Management Information Systems:
The Journey of a Research Field

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ABSTRACT

We have fully read and analyze 550 articles in order to describe the MIS field and its evolution over time. This paper has three objectives. First of all, we are attempting to describe the field of MIS on several dimensions, by identifying the issues, areas, methodologies, and so on. Then, we will attempt to understand the evolution, and in particular, confirm or invalidate the hypothesis of the fragmentation of this research field. Finally, in light of our analyses, the objective is to contribute to the advancement of a recurring debate at the heart of the scientific research community in MIS: the diversity and the relevance of academic work.

1. HOW TO DESCRIBE A RESEARCH FIELD?

Describing a research field and its evolution brings with it formidable difficulties. There are numerous choices to make: "Which period should be studied?", "Which references should be retained?", and most of all, "Which model should be adopted to classify and qualify the studies?". The researchers have come up with very different answers to these questions, as shown in table 1. We have made our own choices, which we feel are important to justify.

1.1 Choosing a study base

First of all, we wanted the study period to be long enough to really be able to perceive changes. This is why we analyzed publications over the last twenty years. Few works on the description of the field of information systems have studied such a long period. Our analysis has also an advantage of being, somewhat, in line with the work of Alavi and Carlson (1989, 1992). The year 1980 seems to be a date that holds significance as this was the year when the first International Conference on Information Systems was held (ICIS, Philadelphia, Pennsylvania).

Regarding the bibliographic sources, we also noticed different choices, depending on the study. In the first works describing the field, articles published in journals or conferences labeled "information systems" (MISQ, JMIS, ISR, Info & Mgt, ICIS) were analyzed together with articles from generalist journals (AMJ, AMR, ASQ, SMR, HBR, Mgt Science) or from computer magazines and journals (Database, Decision Science, Computer Surveys, CACM)[1]. Two reasons justify this choice: information systems literature is in its infancy, the article base of information systems is relatively weak and the authors of this domain are looking to know precisely whether information systems is in the process of emerging as an « autonomous » theoretical field, compared to management and computer sciences. This is the principal question that was asked by Culnan and Swanson (1986), and was taken up again by Cheon et al (1991). The choice of journals analyzed was justified, of course, by the authors' questions. Therefore, Lee et al (1999), wanting to compare the topics of the researchers and practitioners in management science, studied works from research journals and practical magazines. Swanson and Ramiller (1993) were able to restrain their analysis to a single reference, since their objective was to understand the field's image through a new journal.

To propose a description of the research field in information systems and attempt to understand its evolution, we want to rely on an indisputable basis: the principle research journal and the proceedings of the largest conference in the field, could not be ignored. In the previous works on the field, MIS Quarterly is by far the most studied journal; ICIS was rarely as often analyzed. We feel it is important to accept a journal and a conference, so as to represent the two biggest outlets by which scientists can share their knowledge: publish and communicate. Finally, the previous works only relied on anglophone literature. It is certainly the primary literature that exists on this field, but it is also our responsibility, as francophone researchers in Information Systems, to wonder about our "own" production. The journal "Systèmes d'Information et Management", which first appeared in 1996, already offers a total of 69 articles.

[1] See the list of journals and the significance of the acronyms in annex 2.

The description of the research field was accomplished from entire articles, titles and abstracts, key words or bibliographies of these articles. Here again the objectives of the researchers imposed choices. Culnan and Swanson (1986) applied bibliographical techniques to define the researchers' independence from the field, and as a result they used the bibliographies from the articles. Barki, Rivard and Talbot (1988, 1993) had as an objective to create and validate a list of key words in information systems, with the goal of facilitating or rendering uniform the choices of words by authors for future publications. They therefore started from existing key words to propose a classification. Swanson and Ramiller (1993) were interested in research issues; the reading of the title and abstract of an article was sufficient to identify them.

Wanting to describe the domain of information systems on many dimensions (issues, problems, domains, topics, levels of analysis, epistemological, methodological…) it was necessary to go and find the information in the entire articles.

1.2 Defining a multidimensional classification

The number of retained topics to classify the works could be reduced substantially from the start: five types of research for Ives, Hamilton and Davis (1980), four reference fields for Culnan and Swanson (1986). In more recent analyses, the number is far more significant; Lee, Gosain and Im (1999) distinguished forty-eight categories. This evolution seems quite natural: the research field is developing, a more precise description is needed, and it requires a finer model; unless this is already the sign of a fragmentation of the domain. We felt we had to find a middle ground and adopt a model that was not too "loose" yet not too "tight", while retaining a number of categories that allows a precise enough description of the scientific production but that does not mask a complete view of the subjects.

Swanson and Ramiller (1993) built their classification model in an inductive manner, which presents, in our opinion, too high a risk of depending on a "technological trend". Above all we are not considering these subjects, like DSS or CCWS, to be research questions; instead these are tools, corresponding to application program domains or usage types. In this work we set out to properly distinguish the addressed subjects on one hand, and the studied technologies on the other.

We have adopted a deductive approach to define the research subjects in Information Systems. The models proposed by each of us have been compared. In the end, we retained 13 **subjects** grouped into three large themes corresponding to the three principal levels of management: *the strategic management*, *the development*, and *the evaluation* of Information Systems. In every studied article, we assigned one or two subjects. We noted, whenever necessary, the computer tool in question (e.g. expert system, E.I.S., Enterprise Resource Planning, Messaging, GroupWare…). We distinguished five large domains: (1) *the informational* domain which covers the management of data and knowledge, (2) *the functional* domain which encompasses the processing of transactions and assistance in operational tasks, (3) *the decisional* domain which deals with decision making processes and the decisions support issues, (4) *the rational* domain which includes together communication processes and communication support issues, and finally (5) a *general or undifferentiated* domain where the article refers to the Information System as a whole. The study of a particular computer tool, in itself, is not an indication of the specific domain in question. For

example, EDI can be viewed from a relational perceptive (i.e. transmission of information between organizations) or under a functional perceptive (i.e. order, delivery, invoice processing). Similarly, GroupWare can be used as a group communication tool or as group decision support system.

