

Reflections on an Qualitative Study

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Abstract:

This paper considers questions of research method and process in Information Systems research. The context for the questions is the current debate within IS about methodologies and their underlying philosophies. The empirical content is supplied by the experiences of the researchers while conducting 12 interpretive case studies over periods ranging from one meeting to two and a half years. Looking back, the research process can be seen to have comprised eight primary activities: "framing the questions", "meeting people", "asking questions", "analyzing responses and other material", "writing", "criticizing one's own work", "refining" and "reading". Each activity continued throughout the research. Issues for the research included both the limitations of the methods chosen and the relationships between "researchers" and "researched". The paper concludes that as IS researchers we are not immune from the intellectual currents of our time. It invites "philosophers and others" to join us in the search for coherent and rigorous research methods and calls for a more liberal, pluralist approach to IS and IS research.

Reflections on an Qualitative Study

Introduction

This paper considers the research process employed in a major empirical study of how organizations evaluate their IT investments. The principal concern is not so much the results of the research, those are to be written up in more detail elsewhere (Farbey et al 1998 and forthcoming (a) and (b)), as the research methods employed in the research, how and why they were chosen and how they worked out in practice. It also looks at the implications for the conduct of research and how research is reported to academics and practitioners. It is therefore a methodological study. A European perspective is taken, and contrasted to an American one.

Research method is a continuing problem in Information Systems research. The problem is first a foundational one: Information Systems is a relatively new field of study, not yet at ease with itself, nor yet quite a discipline - precisely in the sense of not having commonly agreed research procedures. It is still struggling to individuate itself from other related disciplines including Computing Science, Software Engineering, Systems Science, Information Science, Cybernetics, Management and Organizational Behaviour. The question "What is IS?" arises both in explanations to skeptical outsiders (Avison, 1995), as well as in conferences (Wand and Weber, 1990; Angell and Straub, 1993) or Delphi studies within the profession (Backhouse et al, 1991; Glasson, 1996).

There are signs of progress. IS research and teaching is well established in many parts of the world. The 1960's saw the development of the earliest University courses in the USA and Europe. Successive IS curricula for graduate and undergraduate courses have been created and published since the early 1970's, sponsored by both professional and academic bodies such as the Association of Data Processing Managers (ADPM), Association for Computing Machinery (ACM), IFIP, the British Computer Society (BCS) and the Council for National Academic Awards (CNAA).

In Europe and elsewhere outside the USA, institutional structures are beginning to develop. Some of these have been established recently as a response to the failure of IS to win full recognition from other academic disciplines and institutions. Examples, which indicate the current status of, IS include professional or learned societies, well established, continuing journals, senior academic posts and conferences.

Societies include the (American led) Association of Information Systems (AIS), the European Chapter of the AIS (established 1994) and the UK Academy of Information Systems (UKAIS) formed specifically to promote IS in the UK (established 1994). There is too the International Federation for Information Processing (IFIP). IFIP is organized into a number of technical committees (TCs) each made up of several working groups (WGs). Information Systems is represented by two thriving TCs - TC8 and TC9. TC8 is primarily concerned with issues of Information systems in organizations, whilst TC9 is interested in Information Technology and Society.

There are many IS journals of standing in the academic community in Europe, for example, the European Journal of IS (EJIS), Information Systems Journal (ISJ), the Journal of Information Technology (JIT) and the Journal of Strategic Information Systems (JSIS) were all established in the early 1990's. In the USA the Management Information Systems Quarterly is a little older (MISQ, 1985) and there are several other US journals devoted to IS including Information Systems Research (ISR, 1991), Information and Management (1977) and Information Society (1981). Authoritative journals include Information Systems Management, Information Management, The Journal of Organizational Computing, the Journal of Management Information Systems and Information Resources Management Systems. The Anbar Management Magazine lists 38 journals worldwide, not including MISQ and ISR.

These journals are often fed by established conferences such as the International Conference on Information Systems (ICIS) and its European cousin (ECIS) and many other conferences such as those sponsored by the IFIP WGs under the auspices of TC8 and TC9. Alongside these the discipline has launched successful Ph.D. colloquiums at national, regional and international level.

In the UK, IS academics are negotiating for separate status in Research Assessment exercises carried out for central government. Although there are Chairs in Information Systems (the earliest in Europe came into being in the early 1980s at the London School of Economics and Political Science in 1982) and people appointed to lectureships in Information Systems, many departments are small and/ or not self standing. Currently researchers tend to be subsumed for the purpose of the Research Assessment into Management, Computer Science or Information Science (librarianship) departmental assessments, or form an awkward alliance with an expedient "other(s)", for example Industrial Relations and Management (Research Assessment 1996, Information Systems Department, London School of Economics).

In other words the IS community is recognizable to itself if not always to outsiders. There is nevertheless not yet a degree of disciplinary identity comparable with more mature fields of study.

A second complicating factor in IS research follows from the first. If the research field and community are only loosely defined and run across the boundaries of several existing disciplines, it follows that what constitutes an acceptable procedure in a particular research project is not pre-determined. Researchers starting out on a new piece of research need both to choose a procedure and justify it.

