field are weak and going from one fragment to another is quite easy. Reputations are fairly fluid, control of resources is unstable, coalitions are likely to be ephemeral and leadership is often of charismatic nature. Common-sense languages dominate the communication system.

The classification of MIS can first be established on the basis of what its members say about the field, its research objects and its methods. As a matter of fact, these have been the themes of many papers, colloquia or conferences and the resultant epistemological considerations of active contributors to the field constitute a rich material for the purpose at hand.

Anyone could list quite a number of definitions of the central object of our discipline, a management information system, by looking up in textbooks or research papers, but no one seems to have taken the trouble of extensively examining these often conflicting definitions and their implications. On the other hand, complete definitions of the field of MIS are scarce, if there are any. It is not easy to define a field whose members ". . . still have not settled on what should be included or excluded from [their] area" [16, p. 3]. On the same matter, Culnan and Swanson [14, p. 288] state that the field of MIS is only emerging and have found "no evidence . . . that a consensus has emerged as to the body of MIS work held to be integral to the field." This is hardly a surprising situation for a discipline whose members ". . . get diverted almost daily, by new research ideas, gee-whiz applications, consulting, etc." [25, p. 10]. In terms of the model used here, this indicates a low degree of strategic dependence and also hints at a low degree of functional dependence since researchers' contributions are driven more by the new technological opportunities (the weekly technological events of [16]) or research fads than by concern over their colleagues' contributions, evaluation and judgment.

Ein-Dor and Segev [17, p. vii] have proposed a paradigm for MIS (even though the use of paradigm gives them trepidations caused by the "recent disrepute into which this word has fallen.") Their book contains 93 propositions linking over 100 variables partitioned between success of MIS, as dependent macro variable and many independent variables. These propositions are extracted from an extensive review of the literature related to MIS and are presented under seven loosely coupled headings indicating a fragmentation of research topics. The expression *identity crisis* used in [16] to characterize MIS illustrates this situation in which a researcher can build his contribution upon previous results of colleagues in one or two of these domains but most probably not in all of them. This results in a low degree of functional dependence among members of the field and stands as an indication of a high strategic task uncertainty as it is difficult to build an overall consensus on an exhaustive hierarchy of all the topics included in this multiplicity of research areas.

As to the research methodologies used in the field of MIS, one can refer to the proceedings of an IFIP colloquium [41, p. 4] at which methodologies ranging from surveys to phenomenology were discussed and in which "a theme that emerged very strongly was that we should let many flowers bloom." The conclusion drawn by Bjørn-Andersen in [6, p. 275] is: "However the main conclusion has been that of methodological pluralism . . ." This conclusion can be related to Keen's [25, p. 10] statement that in MIS, "there is no clear theoretical base and no match between theory and method." These indicate a high level of strategic task uncertainty in the field as a whole. Klein's [26] opinion on the IS development methodologies refers to a topic that, although of relatively narrow scope, is of major importance as it is concerned with the central object of our discipline. Saying that "disagreement about the right methods of IS development extends to the diagnosis of what causes the problems" directly expresses a high level of strategic task uncertainty in a fragment of MIS concerned with a rather concrete problem.

To document our case from another angle, let us go "inside" the field of MIS and consider the "cognitive styles" research theme which has been quite popular for nearly a decade. A number of research papers related to this theme has been published since the seminal contribution of Mason and Mitroff [35]. The popularity of this research area could have been easily interpreted as the signal of the emergence of a cumulation of research results. In 1983, however, after an ex-

tensive review of this literature, Huber came to the conclusion that the results of these studies did not lead to "operational design guidelines" and that future research on this theme could not result in a significant contribution to the establishment of these guidelines. To him, the bulk of these contributions amounted to much ado about nothing. Even though Robey [48] did qualify Huber's conclusion, he nevertheless conceded the thrust of his argument.

The fact that Huber had not been himself one of the main contributors in the cognitive styles "fragment" but cared enough to undertake its evaluation indicates the presence of a strategic dependence within the MIS field. Indeed by so doing, he was, in fact, questioning the significance and importance of this problem for MIS as a whole. This can be taken as an indication that strategic dependence is high enough within MIS to qualify it as a field, at least as far as this particular dimension is concerned. But then, the fact that this research theme was not fundamentally questioned by other members of MIS not directly involved in this area for almost 10 years shows that strategic dependence could hardly be seen as very high. Moreover, as Huber indicates, these research results did not find their way into design methods; this is an indication of low functional dependence.

Fragmented adhocracies have been described as displaying weak barriers to entry. That entry in the field of MIS is perceived as fairly easy by many, at least in the academic world, is exemplified by the content of a brochure presented by the American Assembly of Collegiate Schools of Business [23]. In the presentation of the '1987 Information Systems Faculty Development Institute' that offers a highly intensive, **four and one-half weeks program**, it is stated that "The course is specially designed for terminally qualified business school faculty members whose specialization and training is not in MIS, but who wish to move in this area **to teach and do research**. Management scientists, accountants and organization behavioralists are examples of intended participants . . . [others] are faculty holding a doctorate from non-business fields such as mathematics, computer science, information science, the behavioral sciences and education who wish to shift to a business school position."