We are not only interested in the subjects addressed in an article; our objective is also to describe the field on other dimensions: object, perspective, level, epistemology, and methods. The **object** of a research project can be *conceptual* (e.g. the information, the decision), *technical* (e.g. the functioning of a tool), or *organizational* (e.g. the intersection between the technology and the organization). The **perspective** could be the *engineering* (ex-ante) or the *integration* of computer systems in organizations (ex-post). The **level of analysis** could be the individual, the group, the organization, or *many organizations*. The publications can be theoretical or empirical, and for the latter the **epistemology** can be *positivist* or *interpretive* and the **methodologies** diverse (surveys, case studies, research action, experiments…).

In the previous studies, references were put in classes, at best, according to two aspects: the subject addressed and the methodology used (see Ives, Hamilton and Davis as well as Alavi and Carlson). The classifications developed by Barki, Rivard, Talbot or by Swanson and Ramiller presented two limitations: an excessive level of detail to preserve a synthetic view of the subjects, and some categories defined according to heterogeneous criteria (subjects, computer tools, levels of analysis).

The description of the field of information systems that we have proposed here is really multidimensional since it concerns the subjects, the application domain, the issue, the level of analysis, the perspective, the epistemology, and the methodology. The breakdown of every dimension of our chart is given in Table 1.

Study	Ives, Hamilton, Davis (1980)	Culnan, Swanson (1986)	Cheon, Choong, Lee, Grover (1991)	Culnan (1987)	Barki, Rivard, Talbot (1988)	Barki, Rivard, Talbot (1993)	Alavi, Carlson (1992)
Period studied	1973-1979 6 years	1980-1984 5 years	1980-1989 10 years	1980-mid 1985 5 years	1977-1987 10 years	1987-1992 5 years	1968-1988 20 years
Bibliographical Sources (magazines, journals, indexes, databases)	Comprehensive Dissertation Index (engineering, math and statistics, education, information science, mass communication, psychology, business)	MISQ ICIS CACM Mgt Science AMJ AMR ASQ	MISQ, JMIS, Database Mgt Science, Decision Sc CACM, Comp. Surveys HBR, SMR, AMJ	Databases: Social Sciences Citation Index (1400 research journals + 3200 practical magazines)	MISQ Info & Mgt CACM Mgt Science Decision Sc	MISQ Info & Mgt CACM Mgt Science Decision Sc JMIS ISR Organization Science	MISQ CACM Database Decision Sc. Mgt Science SMR HBR JMIS
Work base	331 abstracts	271 bibliographies	929 bibliographies	281 bibliographies	2000 key words	2000 key words	908 articles
Number of retained themes	5 types of research defined by 5 intersections in the research model	4 theoretical fields	4 theoretical fields	42 authors 5 factors	1100 key words 57 sub-categories 9 principle categories	1300 key words 56 sub-categories 9 principle categories	9 principle categories
Classification, adopted steps	Deductive reasoning Research Model 9 variables split in 3 groups: 1. the IS 2. the processes 3. the environment 1 type by thesis + methodology	1 Work point per article (journal field in which the article is published 1 Reference point per article (major field of the journals where the cited articles are published)	1 Work point per article 1 Reference point per article	Cited authors	Deductive reasoning stemming from variables of Ives, Hamilton, Davis' model + Inductive process of classification of the 2000 key words, creation of new categories	Barki, Rivard, Talbot (1988)	Barki, Rivard, Talbot (1988) + methodological type deductive process + orientation : research or practice 1 type per article 1.83 subjects per article on avg
Analysis	Relative importance of the research types Comparison with the methodologies	Bibliographical : common quotations, comparison between the 4 fields Evolution Independence of the field of IS	Bibliographical : common quotations, comparison between the 4 fields Evolution	Bibliographical : common quotations of the authors	Creation and validation of the model	Update of the model	Relative importance of the subjects Evolution of the methods Intersection of subject/ method Comparison of research/ practice
Description of themes, types, variables or categories	1.A single variable group 2. Process x environment 3. Process x IS 4. Environment x IS 5. Process x environment x IS	1. Management Information Systems 2. Computer Science 3. Management Science 4. Organization Science	See Culnan and Swanson (1986)	1. Foundations, 2. Individual approaches to MIS design & use 3. MIS management 4. Organizational approaches to MIS design & use 5. MIS curriculum	1. References disciplines 2. External environment 3. Technological environment 4. Organizational environment 5. IS management 6. IS development and operations 7. IS usage 8. Information systems 9. IS education & research	1. References disciplines 2. External environment 3. Information technology 4. Organizational environment 5. IS management 6. IS development and operations 7. IS usage 8. Information systems 9. IS education & research	See Barki, Rivard, Talbot (1988)

Study	Swanson, Ramiller (1993)	Reix, Fallery (1996)	Lee, Gosain, Im (1999)	Desq, Fallery, Reix, Rodhain (2000)
Period	1987-1992 5 years	12/1993-09/1995 2 years	1991-1995 5 years	1980 – 1999 20 years
Journals	ISR	MISQ Info & Mgt ICIS US theses + 21 journals	ISR MISQ CACM JMIS + 5 practical magazines	ICIS MISQ SIM
Work base	397 abstracts of submitted articles	510 abstracts + key words from 800 abstracts	700 research abstracts or articles + 2300 professional articles	550 articles
Number of retained themes	275 key words, 37 categories, 9 themes	9 themes for the 510 abstracts 37 categories for the others	48 categories	13 problems, grouped into 3 themes 5 application domains 3 research subjects 2 research perspectives 4 levels of analysis 3 epistemological positions 5 methodologies
Classification, adopted steps	Inductive process, iterative 1 category per article	Swamson & Ramiller (1993) 1 theme per article for the 1st 510 Multiple categories for the others	Swanson & Ramiller (1993) + Inductive process, iterative 1 category per article	Deductive and iterative process 1 or 2 problems per article 1 choice per article for the 6 other dimensions + studied software (eventually)
Analysis	Evolution Relationships between categories Differences between articles submitted and published	Relative importance of the themes Competition between themes	Evolution Comparison between journals Differences between research journals and practical magazines	Evolution Comparison between the dimensions
Description of themes or categories	1. Computer Supported Cooperative Work 2. Information and interfaces 3. Decision support systems 4. Systems project 5. Evaluation and control 6. Users 7. Economics and strategy 8. Introduction and impact 9. IS research		*The 7 most frequented categories :* 1. System design, development, methodology 2. Advanced techniques 3. CSCW 4. IT usage and user models adoption 5. Expert systems / Agents / NLP 6. DSS / EIS 7. IS research	

2. THE EVOLUTION OF MIS

Our data come from MISQ (the entire analysis from 1985 to 1999, i.e. 314 articles), ICIS (every two years from 1980 to 1999, i.e. 167 communications), SIM (the entire analysis from 1996 to 1999, i.e. 69 articles). This sample of 550 references is therefore not totally homogeneous with respect to the « spatial » distribution (e.g. the domination of MISQ) nor with respect to the chronological distribution (e.g. few articles from the 1980-1984 period).