In practice they find a number of conflicting schools. There are those who emphasize the technological (IT) component of an information system - the hardware and software and the design methods used to bring the technology into productive service (An example is Falkenburg et al (1995). A second group regards an information system as a social system which uses technology (notable examples are: Land, 1975; Mumford and Weir, 1979; Lyytinen et al,1991). A third group stresses the systemic nature of an information system and bases both analysis and design methods on systems theory (Checkland, 1981; Checkland and Holwell, 1998).

The first group on the whole see themselves as scientists, engineers and technologists and adopts the methods of the sciences. The British Computer Society for example, has a close association with the

Engineering Council. This group often makes use of laboratory type experiments, for example, by taking a group of MBA students and measuring their performance and variations in performance in relation to some IS task.

Within the social systems group there are those who look to science and the classic scientific method as the appropriate reference for IS research and those who look to the humanities, for example anthropology and sociology, for research models. Some researchers adopt qualitative methods believing them to be especially appropriate, others rely on quantitative ones (Benbasat, 1990; Boland, 1989 and others in the same volume; Lee, 1991; Markus, 1983 and Markus and Benjamin, 1997; Rohrbaugh, 1989 and others in the same work). There are debates as to the relative importance of "rigour" and "relevance" (Keen, 1990). Researchers in the third group have developed the widely discussed notions of soft systems (Checkland 1981, Checkland and Holwell, 1998; Schafer, 1988).

All these debates matter to the researcher, and not only conceptually as part of academic debate. Few teachers or researchers adopt a pluralistic approach. Despite attempts to develop a contingency framework (Galliers and Land, CACM, Galliers, 1992) which set out to link the nature of the research problem to the most appropriate research method, the arguments are being rancorously institutionalized in professional groupings and in the journals. IS and IS research are in danger of "Balkanisation". This makes it more difficult to present a united front to outside institutional assessors with all that implies for future funding and recognition. It also means that researchers are not only choosing and justifying a method; they are choosing a philosophical position, a research sub-culture and a possibly a professional faction (see also Cornford and Smithson, 1996, Chapter 2).

A third, external, complicating factor is the rapid pace of change in organizations and in technologies. Wherever the organization enters as a subject of research it is necessary to recognize that it is in a state of continual flux so that the subject today is not necessarily the subject of tomorrow. The assumption of stability "cet par.", which so often provides the foundation for statistical work, is problematic. Since much of IS research is applied, there is a potential risk that by the time the research is complete, its subject will no longer exist. Further, the practitioners with whom we work are perfectly aware of the irrelevance of such research. Under these circumstances what are the most valuable lessons that can be abstracted (salvaged) from research and what difference might this make to the choice of methods?

To this we can add the rapid advances in technological capabilities, combined with the adoption of these technologies in innovative ways, for example, the spread of the Internet. These frequently leave the academic community scrambling to catch-up and searching for ways of explaining the new phenomenon and its likely impact on business and society. If a test of a mature scientific discipline is its ability to predict futures, or at least to accommodate newly discovered phenomenon in existing paradigms, the academic discipline of IS fails the test.

This paper seeks to explore the choices made by the authors in the course of a particular project and to ask whether their experiences in the project can shed some light on the complexities of choosing a research approach. It briefly describes the research project, its motivation and objectives, the substantive results and conclusions. It looks back to examine individual phases of the work and the

issues they raised, emphasizing methodological issues. It looks next at the day to day research effort required, the practical difficulties and the effect of the research on the organizations in which the research was undertaken. Beyond that it seeks to compare and contrast the different research methods used, their strengths and weaknesses, the theory as well as the practice. Finally it considers the cultural context in which the research was carried out, locating it within the post-modern and looking briefly at the implications of that view for what we have done.

Our position

Given what we have written above, it is necessary to define our own position before starting on the body of the paper. In broad terms it would be that:

- contemporary information technologies offer genuinely new media for information handling and communication
- the technologies are in themselves immensely complex and a proper subject of scientific interest
- the development of the technologies is already of interest, both as an historical phenomenon (Caminer ed., 1996) and under the rubric of the sociology of technology and innovation
- their widespread deployment, not least in the work-place, raises serious human and organizational and societal issues which are of academic as well as practical interest
- these issues are multi-faceted and often best illuminated by focusing a range of different "searchlights" positioned by scholars versed in a range of different disciplines: psychological and physiological (HCI), sociological, managerial, technical, political economic and others.
- the research methods which are applicable are contingent on the question being asked and the balance of interest inherent in a particular situation. There is no one best way.
- input to the research process itself is pluralistic. It can comprise both interpretation, that is the social construction of the phenomenon under review by the researcher, and the recording of real world objective data or facts. For example a budget forecast is represented by a specific numerical value. However the creation of that budget forecast may involve a complex interpretive process.
- we recognize that an interpretive research process is akin to a hermeneutic process (Davis and Lee, SOS; Lee, 1994), and that the researcher has to be aware of the problem of the double hermeneutic: that the researcher in seeking information from a respondent may be imposing an interpretation on the respondent's own interpretation of past events

Moreover, our own interests are in the human, social and organizational context, rather than the strictly technical. Our experience suggests that in practice information systems are always part of a greater whole and in general, organizational issues and choices overwhelm the technical ones. The problems we have encountered in practice are generally ill defined, not "tame" in the sense of being able to define and get agreement on the questions of significance before the start of the research.