Finally, some writings can be interpreted as a global opinion on the fragmented nature of MIS. For example, according to Culnan and Swanson [14], "Davis suggests . . . that MIS represents the intersection of six fields of knowledge: computer science, behavioral science, decision science, organization and management, organizational function and management accounting." And the fragmentation has an impact on doctoral students who, according to Keen [25, p. 11], "since there is no consensual core to MIS research, . . . are often puzzled as to how to structure their preparation."

In our view, all the considerations presented suggest a strong fragmentation of MIS in relation to the research themes and methods and support a classification of MIS as a fragmented adhocracy.

Contextual Factors Applied to MIS

The fact that MIS is not purely academic, that MIS departments are to a large extent vocational schools in that their graduates are eagerly recruited by a supportive business community, has a considerable impact on the actual and future states of our field. Whitley has documented the influence of practitioners in similar cases, namely that of management sciences [61], of administrative sciences at large [60], and of business finance [59]. One of the main consequences of this influence is that "long term, theoretically oriented research programmes are not very likely . . . to be developed and followed in these sorts of fields," [61, p. 4].

Maintaining links between academics and consultants or MIS employees within organizations (audiences with possibly divergent objectives and methods) will always result in the actual low functional dependence. This would then imply that those requesting a kuhnian paradigm, thus a high degree of functional dependence, may implicitly be asking for a separation between academics and other members of MIS. This would result in the fore-mentioned possible break-up of our field as it is currently constituted with the academics forming a new field. The setup of a new IS *research* journal by The Institute of Management Science [*MIS Interrupt*, August 1987] promotes such a separation. While this would probably result in higher functional and strategic dependences, chances are that this new field could rapidly resemble the actual OR field [59, 61].

On the other hand, maintaining links with practitioners does not necessarily imply that MIS will remain a perpetual fragmented adhocracy. A possible evolution is illustrated by the example of *business finance.* This field has maintained strong connections with the practitioners, whose problems have always been considered worthy research topics, and who, being educated by the academics, have always applied to their practical problems the sophisticated methods they had learned. These strong connections, also reinforced by the fact that academics have themselves often been very active as practitioners, account for a high degree of strategic dependence and reduced strategic task uncertainty. This example suggests that MIS, even with its strong vocational character, might evolve toward something else than a fragmented adhocracy. According to [59, pp. 5–6], the field of business finance has moved from fragmented adhocracy to partitioned bureaucracy but could return to a fragmented state if "scientific reputations in the field become less strongly determined by analytical standards, and/or valued resources can be obtained through other routes."

Indeed, as stated earlier, analytical standards such as the use of mathematics, or statistical methods, reduce task uncertainty, restrict audiences and give access to prestigious audiences. Thus, requesting the use of statistical treatment of empirical data as the only legitimate research strategy for MIS implicitly promotes a move toward the partitioned bureaucracy or the conceptually integrated bureaucracy types. This transformation would be facilitated by the induced reduction

in the number of participants and by more precise and stable problem formulations as they would have to be amenable to empirical investigation and statistical treatment. The side-tracking of possibly valuable contributions by researchers not privileging statistical or empirical approaches and the potential reduction in the issues addressed have to be considered.

The creation of other prestigious MIS journals would have the effect of increasing the degree of functional and strategic dependences since MIS authors could refer more easily to other MIS productions and less to management theory for publication in Administrative Science Quarterly, less to computer science for publication in the Association for Computing Machinery journals, and so on.

In conclusion, the contextual factors constitute the more dynamic part of Whitley's model; any transformation in these may influence a field's status. But the mechanisms that produce these transformations are subtle. No individual member of our field can impose changes on these contextual factors and the resulting characteristics for MIS. A scientific field is perpetually created by the community of its owners and cannot be deliberately changed without the convergent actions of a large segment of its members. Given the fragmented nature of MIS and the relative strength of each of its many fragments, it seems unlikely that it will become a monistic scientific field in the near future. But the main lesson to be drawn from the application of Whitley's model is that a field should be accepted for what it is.

The Value of MIS is Independent of its Classification

The classification scheme just presented is, from a strictly epistemological point of view, value-free. A conceptually integrated bureaucracy can never be said to be better than a professional adhocracy. Any change in the classification of a field brought by a change in its characteristics should not be seen as either promotion or demotion; going from conceptually integrated bureaucracy to professional adhocracy is no worse or better than going from polycentric oligarchy to technologically integrated bureaucracy. On the other hand, saying that no one box of the Whitley model is better than the other does not mean that these boxes are equally comfortable for researchers. Fragmented adhocracy, as it is the most liable to identity crisis by its very nature, is understandably less comfortable to some of its members than others.