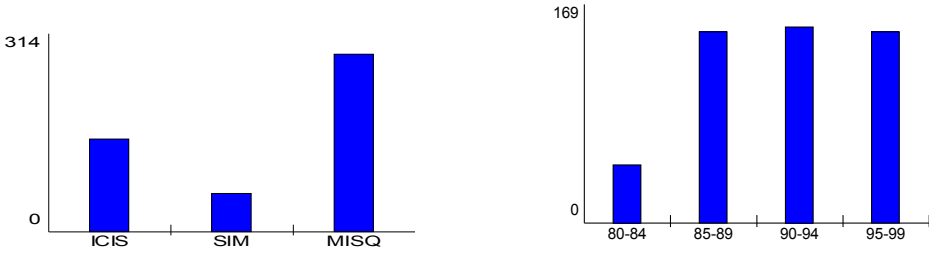

Figure 1 : Distribution of articles by journal and by period

We will present the main results of our comprehensive analysis and our chronological analysis, respectively.

2.1. A positivist epistemology, an "Ex post" vision, and a problematic of the evaluation.

Very clear results emerge from the analysis of the raw data. First of all, in 82% of the cases, the object of the study concerns the organization. Only 11% of the studies have concern technologies.

As shown in the figure2, the researchers in information systems are in majority attached to a positivist epistemology. But this epistemological positioning does not seem to impose a particular type of methodology, since different methods are regularly used, in relatively similar proportions.

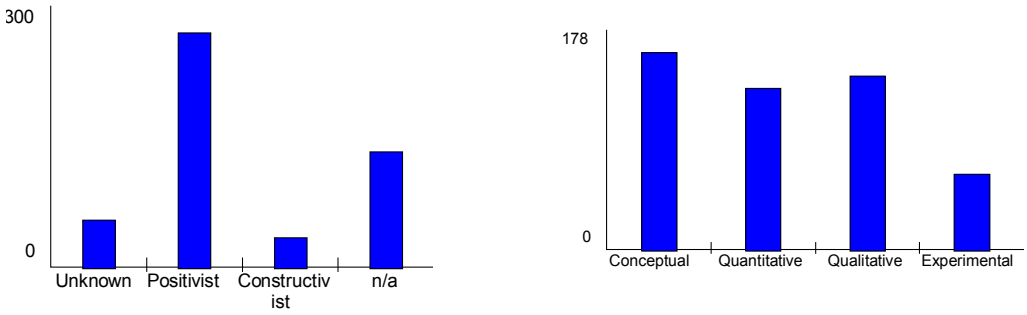

Figure 2 : Distribution of the articles by epistemology and methodology

The analysis of the distribution of articles by subjects clearly shows the dominance of the question of the evaluation, throughout the period studied.

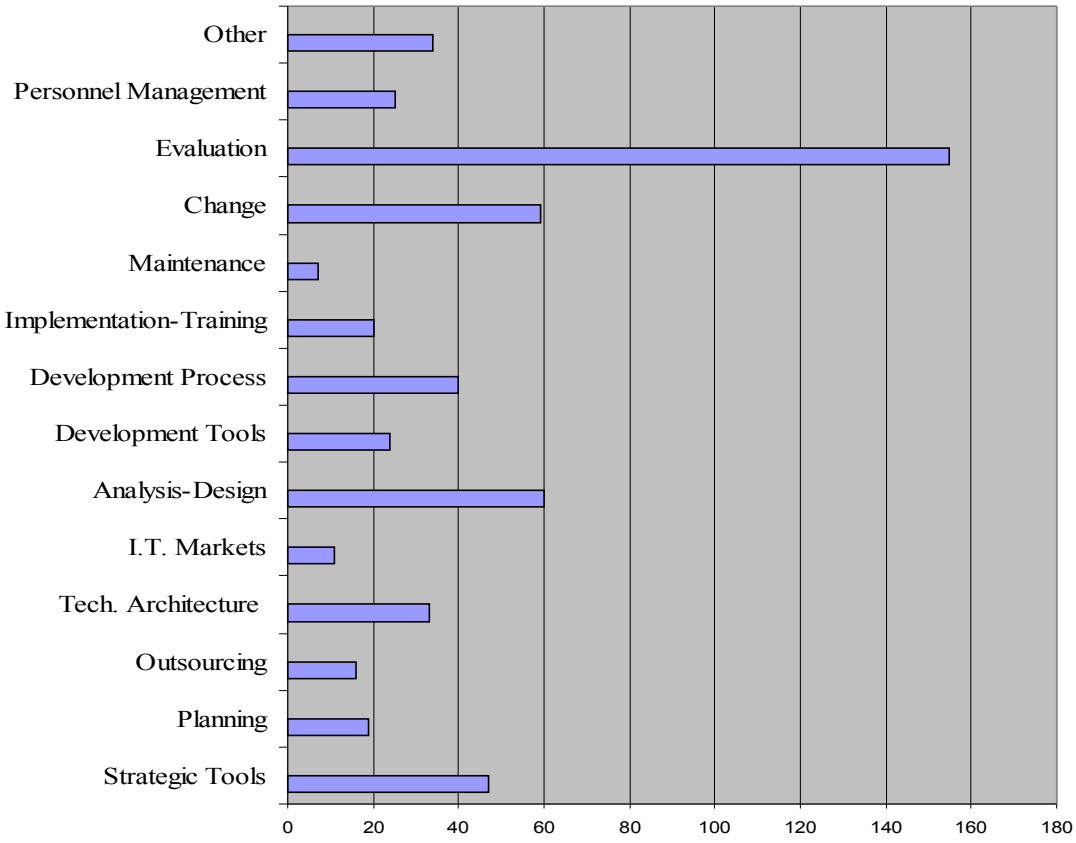

Figure 3 : Distribution of articles by subject
(an article can have one or two subjects)

The classification of the subjects into the three large themes that are defined confirms that the evaluation is represented most often. We can see that it is accompanied by the dominance of the perspectives of integration or "ex-post" analyses.