Because of this we have a bias towards softer and qualitative research methods, at least as a starting point for research, without at all eschewing quantitative methods where we think they will be valuable. We wanted to look at "whole, messy situations in real-life contexts" because that is where

we see the most serious problems¹ The methods in the study described in this paper are all qualitative.

At the same time we recognize and are concerned by problems of proof and validity. To the extent that the research is interpretive, that is relies on the interpretation of the phenomena being studied by both respondents and researchers, there are problems of providing proof:

- that the phenomenon is described objectively by both groups
- because all researchers have to make choices about the degree of detail studied (the granularity), that the choices have been made objectively
- that the causal relationships between elements of the phenomenon posited or deduced by the researchers are valid.

The study

Background and motivation

The study was carried out by the authors between 1993 and 1996. The nub of the research was an empirical study of 12 projects in 9 organizations. The characteristics of the projects and their host organization are summarized in Table 1.

The study followed an earlier research project in the same area that is the evaluation of IT investment (Farbey et al, 1993). It was enhanced by three other studies, the first a short study by the authors for a government department on Benefits Management in 1996 and the second a Ph.D. study undertaken in collaboration with the main three year study (Canavet, 1996). The third was a study undertaken as part of an MBA project in Portugal (Santos, 1997).

All four were part of a wider climate of concern about the quality and usefulness of current methods of evaluation. At the outset of the research the outline of a contemporary response was beginning to emerge in the literature, as reflected in calls for a more accurate and broader view of what was to be evaluated (Earl, 1989), including for example portfolio evaluation (Ward, 1990). There were calls too for a more holistic approach (Galliers, 1995, Wolstenholme et al, 1993).

The wider purposes of evaluation were just being given due recognition, for example the contribution of evaluation to organizational learning (Symons, 1990) as well as the political and persuasive effects of evaluative activity (Farbey et al, 1993). This was accompanied by calls for further exploration of softer methods for determining costs, benefits and risks in I/S development generally (Walsham and Symons, 1991; Walsham, 1993; Keen, 1995; among others). Even the more traditional methods were seen as being in need of development, for example by paying more attention to accounting systems in theoretical work and in practice (Willcocks, 1992) and laying greater emphasis on post-implementation audits (Farbey et al, 1993).

¹ This comment about our approach was made by Peter Reason. We think it apt and are grateful to him.

A very specific driver for the evaluation of IS investment was the so called "productivity paradox" - the difficulty of finding any statistical evidence linking productivity, and in particular the productivity of white-collar labour, to investments in IT and IS (Landauer, 1995; Brynjolfsson, 1993 among others).

Beside the general concerns about evaluation there were specific questions arising from the authors' earlier research. First, the previous research had looked only at projects that had gone ahead. That research had relied on people's recollection of what had happened on the one hand, and was by definition confined to continuing projects on the other. We were interested to discover the decision-making processes as they happened and to find out about projects that had been curtailed. Second, for the same reason the previous research had given little feeling for project "dynamics" i.e. how project decision making processes evolved across the lifetime of a project. Third, a method for matching project type to evaluation method had been developed empirically in the earlier work. It worked effectively but lacked a strong theoretical basis and we wished to investigate this further.

The research therefore was not starting de novo. It was addressing well-established concerns of both practitioners and theoreticians and there was already a substantial body of research addressing these concerns. The researchers were not novices, either with respect to research or the topic of evaluation (Land, 1975). We began with ideas about research and the substantive area. Above all we began with ideas about Information Systems. The process of research was bound to be influenced by our previous experience in the area.

The research process

The principal formal stages in the research were:

- making a research proposal designed to obtain funding
- formulating the research problem(s)
- choosing a research method
- choosing research subjects and gaining access
- data gathering
- data validation and verification
- data analysis
- data interpretation
- definition of limitations and gaps in the research

In practice these stages overlapped and interacted so that one was always moving around a network of activities as depicted in Figure 1. These included:

1. "*framing*": There were four parts: a) formulating the objectives and research questions, b) choosing a methods or methods, c) understanding the specific characteristics of the methods chosen and d) determining the criteria for research sites, for example large organizations vs. SMEs
2. "*meeting*": gaining access, first to the organization as a whole and then to individual members or groups within it and keeping that contact going
3. "*asking*": interviewing, observing, intervening or reading organizational literature

4. "analyzing": analyzing research material
5. "writing": papers, research notes or reports
6. "criticizing": one's own work. This would include presentations to people outside the project at seminars and conferences for example, as well as debates amongst the research team and discussions with people from the organizations being researched.
7. "refining": in the light of the criticism
8. "reading": choosing and reading the reference literature(s)

The arrows in Figure 1 indicate a roughly circular process starting with research questions and ending with new ones, informed by reading and research philosophy. This process is akin to the process of hermeneutic inquiry (Davis and Lee, SOS) in which the search for a satisfactory interpretation sets off a new cycle of inquiry when an anomaly or inconsistency is revealed in the previous cycle.