This leads us to Chalmers' opinion quoted earlier stating that any scientific field is to be judged relative to its aims, their social importance and their degree of fulfillment. The failure of management science (or operations research as it is also known) and its prestigious mathematical apparatus to solve the problems of the larger field of management theory should stand as a proof that success in a field can be as relative to the simplicity of its objects or aims as it is to the power of its tools.

As to the value of our field considered from the point of view of Chalmers' criteria, a few things may be said that could comfort those who feel uneasy about the importance of the contribution of MIS members to the accrual of knowledge. With an original perspective centered on either management, information, systems or a combination of these, MIS has made significant contributions in many domains of knowledge. There are ways in which such fields as psychology, computer science, decision science, organization theory, managerial practice and others have benefited from work done in MIS. Let us simply mention two examples.

In recent years, the field of *decision science* has probably been more inspired by the efforts in the development of DSSs than by anything else. The actual works on end-user computing could very well result in concrete means for managers to (at last!) recapture the control over this very critical organizational resource that information is.

This list could be extended as every member of MIS can think of significant contributions, but it should be emphasized that, even though the length of the list is important, it is not the only, or even the main, criterion. It is paramount to realize that these contributions have been made by scholars, or practitioners, who were having the kind of good time Simon was referring to. Let us keep this kind of spirit alive.

CONCLUSION

A preoccupation with the actual state and possible future of any scientific field is a legitimate and necessary epistemological quest. However, it is not an easy task. Such a preoccupation should be received with great care as it can be loaded with questionable implicit assumptions about the very nature of science and scientific work. It can be supported by monistic models such as Kuhn's which may seem appropriate to the past and present state of certain scientific disciplines such as physics. But, as it has been argued in this article, such a monistic model cannot be used to properly understand the actual state and possible future of the MIS field.

MIS is a fragmented field or, to put it in other words, an essentially pluralistic scientific field, especially in view of its vocational character. It can thus be understood and analyzed only with the help of pluralistic models. Such models exist and Whitley's can be brought to contribution in the case of MIS with great benefits. It has been used here in an attempt to analyze the characteristics of MIS and understand the ways and means of change in these characteristics.

Any scientific field is a perpetual and continuous social construction [1] that can be influenced with the proper tools. MIS can be changed but it will never be by a simple decree aiming to reduce it to a portion of itself or to make it into something it is not. Changes will result from the action of colleagues-competitors working on both the foundations and the emergent parts of MIS. This field is attractive to many, including the authors, because of its great variety of approaches

and their potential and actual cross-fertilization. For example, the practical implications of alternative perspectives on organizations [2, 9, 39] or on IS development [27, 28] have not yet been fully drawn. Members of the MIS field should not refuse any help from other disciplines given the richness and complexity of their main research object, management information systems, and their numerous facets. There is room in MIS, and so should it stay, for the indispensable free enterprise will.

Members of the MIS field should not refuse any help from other disciplines given the richness and complexity of their main research object, management information systems, and their numerous facets.

The call for more unity in MIS has been made by some on the ground that MIS is presently degenerating in a so-called free-for-all situation. Those supporting that point of view are, in fact, predicting the death of MIS . . . unless something is done. For them MIS is presently too much of a free enterprise.

It is first interesting to note that some of those who make this plea implicitly assume that scientific knowledge has some intrinsic characteristics that distinguish it from other forms of knowledge. So the plea for more unity very often becomes an implicit plea to abide by the standards of good scientific practice and a call for less laxity in the field. Arguments tend to be rational and the assumption that colleagues are ready to accept changes if these arguments are correctly presented is made. Unfortunately, such a set of intrinsic and permanent standards does not exist. The failure of all the attempts up to now to identify and formalize them since the 1930s by the Vienna Circle members and their followers [4, 10, chap. 2–4; 21, 34, 43, chap. 2] is an eloquent testimony in this regard. Of course, scientific knowledge must abide by some standards, but these standards are socially defined and redefined with time. So that what was considered to be scientific knowledge fifty or one hundred years ago may not be so today. It is at this point that Whitley's model is interesting for MIS since it points at factors that are likely to influence the position of actors in the debate for gaining control of the definition of conditions and rules of knowledge production and validation. By so positing the problem, Whitley's model is a good instrument for reflecting on the ways MIS may evolve.

As a last word, let us return to two of the central concerns of this article: maturation and progress. In our view, *maturation* is not a one-way street leading necessarily toward the conceptually integrated bureaucracy square of Figure 1 that represents Kuhn's normal science. This is a too restrictive view for MIS. *Progress*, on the other hand, cannot be seen to occur only when there seems to be cumulation of research knowledge as was the case during the early years of the cognitive styles research theme era. To see progress in this way would be, according to Popper [46, p. 341] assuming that ". . . our mind resembles a container—a kind of bucket—in which perceptions and knowledge accumulate." Progress can also occur when cumulative research knowledge is challenged. On the matter of progress, we do share the view of Mary Hesse [20] who observes that it is pragmatic success that cumulates in science, not necessarily the amount of knowledge.

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