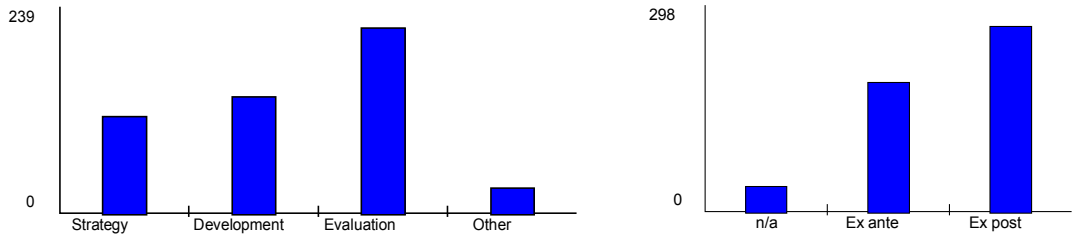

Figure 4 : Distribution of the articles by theme and by perspective

Lastly, a multiple correspondence analysis comparing the different dimensions is interesting, in the sense that it highlights three groups and their direction of research in the domain of Information Systems.

- The first group characterizes the research on the subjects of development from an engineering perspective (ex-ante).
- The second group (largely dominant) characterizes the research centered on the subjects of evaluation from an ex-post perspective with an epistemology that is mostly positivist.
- The third group includes the research centered on strategic management, which favors the organizational or inter-organizational level, and has an epistemology oriented towards constructivism.

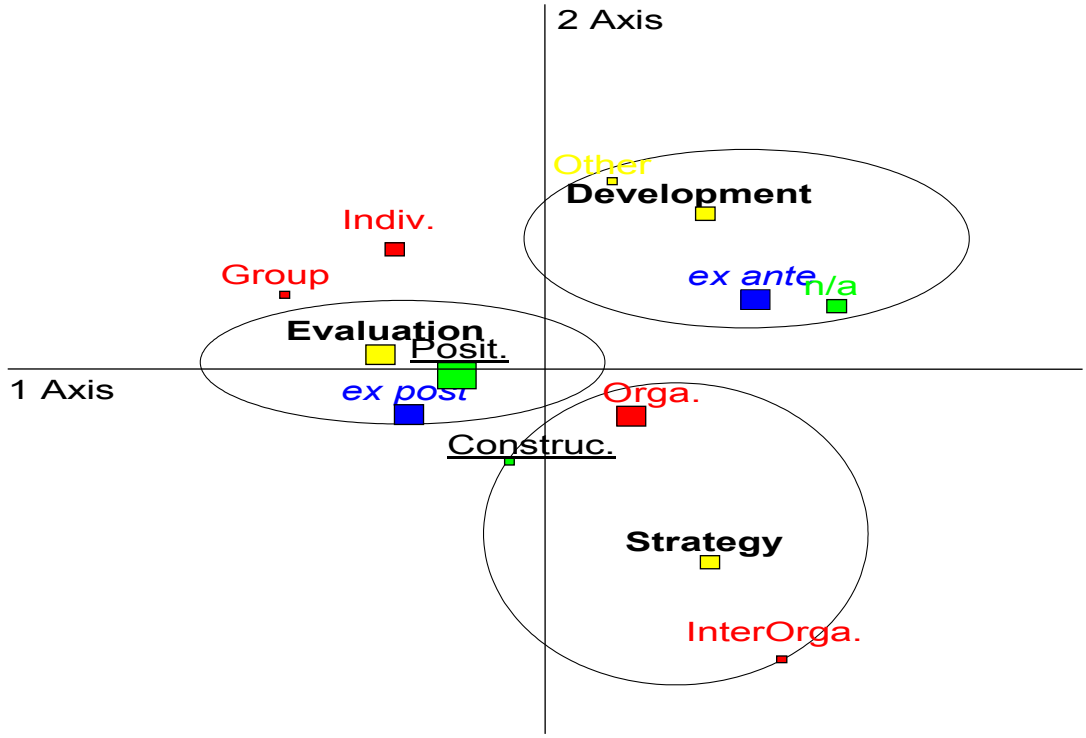

Figure 5 : Intersection of the research themes, perspectives, levels of analysis, and epistemologies

2.2 A research field in perpetual evolution, but centred on the Evaluation of Information Systems

The most obvious general characteristic that comes out of almost every chi2 test, is of course the evolution of the research field: when we seek to compare the different five year

periods with every successive dimension, all the chi2 tests are statistically significant, except for the methodologies. In the tables presented here, when we read the partial chi2/ total chi2 percentages, the sign represents the difference between the observed and the expected values, and the bold values are the most significant.

The table 2 compares the three big subjects of the field and the four periods, and we can see that the problem "Design/Development", which was without a doubt the foundation of the discipline twenty years ago, has lost its initial importance. The view of information systems as a "strategic tool", hardly considered before 1995, has become important, but **the field continues to be mostly characterized by the general and constant consideration of the "Evaluation"**, which is very characteristic of the Management Sciences.

Problems/periods	80-84	85-89	90-94	95-99	TOTAL
Strategy	**-12**	-1	0	5	126
Development	**35**	10	-2	**-22**	151
Evaluation	-5	-3	0	4	239
TOTAL	38	161	159	158	516

THE DEPENDENCE IS STATISTICALLY SIGNIFICANT (chi2 = 36.1, d.f. = 6, p = 98.9%).