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Taking these in turn and expanding each stage:

1a) Framing - formulating the research problems

One of the determining influences on the research was the ongoing intellectual partnership of the researchers. We had worked together previously on the same topic and were still convinced of its importance. The questioning did not stop at the end of the first project. The debate about objectives for a new project therefore took place informally over a period of months prior to the submission of the research proposal and well before the formal starting date. As new questions began to crystallize and became more subtle or old ones re-asserted themselves, we moved towards a formulation of the problems as we saw them that was sufficiently sharp to attract both sponsorship from a research funding body and we hoped, interest from organizations.

There was no formal problem structuring process (systematic problem structuring methods exist, as described in Rosenhead, 1989). Looking back one can see that we took a more stately route towards developing a common discourse and with it a commonly agreed perspective, objectives and approaches. It was effectively an emergent research strategy, rather than a planned one and, given its genesis as the gradual drawing together of three minds, falls closer to "strategy as perspective" than as "plan" (Mintzberg, 1991). This research approach is closely related to Ciborra's (Ciborra, 1994) notion of bricolage as applied to management planning.

The specific objectives with which we began, and on which the funding was based, were to:

- reinforce and amplify existing knowledge about the decision processes surrounding strategic investment in I/T for a project or group of related projects
- find out more about the role of evaluation at different stages of the life of projects or strategies which depend in high degree upon investment in I/S
- find out more about the influence evaluation has on the decision process
- find out the benefits and problems of using a wide range of evaluation methods

- develop and validate a method for matching the evaluation of the I/S content of a project with an appropriate evaluation technique
- identify and characterize best practice.

This part of the frame changed surprisingly little during the project. On the one hand this could be attributed to a robust formulation at the outset. It is also though a function of the way research is funded. Once started there is an obligation to produce something approximating to that for which one is being paid, or go back to the funding body to negotiate new questions. In this case although many new questions arose in the course of the research, the original ones did not go away. The responsible strategy we thought was to answer the original questions and then seek more funding for the later ones. This paper is the start of that second process of new formulations and new questions.

1b) Framing -choosing a research method

As discussed above, our choice of research method was primarily influenced by the situation as we saw it and by our preference for studying human and organizational issues. The broad choice was between a large scale quantitative study, a survey probably, of a range of organizations to complement and flesh out what we had found in the first study, or smaller scale, qualitative studies, probing the results of the first study and updating them i.e. going deeper rather than broader. The operative constraints were the nature of the problem and the specific research focus.

The problems were "messy" rather than "tame". "Tame" problems are those where it is possible to define and get agreement on the questions of significance before the start of the research (Rittel and Webber, 1973). We did not see that the problems we were addressing in the research were tame, rather we expected the detailed questions and their relative importance to emerge from the course of the research. Our reasoning was that there was too much variety and movement in what we had seen to allow us to neatly parcel off a chunk for pre-arranged questions.

The problems were complex. Complexity in this case includes a great variety of stakeholders and stakeholder interests, a variety of available methods for making investment decisions, each resting on numerous, often unstated or unrecognized assumptions, and complex objects of research, namely socio-technical systems

Our concerns were in process and interaction: that is not only what decisions were made, but by whom, to whose advantage, how they evolved and the role of evaluation and in project dynamics. Our previous research and much other writing (for example OASIG, 1996) had indicated that projects did not move smoothly along a pre-destined life cycle. We were curious as to what happened to projects, particularly those that did not make it to implementation. This suggested a longitudinal study, following projects across the life cycle or equivalently, choosing projects in different stages of the life cycle from strategy formulation through to post-implementation. Longitudinal studies are not uncommon in IS (Drummond, 1997; Kaplan, 1996, based on Rogers 1983; Newman and Sabherwal, 1996)

In the earlier project we had observed a wide range of sophistication in evaluation techniques and practices. In this one we were concerned to document "best practice" and to discover whether there were changes in practice from what we had previously observed.

All of these, coupled with our own bias towards soft methods, led us away from the large-scale survey, quantification and positivism and towards case studies, qualitative and interpretive methods with a mildly critical flavour (Hirschheim and Klein, 1989). In particular we chose to do case studies using action research. The action research was complemented by semi-structured interviews with key decision-makers and co-operative inquiry (Reason, 1988). We also made use of company documentation including Annual Reports, in-house magazines and project documentation.

c) Framing - specific characteristics of the methods chosen

Every research method has its *raison d'être* and characteristic features (Harre, 1985). It will have a history of use in one or more fields and a research "pedigree", including the arguments for and against its application in a given situation, a history of its use in IS and underlying principles whereby knowledge is recognized as valid.

In the research we drew on several methods and approaches using the methods of case studies and action research and taking a grounded theory approach to the analysis of data.

Case studies

Case studies are a form of empirical investigation in which "a contemporary phenomenon is studied in a real-life context" (Yin, 1994). Case studies are used to "describe, explain and explore" (Greenwood, 1994).