Table 2 : comparison of the themes and the periods

A precise representation of the subjects, year by year, presented in Figure 6 shows that the X' axis can be interpreted as the axis of time (except for the unique cases of 1985 and 1991). The dependence by way of the chi2 is largely significant, although the number of cases with a theoretical result less than 5 makes the interpretation rather delicate. **The old problem of "Design/Development" opposes itself to "Strategy", which is rather new, and to "Evaluation", which is still current.**

The year 1985, which was atypical, is that of Harvard colloquium on the strategic aspect of I.S. And the year 1991, also atypical on the factorial axis, can be explained by the regained interest in the new development methods (RAD, Objects…), or more simply by editorial politics unique to the journal MISQ…

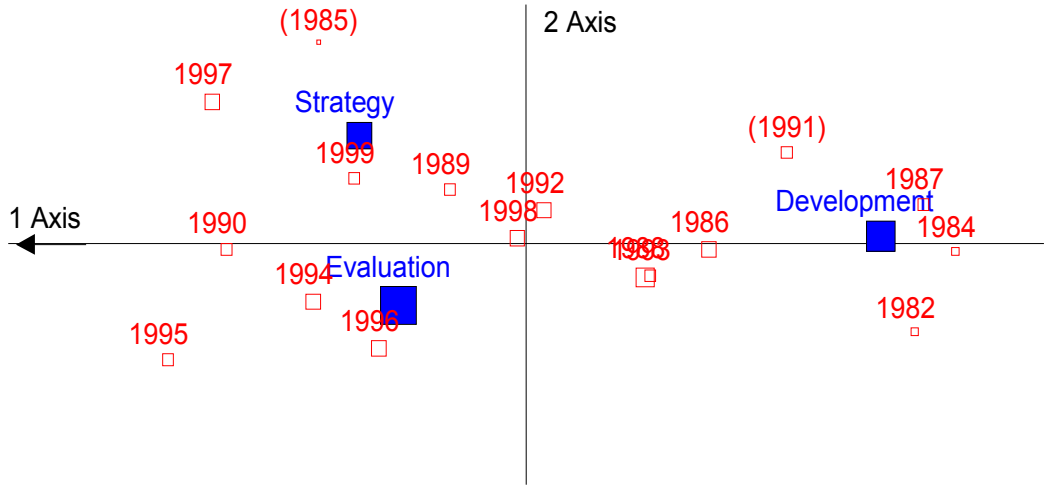

Figure 6 : Intersection of the themes and the years

The following table compares the four domains and the four periods, and we can see a reversal starting from 1995: the domain of the "decision making", still having a majority, has lost its importance relative to the "informational" domain. **We can see the new preoccupation, mostly academic, with the management of knowledge**, organizational learning… We can also see a more professional preoccupation with data mining and the storage of information.

Problems/periods	80-84	85-89	90-94	95-99	TOTAL
Informational	0	**-10**	**-14**	**31**	30
Functional	3	0	-2	0	42
Decision Making	1	7	3	**-17**	84
Relational	-8	-1	3	0	57
TOTAL	15	62	50	86	213

THE DEPENDENCE IS STATISTICALLY SIGNIFICANT (chi2 = 51.0, d.f. = 9, p 99.9%)

Table 3 : Comparison of the application domains and the periods

An evolution with respect to time can also be seen in the two following tables: diversification of the **levels of analysis**, and a lighter diversification of the **epistemological positions.**

Starting from 1985, the level of "individual" analysis, which was dominant, dropped at the expense of the "organizational" level that became dominant. And starting from 1995, the positivist perspective, which is still as dominant in our field, is no longer alone. A minority constructivist perspective today can be found.

levels/periods	80-84	85-89	90-94	95-99	TOTAL
Individual	**32**	**-7**	**-8**	5	148
Organization	**-12**	**8**	2	-5	271
Group	0	-2	5	0	28
Inter-Organizations	-6	-2	0	5	20
TOTAL	36	135	133	163	467

THE DEPENDENCE IS STATISTICALLY SIGNIFICANT (chi2 = 25.0, d.f. = 9, p = 99.7%)

Table 4 : comparison of the levels of analysis and the periods

EPISTEMO./periods	80-84	85-89	90-94	95-99	TOTAL
Positivist	-9	-1	2	1	300
Constructivist	0	-6	-10	**30**	39
N/A	**20**	6	0	**-15**	149
TOTAL	34	156	138	160	488

THE DEPENDENCE IS STATISTICALLY SIGNIFICANT (chi2 = 76.2, d.f. = 6, p 99.9%).

Table 5 : comparison of the epistemologies and the periods

As shown in the following multiple correspondence analysis that links the different variables with one another, there are clearly **three big poles** in the research field: the "Development/Decision Making" pole, characteristic of the 80s, the "Evaluation" pole, constant throughout the field, and the "Strategy/Informational" pole, characteristic of the 90s.

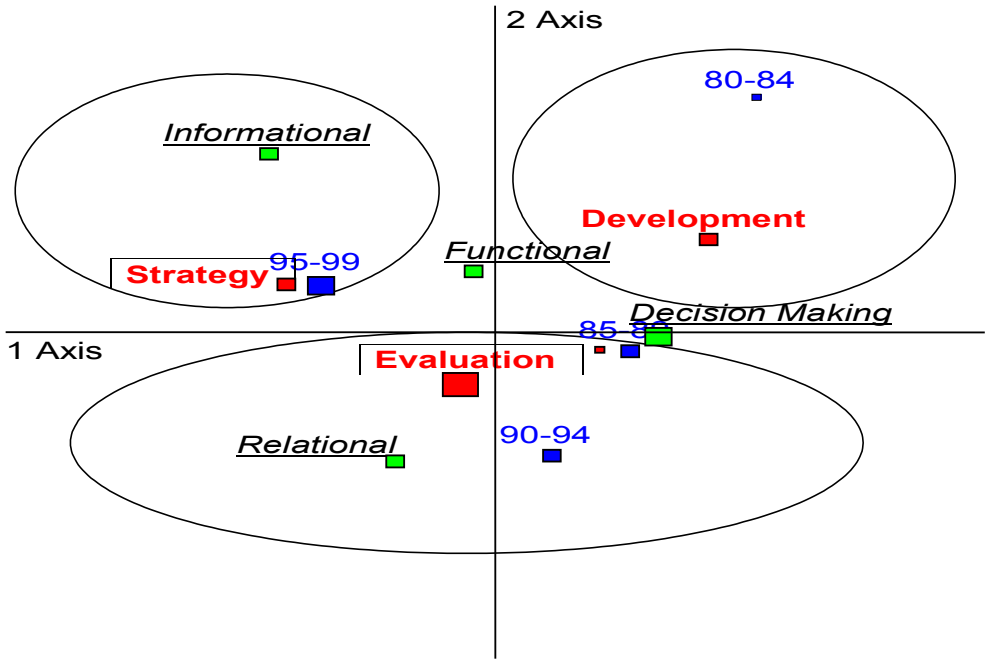

Figure 7 : intersection of the periods, the themes, and the domains

In conclusion of our analysis, the hypothesis of the fragmentation of the research field cannot be confirmed. The predominance of the organizational object and the persistence of a dominant preoccupation with the evaluation show, on the contrary, the emergence of a stable positioning of the subjects in the management sciences. Indeed, the definition, of a limited number of categories of analysis and the use so few sources can render this conclusion contestable, by introducing a favorable bias to the homogenization. However, the fact that so little research was classified in the category "other", constitutes a certain argument in favor of the rejection of the fragmentation hypothesis, and rather confirms the identity of the field characterized by three sets of subjects.

3. THE RETURN OF AN OLD DEBATE: DIVERSITY AND PERTINENCE STILL IN QUESTION…

Rather regularly, the research community in information systems questions itself on the future of the discipline: its readability, its pertinence, its autonomy, or its long term viability (Reix 92, Fallery and Reix 96, Swanson and Ramiller 93). The study on the "core" of the scientific production in the last twenty years confirms that the prior questions did not find definitive answers, particularly regarding two major characteristics:

- diversity, or the heterogeneous character of the publications,
- practical relevance, or the level of adequacy for the needs of the professionals in information systems.

The first characteristic was studied in the journal Information Systems Research, in its last issue of 1996; the second characteristic was again recently debated near the end of 1999 in MIS Quarterly. It is from these two journals, very representative of the academic conceptualization of the field, that we will borrow the essential terms of the debate.

3.1 Perils and promises of diversity

Asking the question of diversity, is, in a certain sense, asking the question of identity: a fragmented field, unstable becomes hardly readable; easily qualified as "transversal", without homogeneity, can be condemned to disappear by dismantlement and absorption within the neighboring disciplines.

As per Benbasat and Weber (1999), the diversity can be characterized according to three dimensions:

- the diversity in the methods of collection and treatment of the data,
- the diversity in the theoretical foundations and reference disciplines,
- the diversity of the topics and the problems undertaken.

Our analysis showed a (weak) growth in the methodological diversity. The information systems field, dominated for a long time by a positivist epistemology supporting quantitative methods, has opened itself progressively to interpretive and constructivist approaches and to qualitative research. This evolution, common to the different management science domains, does not seem to raise any particular problems.

Resorting to the variety of reference disciplines (economics, sociology, psychology, cognitive sciences…) seems to be a shared characteristic, to differing degrees, by the other domains of management science. In the sense that the researchers in information systems are part of a work field that is distinct from the reference fields, the risk of loss of identity associated with this pluralism of theoretical bases seems weaker now compared to a few years ago, without having totally disappeared. In effect, this theoretical diversity controls, in part, the evolution of the field; as per Benbasat and Weber (1999), "a discipline attains and maintains its place among the others if it has developed a powerful and general paradigm, accepted by the majority and distinct from the other disciplines. To rest on the reference disciplines is a short term vision leading to the loss of identity, because it does not promote the emergence of a community of researchers".

The diversity of the problems undertaken is a reality that cannot be challenged: the study of twenty years of literature (though centered on a principal journal and a conference) clearly confirms this feeling of fragmentation of the field, linked to the multiplicity of the

subjects undertaken: variety of the research questions, variety of the perspectives, variety of the levels of analysis….this seems to be the dominant rule. There are many explanations originating from this diversity:

- the varied origins of the researchers combined with a dominant structure of individual works, the absence of long term research programs, conducted by strong teams, has a certain impact,
- the ambiguity of the founding concepts such as the information and variety of assessments with respect to the technologies in the discipline,
- the evolution of the technology with the appearance of easily spotted trends (expert systems, individual computing, collectivists, Internet, Enterprise Resource Planning …).

The diversity is a reality; should we consider it as a menace (and if so, what are the particular measures to limit it ?) or, on the contrary, like an attraction (and therefore, what can be done to maintain or develop it ?) ? We will satisfy ourselves in summarizing the arguments from the two sides to open a necessary debate.

The dangers of too much diversity

For Benbasat and Weber (1999), diversity exposes significant risks.

First of all, diversity leads to a weak visibility of the discipline, compromises its identity and its legitimacy. Without an agreement on certain fundamental specific questions, the field becomes ripe for a hostile takeover; the surroundings too narrow with its reference theories can then lead to a pure and simple absorption. In the absence of a unifying paradigm, the information systems field is diverse and fragmented.

Then, diversity renders exchanges between researchers more difficult: the obstacles to communication increase, and the conflicts emerge. The proliferation of the theories that are evoked, like that of the research topics, can lead to the emergence of the field, while the scientific community hasn't even reached a sufficient mass. The risk of explosion is added to the risk of absorption.

Lastly, too much diversity slows the appearance of the cumulative effects in the discipline, because of the scattering of the efforts it entails. Diversity can look after itself on its own.

To limit the risks, the measures are relatively few in number; editorial politics of the principal scientific journals, the selection practices of the scientific committees for conferences and the decisions of the authorities on evaluation, constitute the essential elements of control for published research. The rest is a problem of individual choice.

The interest in considerable diversity

The pessimistic analysis of Benbasat and Weber is not shared by all the members of the MIS community. Robey (1999), taking up certain arguments from Landry and Banville (1992), believes that diversity presents certain advantages:

- it widens the foundations on which is built the knowledge of the field ; to resort to multiple paradigms reinforces the quality of the knowledge produced,
- it contributes to the attraction of good researchers from other disciplines into the field of information systems; theses researchers are interested in possible applications of their theories,
- it stimulates creativity; in a research field where a dominant paradigm has imposed itself, controversies are rare, conformism is mandatory and originality is poorly rewarded,
- it is the expression of academic freedom in the choice of questions to study, like the choice of knowledge development methods.