The use of case studies is an accepted research technique in IS as in other social studies (Galliers, 1992; Bailey, 1992; Greenwood, 1995; Eckstein, 1979; Kennedy, 1979). Characteristically case studies are appropriate when (Yin, 1994):

- the boundaries between the phenomenon that is being researched and the context are not clear
- there are many more "variables" than "data points"
- there are multiple sources of evidence
- the significant questions are "how?" and "why?". In this research the questions were: how and why do people evaluate IT investment, or not?

Studies may consist of the analysis of a single case or of several. There may be a single unit of analysis, or multiple units. For example, in the present study one organization provided three IT projects for study. In the first phase they were treated as separate studies; later they were combined to form a multiple, embedded case.

Cases, or aspects of cases may be retrospective, relying on documented records or the respondent's recollections. Or they may be on going, and genuinely contemporary. In the former, relying on the recollections of respondents the problem of the double hermeneutic mentioned earlier

in the paper is of particular concern. In Information Systems cases are often one-off studies. They may also be, as on this occasion, medium- or long-term studies.

An individual case may be significant. A single case is sufficient to refute a theory. A single case, because of some specific or unusual characteristic, may point up an important issue which is not generally visible (Greenwood, 1994). For example, in this research all the organizations were large. One case though was extremely large, one of the largest in Europe and this case revealed, as though through a magnifying glass, problems of scale.

The value of a case study is primarily "revelatory" (Yin, 1994). Careful research may reveal phenomena or relationships which are otherwise inaccessible. As Greenwood and Yin suggest, a case can also provide a critical test of theory. As against this, case studies suffer from a lack of rigour, generalizations are difficult if not suspect, they are almost impossible to repeat, and the studies take time. No two researchers are likely to produce identical descriptions or analysis of the same case. This last is an important issue in judging the validity of a case and is taken up again later.

Action research

Action Research is a process whereby researchers "engage with participants in a collaborative process of critical inquiry into problems of social practice in a learning context. It is designed to foster learning by both researchers and practitioners and asks of them that they reflect together on practice and "alternative ways of constructing it" (Argyris et al, 1982). It is characterized by inclusion of the research "subject" in the work of reflection, intervention by the researcher in the workplace and by a cycle of "look, think, act" in which all take part.

Action Research is principally concerned with diagnosing "a problem in a specific context and attempting to solve it in that context" (Mannion and Cohen, 1994). It is usually collaborative, self-evaluative and should, but may not always, contribute to theory. Its principal justification lies in the improvement of practice.

In its pure form Action Research rejects positivism in favour of interpretive methods, including "positivist" notions of rationality, truth and objectivity. In their place it seeks personal knowledge. Where positivism strives for distance between the observer and the observed, Action Research emphasizes participation and collaboration. It seeks "authentic insights" rather than "universal truths" and characteristically draws the observer into practice, while at the same time seeking to raise awareness amongst the participants. Historically the aim is to develop and liberate, as well as inform (Toulmin and Gustavsen, 1996).

The tools of the Action Researcher can include direct intervention in the real situation by taking an active role in the progress of a project as well as simulation of the real situation using techniques such as game playing and role-playing. It is characteristic of Action Research that there is participation by actual stakeholders in the project. Simulations taking place under laboratory conditions or using a group of, say, MBA students would not be regarded as Action Research.

The purpose of the research varies too. It may be concerned with observing a phenomenon in order to provide an explanation of what made the phenomenon occur. Here the researcher seeks to find causal relationships between elements of the phenomenon. In the evaluation research, for example, we sought to describe the observed relationship between the decision process in an IS project and the use of evaluation techniques.

Education has been a prime site for Action Researchers (for example Mannion and Cohen, 1994; Carr and Kemmis, 1986) as has the workplace (Toulmin and Gustavsen, 1996). Action Research is a common technique within IS research, in more or less dilute forms.

Co-operative inquiry

Co-operative inquiry is just one of a group of approaches to action research which is based on a group working together (Reason, private communication). It is singled out here because it encouraged us in our belief that our informants were both able and willing to reflect on their own situation in a way that benefits IS research. This is not necessarily true of all social research methods, for example in social survey work. In co-operative inquiry the researcher works "openly, directly and collaboratively with the primary actors in their various fields of interest" Characteristically co-operative enquiry: establishes a dialogue between researchers and the people with whom they work in order "to discover and realize the practical and cultural needs of those people". It is "part of a developmental process, including education and political action" (Reason, 1988).

It is rooted in the concerns of the practitioner. And is meant to be "useful to the practitioner at the moment of action", rather than be a "reflective science about action" . It may take the form, for example, of a co-operative enquiry group. These are groups established entirely for the purpose of research in which the researchers act as "facilitators and educators". The distinction between observer and participant is almost entirely erased. It is a mutual journey, except in so far as the observer acts as a facilitator and keeper of continuity. In our view, and given the circumstances of the research, there is a further distinction in that the academic researcher ultimately has a further responsibility for dissemination and reporting back to a different, i.e. academic, audience.

Co-operative enquiry is recognized as valid when the conclusions of the research chime with the experience of the researchers (both practitioners and academic researchers) and with their mutual understanding of how things get done.