The leaps from subject to subject, from method to method …(that we have been able to observe in our analyses), the tendencies to refer to multiple paradigms is, for Robey, signs of strength and not symptoms of weakness. Diversity is to be encouraged in a free scientific community where principles of mutual respect, of responsibility, of trust and scientific honesty must be followed; the information systems research field must be one of "disciplined methodological pluralism".

As we can see, the discussion on diversity raises formidable questions. The debate is not only scientific, it is also of a political nature ("How is the production and the dissemination of knowledge managed?"). It is not independent of the fundamental educational debate: "What should be taught in MIS?" ("What is the knowledge that is really specific to our discipline?"). The problems of boundary, of legitimacy and of identity are inevitably asked as soon as we address the question of diversity; all this shows the importance of what is at stake.

3.2 In search of practical relevance

Periodically, the disciplines of management question themselves on the apparent break up of the perfection of research methods and the weak utility of the scientific production published for the practitioners. Every few years, the journal MISQ has published articles related to questions judged to be important by practitioners (Brancheau et Wetherbe, 1996). In a recent issue (vol. 23, n°4, 1999), the journal reopened the debate on the search for the balance, for the information systems discipline, between rigor and relevance.

For Applegate (1999), the problem of the practical relevance of research ("relevance to practice" can be summarized into four questions:

- Does the research in information systems produce the knowledge that the professionals can apply to their daily work?
- Does it concern itself with the problems that the professionals in information systems are confronted with?
- Is it focused on the technological and managerial questions of practical interest?
- Are the research articles in information systems accessible to the professionals?

To properly place the terms of the debate, we will first of all present the position of Benbasat and Zmud and then follow with the replies of the other contributors.

Benbasat-Zmud: the defense of relevance

For these two authors, the report is clear: if we consider the essence of North American published research, the answer to the four questions is clearly negative. Practical relevance is weak because of reasons of content (choice of topics, definition of the problems, absence of the synthesis of results…) and for reasons of form (inaccessible style, theoretical jargon, too big a share of questions on method, lengthy and poorly illustrated articles).

This shortage of relevance has a variety of origins:

- first of all, greater emphasis placed on rigor rather than on relevance, in a young discipline which, to become respectable, must first give priority to satisfying the academic demands on quality (affirmed theoretical models, rigorous practices in the collection and treatment of data),
- weakness (or absence) of cumulative tradition leads to a weakening of the resulting models, unlikely to support normative prescriptions. The multiplicity of reference theories, the weak number of properly validated measurement instruments, the proliferation of the concepts…in short, the diversity referenced earlier is an obstacle to the improvement of relevance,
- rapidly changing technology that encompasses an inevitable time lag between the instant the problem surfaces and the availability of rigorous research results,
- insufficient exposure of academic researchers to field problems,
- strong impact of the driving forces of research in the academic world more sensitive to rigor than relevance.

Having established this diagnostic, Benbasat and Zmud formulated many suggestions specific to improving practical relevance:

- the selection of research subjects must be directly guided by the future interests of the captivated parties (journals, colleagues, students and practitioners),
- the researcher should first look at the practical to identify the relevant subjects and only look at the literature after the subject is chosen,
- group reflection is necessary to identify the essential research questions likely to make an impact on the field,
- researchers must first concentrate on the results rather than on the concepts and the methods,
- researchers have a tendency to produce general models with few variables ; the practitioners await rich models adapted to a specific context. We must, as a result, accumulate varied research on the same topic and produce a periodic synthesis centred on its application,
- relevance will be improved if researchers develop a frame of analysis that is "intuitively significant" to the practitioners,

- the large majority of articles should be presented in a simple manner, concise and clear to be accessible to the group of potential readers of the journals,
- the chief editors and the scientific committees should re-examine their review and decision process to improve the conditions of equilibrium between rigour and relevance.

At the end of the day, practical relevance must be looked for on four principal dimensions: the interest (the problem), applicability (the solution), opportunity (the time frame), and readability (the form). Such a proposition in defense of relevance can only result in stimulating a variety of answers.

A variety of responses

We will present here the positions of the different contributors: positions that will either reinforce or, on the contrary, limit the initial position of Benbasat and Zmud.

Davenport and Markus essentially come back on the terms of the debate on rigour-relevance. For them, Benbasat and Zmud do not go far enough into their critique of the lack of relevance: it is indispensable to envisage more profound changes in the organization of research. Their discussion touches on four points:

- the other management disciplines are not models that our discipline should imitate: instead we should make reference to law or medicine. The principle of cumulative research opposes itself to relevance, in a domain where change is so rapid,
- to see relevance as the application of theoretical research to practical questions may not be the only solution. Publishing evaluative research or the analysis of politics being practised represents an interesting alternative; this type of research is more adapted to generalist journals with extensive readership (Harvard Business Review, Revue française de Gestion….); the academic community must encourage this type of publication,
- in spite of the criticism towards the consultants (their supposed lack of rigor) we must recognize their superior ability of identifying the needs of the users, and their ability to respond quickly. Instead of criticizing them, we should inspire ourselves with their methods,
- our graduate and post graduate students are important “consumers” of research; we must offer them a “consumable product”.

Lyytinen, in his critical essay (1999), first brings up a problem of definition: practical relevance deserves to be better defined. It is necessary to have a complete vision of the relationship between the academic world and practice, to properly define what is meant by practice (which practitioners?), to analyze the professional image of the researchers in information systems, to interest oneself in the politics of the incentives of research in information systems and also in the manner in which we study what we call practice. He underlines the absence of a critical mass of researchers in information systems and the lack of long term perspective that results. The researchers, in particular North American, are strictly specialized and work on their own; they have a weak understanding of technical problems (therefore the practical problems that follow) and, as a result, have an image that is looked on poorly by the professionals. He questions the contents of the notion of relevance: “Is relevance something that suggests an immediate solution after reading an article in MISQ or is it what can restructure or bring up the way to see and react to the long term?”. He thinks that,

without regulation, opposite to what Benbasat and Zmud affirm, relevance will rapidly overtake rigor. Lastly, he regrets that the analysis ignores what he thinks is most important in a debate on relevance: the link between research and education. If we want, as researchers, to be read by professionals, let us teach our students, our future professionals, to read research articles, even if their form is sometimes inaccessible; (it seems impossible, if not inappropriate, to describe, in every case the complexity of the problems studied, in common language).

Lee criticizes the perspective adopted by Benbasat and Zmud, because, according to him, it is limited to a positivist vision of the discipline. Traditionally, this vision does not retain the applicability like an essential criticism of the quality of a study. It is necessary to distinguish the science on one hand, and the technique and the profession on the other hand. Historical analysis shows that the improvements in practice do not always come from theory and that theory has mostly had a role of validating and explaining existing practical solutions. Besides, the instrumental notion of science, where the researcher is the producer of knowledge and the practitioner is the consumer, is not the only one possible: science can (and must?) have a critical attitude towards practice, and social relations at work….that, of course, causes the problem of relevance in very different terms from those retained by a vision of research that is purely instrumental. Moreover, by means of education, research can reveal itself relevant according to different mechanisms from those proposed by this instrumental perspective.

We limited ourselves to a quick presentation of the principal arguments to initiate a debate; even without limiting oneself to the instrumental aspect of the problem, the question of relevance of the research in information systems remains unanswered because, in the long term, the survival of the discipline is dependant on the continuous search for the answer.

However, before undertaking the eventual changes profound in consequences, it seems desirable to better understand the position of the different professional milieus that are concerned. The notion of relevance is a concept that is relative to the users; it would be risky to postulate on the homogeneity and the stability of the demand for knowledge inside the world of professionals. It is therefore important to proceed first of all with an in depth diagnosis on robust empirical analyses.

CONCLUSION

Our work, of limited dimension, cannot arrive at definitive conclusions. A widening of the base of analysis seems to be necessary. The anxiety about the future of the field is not totally dissipated even if our study allows us to qualify some affirmations that have been made, without a doubt too hastily.

A recent, still contested discipline, the management of information systems seems to suffer at the same time from its large fragmentation and from its limited instrumental relevance. The two faults are not entirely independent, as the previous discussion showed. Crudely asked, the essential question would be: “Must the MIS field concentrate itself on a few essential questions not taken up by neighboring domains and of incontestable interest to the community of practitioners (for example, organizational engineering in a broad sense) or must it continue its research constantly renewed by different theoretical references, without

particular attention to the professional world, in an attempt to affirm its academic legitimacy?".

The choice does not only have theoretical implications: the consequences in terms of training content, in terms of the politics of education within the learning establishments, of editorial choice, and of evaluation practices are important. The periodic return of the debate shows that the question has not yet been resolved and that it is important to facilitate the emergence of a diagnosis shared by all the players.

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Annex 1

RESEARCH THEMES AND DIMENSIONS IN INFORMATION SYSTEMS

THE SUBJECTS

1. Strategic management

1.1 Strategic use of information systems, information systems with competitive advantages, strategic vigil, technological vigil
1.2 Information systems planning, planning methods, flow charts
1.3 Outsourcing, management of investments in information technology
1.4 Technological architecture, centralization/decentralization, distributed architecture, client-server, organization of technical means, open systems, data administration
1.5 Information and IT economy, IT market, macro-economic level, diffusion of IT and innovation, international comparisons, intercultural differences

2. Development

2.1 Definition of the needs in information, analysis of existing needs in information, diagnostics, design methods, modeling, data models, processing models, object methods
2.2 Development tools, languages, programming, Computer Aided Software Engineering, re-utilization
2.3 Development approaches, projects management. Development cycle, prototyping, Rapid Application Design, evaluation of project risks, estimation of development expenses, methods of project management
2.4 Introduction, training of end-users, program tests, acceptance, conversion
2.5 Maintenance of information systems

3. Animation, evaluation, control

3.1 Management of organizational change, re-engineering, organizational consequences, appropriation of technologies, promotion of the diffusion
3.2 Evaluation, measures of use, of satisfaction, of performances, quality of the software, computer audits, computer security
3.3 Management of computer scientists, competencies.

4. **Other** (for example "ethics and information systems")

THE APPLICATION DOMAINS or USAGE TYPES

A1 INFORMATIONAL
A2 FUNCTIONAL
A3 DECISIONAL
A4 RELATIONAL
A5 GENERAL, non differentiated, mixed…

THE DIMENSIONS :

D1 : OBJECT OF THE RESEARCH
- T : technique
- O : organizational, information systems
- C : conceptual, information, knowledge

D2 : PERSPECTIVE OF THE RESEARCH
- BE : before, engineering, ex-ante
- AF : after, integration, ex-post

D3 : LEVEL OF ANALYSIS
- I : individual
- G : group
- O : organization
- IO : inter-organizational, macro

D4 : EPISTEMOLOGY
- P : positivist
- I : interpretative
- T : theoretical, normative, compilation

D5 : METHOD (for the empirical articles)
- Q : questionnaire, quantitative inquiry
- INT : interviews, qualitative inquiry
- EX : experimentation
- CS : case study, ethnography
- RA : research-action

Annex 2 :

LIST OF ACADEMIC JOURNALS STUDIED IN THE WORK ON THE DESCRIPTION OF THE FIELD OF INFORMATION SYSTEMS

Information systems journals :
ICIS : International Conference on Information Systems
Info & Mgt : Information and Management
ISR : Information Systems Research
JMIS : Journal of Management Information Systems
MISQ : Management Information Systems Quarterly
SIM : Systèmes d'Information et Management

Generalist journals :
AMJ : Academy of Management Journal
AMR : Academy of Management Review
ASQ : Administrative Science Quarterly
HBR : Harvard Business Review
Mgt Science : Management Science
Organization Sc : Organization Science

Computer journals :
CACM : Communications of the Association of Computing Machinery
Comp. Surveys : Computer Surveys

Database
Decision Sc : Decision Sciences