In IS, much of Mumford's research, for example her work at ICI management services which was concerned with working with a group of secretaries to develop and implement an office automation project for management services can be said to fall into the category of co-operative enquiry (Mumford, 1979,1995,1997). Similar sets of concerns underlie the work of Checkland (1981) and other work in Problem Structuring (Rosenhead 1989).

From the point of view of our research the important distinguishing features of action research and co-operative enquiry are the distance between the observer and the participant and the responsibility each bears for reflecting on the situation at hand. Since it was convenient to shift the boundaries

according to the exigencies of the case study, few cases in our research were pure action research. In some we moved closer to the ideal than others, for example in two cases we clearly moved towards co-operative inquiry by asking informants to keep diaries during a critical period of the project and talking them through with the authors. But interventions were made in each case and we used participant's reflections in each. It was a matter of degree.

1d) Framing - choosing the research sites

There were 12 case studies in the main body of the project. One further project was studied as part of a Ph.D. thesis, one was an MBA student project and another led to only one meeting before it was unexpectedly cancelled. The choice of research sites followed a number of criteria:

size: we chose to study large organizations and large i.e. strategic, projects

position in the development life cycle: we were looking for projects across the life- cycle from strategy through specification, development, implementation and obsolescence

contrasts: we wanted to observe two projects at each life-cycle stage, cover overall a range of private and public sector organizations applications and include a range of projects from those where the objectives were clear and the technology tried and trusted, to those where the objectives were uncertain or disputed, using radical technologies

On the whole these criteria were met. Table 1 shows the main characteristics of each site. Frustratingly there was only one public sector organization on board. Five out of the twelve cases were in the Financial sector, reflecting perhaps the huge investments made by that sector in IT and a certain unease among senior managers at the value obtained from those investments. All except two organizations were in the UK, the exceptions being in the Republic of Ireland and in Portugal. This was a condition of funding. Work in other European organizations would have had to be funded from elsewhere.

2) Meeting - gaining access

Access to organizations came principally via personal contact, with the public sector organization joining after a public seminar on the research. Some came from long-standing working relationships, implying a certain prior degree of trust and good-will. This proved immensely helpful in the earlier part of the work. One or two, like the public sector organization, were virtually "cold". These required an extra layer of introductions and a bit more persuasion and cross checking within the organization.

The initial concerns of the research sites were time, confidentiality and the value of the research to the organization. There was immense pressure on people in the work-place, both in terms of their time and the return on that time. The idea that researchers will "waste time" was prevalent, at least until the first interview. Although all the organizations were concerned to some extent about confidentiality, the issue of confidentiality was paramount for some. The promise we made was to keep published results anonymous, at sector level. Questions about what their organization would gain from contributing to the research were present, but with hindsight, surprisingly muted.

As time went on some of these concerns faded. In part the difference was simply familiarity - people became used to a researcher earnestly taking notes at meetings. Sometimes it followed an intervention that was perceived as useful. As the interviews got underway and people found the experience interesting or helpful they began in turn to take an interest in our research. This interest inevitably fluctuated. In several organizations our original sponsor moved on leaving us in the case of his/her successor which meant that new relationships had to be made and new explanations given. But we did not in the end "lose" any of our research sites, although our involvement was several times cut short because the project in question was curtailed (Farbey et al, 1997)

3) Asking - data gathering

Data came from:

- * interviews
- * attending meetings
- * telephone conversations
- * company documents and press cuttings
- * diaries kept by informants (2)

The interviews were semi-structured. An extensive list of questions was compiled covering a wide range of issues. Each person was asked only a subset of the questions. Their replies were then collated and crosschecked to provide a comprehensive basis for the case study.

Most organizations allowed us to attend at least one meeting at which decisions concerning the project at hand were being made. One organization was extremely helpful in arranging for us to follow three projects and to attend all project board meetings. Two of these projects were curtailed, one after the very first meeting we attended, and the second after nine months of involvement including six project board meetings. The third project, which went into implementation, involved us in more than twenty-five meetings over almost two years. We wrote reports for four projects in two organizations and presented our preliminary findings in a service-wide workshop for one of them.

Telephone conversations were frequent. Their primary use was to keep the contact going when there was a gap between meetings or to re-establish contact when enthusiasm seemed to be flagging. Their use in data collection was much more suspect. The very informality of the telephone contact, which promoted the relationship, worked against it as a reliable source of data. Only in one case, where the conversations provided much needed on-going explanations as to the observations we were able to make by other means, do they form part of the narrative of the case study. The conversations were later broadly confirmed from other sources.

Company documents and external sources such as press cuttings - these were after all household names - were extremely helpful in describing the avowed policies of the organizations. To our surprise house journals were often very informative, particularly given the rapid pace of change to organizational structures. They were used by management as a means of propagating and propagandizing change, including change in IS. Both the content and the language were revealing: not quite Standard English, but not quite company jargon either. Something an outsider could

grasp, although probably not entirely. For example, we could read about far-reaching organizational changes in house magazines such as the merger of two major departments. What was much more difficult to grasp was how these changes sat with other simultaneous changes in the organization of the company and their significance. Even the significance of the new departmental or role titles was not clear and certainly the complexities of acronyms, familiar to employees, were sometimes beyond us.

Over three years the pile of accumulated papers filled two filing cabinet drawers. A further large pile of papers comprised our own "research notes", a series of informal papers, written only for team discussion. It was time for the analysis.

4) Analyzing - Data analysis

The analysis of the data took place in three parts: first the individual case studies were written, second the salient issues across several cases were abstracted and third the existing writings and analyses were used as the basis for grounded theory. This last is still ongoing.

The narratives of the individual cases followed a roughly similar pattern. An outline of the strategic intent of the organization was compiled from the documentation using a common coding frame (see Figure 2), followed by a case history also linked to the coding frame and ending with a discussion of the issues for IS evaluation and management.

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The coding framework for the histories and their strategic context was first developed as a way of organizing the very large number of reference disciplines that we needed to cover to give a rounded account of what we were observing. Later it served as a device for keeping the accounts of each project coherent and comparable with each other. Each document was coded including company documents as described above and our own notes on meetings and interviews.

Coding was achieved by re-reading the documents time and time again for elements which fell under the given headings, as opposed to a one-time reading and coding of each unit of text. This was an extra-ordinarily inefficient method and not recommended for any researcher with a tight deadline. However, it had two advantages. First, repeated reading of the text itself and a comfortable familiarity with it in the end yielded a depth of insight which we believe would not have been possible with a more mechanical method of coding and analysis. Particularly in the cross case analysis we began as it were to hear the quieter themes and the dogs that did not bark. Second, each conversation remained whole and present: one person talking to another, rather than a disembodied text.

Nevertheless a comparison of several coding procedures would be valuable and we hope to get permission to archive at least some of the interviews so that we and others can perform further analyses.

The three purposes served by the framework, for narrative, for coherence across cases and as a basis for grounded theory are radically different. For the first two it is simply a way of keeping one's thoughts together and not overlooking the obvious.

For example, in one case the organization had not long since outsourced development and maintenance of their systems to a third party. We were asked to comment on the development of a new system, in-house by a newly (re)formed IS department. The principle purpose of the proposed system was not so much to produce a working system as to frighten the outsourcer and to re-establish IS as a power-base for the new IT director, in contra-distinction as it happened to the rump of the old IT section. The focus on strategy provided by the framework produced the idea of IT strategy as "ploy" (Mintzberg 1987 & 1991), as opposed to "plan" or "pattern" i.e. the observation was easily explicable in terms of one of the reference disciplines, but not necessarily so within IS.

If the coding frame in Figure 2 is to be used for the purpose of developing grounded theory however, a much stronger intellectual coherence is required. Currently we would explain it as resting on a number of propositions:

Appraisal and evaluation are meaningful only in the context of a theory of the business as defined by its strategy

The theory of the business is displayed in the strategic intent of the organization and made manifest by the strategy in use: planned or emergent

Limitations to the strategy exist because of its socio-technical nature. "Pure" strategy is bounded by technical, social, economic and individual psychological constraints, including most pertinently those on organization, and depicted in the theories attached to each of these areas.

Behaviour and change in their broadest sense are what we are able to observe or interfere with empirically. There are theories associated with each i.e. Organizational Behaviour and Management of Change which further contribute to the picture.

These constraints change as say, the technology develops or the business landscape changes and the strategy side responds, i.e. it is a dynamic framework. The construction of the diagram is meant to suggest an interference pattern.

At this level of explanation the frame is not unlike the picture put forward by Scott Morton et al in "The Corporation of the 90's" (1991) . The external environment and strategy boxes of the MIT 90's model are gathered on the left-hand side and provide the fundamental reason for investment and the sources of value. Structure, processes and individuals in the MIT 90's frame are included under the base theories of organizational design, behaviour and management of change. The technology box of the MIT 90's framework is replaced by socio-technical theories, including social theories, culture, computation, information and communication and language as well as technology. But neither their framework nor this one is (yet) connected to theory.

Specifically in this framework the dynamics of the frame need to be explained. One theory which would accommodate this and has already been broached in the IS literature is the theory of structuration (Giddens, 1984; Walsham and Han, 1990) . In this sense change would be the result of multiple, possibly inter-related patterns of structuration within and beyond the project and organization. Other dynamic models exist, for example notion of "situated action" in the work of

Suchman (1987) and her argument for the role played by plans (strategies) as elements in, rather than determinants of, action.

Further, the process of building a theory based on views from several underlying theories needs to be explicated and justified. Calls for multiple reference disciplines are present in the literature on Evaluation, for example in the work of Scriven, (1983) but are not common in IS.

These issues are taken up again towards the end of the paper in the Discussion. First however, the paper looks at the other stages of the research .

The cycle of interpretation: writing, criticizing, refining (including more analysis), and reading, which last in fact is continuous throughout the research.

Because the research was qualitative, and because more than one method was used to extract data, especial care was taken to go through the cycle of reading, criticizing and refining several times for each case. As with the initial framing having three researchers on the team proved invaluable in getting early feedback and criticism. Major conclusions were checked informally with people in the collaborating organizations.

A second line of attack which proved helpful in maintaining a systematic approach to the data was a series of informal research notes, not intended for publication or even circulation beyond the research team, but simply recording ideas and comments which might otherwise have been lost in the relatively long time-scale of the research. In retrospect a full "research diary" would have proved extremely helpful as a record of events and responses by the research team to the day-to-day action.

Results

The research process produced a multiplicity of raw findings which are being reported elsewhere (Farbey et al 1998 and forthcoming (a) and (b)). Here we report only a few, namely those which illustrate the arguments we make later in the paper.

In contrast to our previous research findings we found that formal evaluation was required for all the projects. Generally these formal evaluations took the form of ROI analyses plus a statement as to the support provided by the project to the strategic aims of the organizations. A variety of less formal methods were in use, for example adversarial debate and connoisseurial judgement, but were not always recognized for what they were. It is possible that this was due to the size of company and of the investments, which were on the whole much larger than those we had studied previously. One suspects that the harsh economic climate of the time also affected the amounts senior management was willing to authorize without a thorough justification. Other researchers are currently reporting similar results.

We found that the value of projects/ investments could seep away at any stage and for many reasons. Some were external, for example the collapse of a supplier or the collapse of a competitive market. Others were internal to the organization but outside the system, for example, re-engineering of processes rendering the system redundant, loss of senior management interest, restructuring of the

organization. Some were project related, for example, one project collapsed because of a lack of agreement on numbers in the business case.

Nevertheless, although evaluation is used more frequently than before, we still see it as a "wicked" problem. The sources of wickedness are continual change, the many purposes of evaluation (both overt and covert), multiple stakeholders, multi-factorial problems and the interdependence of projects (Rosenhead, 1989).

Moreover, evaluation is used for many purposes, which are related to but not the same as that of decision-making (Hawgood and Land, 1987). For example it can be used to call people to account or as a process for organizational learning (Stufflebeam, 1983; Scriven, 1983). It can give feedback to evaluate a decision or feed-forward into a new one (Canavet, 1996). It can be used as a marketing tool to get buy-in. It can be used to deceive as well as to illuminate.

The conflicts of interest inherent in a multiplicity of stakeholders tended to be resolved politically rather than by carefully reasoned argument. This is not new. What the project did reveal was that the political struggles visible in one project could and did reflect conflicts carried over from other areas. People (and their bosses) did not relate to one another simply in terms of what was happening in the project, but in terms of all the other places and projects where they met. There were many interlocking strands to a relationship.

The important role played by emotion in mediating the decision making process. Time and time again a process which is set up as a formal, rational process to arrive at an appropriate decision is overwhelmed by the emotional baggage carried by the participants in the process.

Discussion

Research method

Case studies

Case Study research has grown in popularity because of its potential to dig beneath the surface in contrast to scientific or positivist approaches. However, as we have said, it has been criticized because a number of strengths of positivist research - including objectivity, reliability and replicability - are absent. A number of researchers have suggested criteria for distinguishing good case study research which can help to counter such criticisms. We have chosen one set of criteria, proposed by Golder and Tellis (1993) to help judge interpretivist research and show how the research described here met those criteria.

The criteria proposed by Golder and Tellis are:

- Competence: is correct information reported?
- Objectivity: are the informants able to provide unbiased information?
- Reliability: would others derive the same information from the case?

- Corroboration: is there confirmatory evidence?

Our methodology helps to ensure that these criteria are met, at the factual level, in the following ways.

Factual information was wherever possible double-checked with company documentation: brochures, manuals, house journals and records of meetings. For some information there were other, external reliable sources such as the Financial Times . Most of the cases were after all large public companies or government bodies. Information conveyed in interviews was compared with other related interviews and again double-checked. Where there was disagreement we went back to the source, or if that were not possible, discounted it in our account. Interviews were taped, providing a check on the notes. Drafts of the case studies were sent in each case to our principal informant, for comment and correction of factual matters. Results such as the finding that all the companies were using formal evaluation procedures were based on this kind of activity. So was the finding that value could slip away and for a number of reasons.

The observations that evaluation is still a wicked problem and that evaluation serves many purposes are inherently repeatable, within a given research perspective. These are the kinds of observations which are theory driven, or theory laden, but which would be made by any observer holding such a theory. We were not exactly looking for conflict, but hardly surprised when we found it, given our starting position. (as it happens on several of the more conflictual cases conflict was reported in interviews as well, thus corroborating our view).

However, much of the material is subjective, peculiar to one person, but nevertheless significant. Respondents could, and did, provide factual information that was unbiased. But, to take the softest issue that arose as an extreme example, the anger and disbelief that followed the cancellation of one project is not just a "fact". The anger was visible to a researcher on the spot. The realization that there was confusion as to the reason for cancellation was pieced together from the interviews with both those who did the canceling and those who were "cancelled". The latter ascribed the cancellation to political chicanery or dumb administrators. The former saw themselves as behaving rationally. In their view projects which did not have a sound business case were being stopped, the project in question did not, therefore it was cancelled. The question for the researcher was not "is this objective information?", or even "which of the two is correct?" but what is to be concluded from the two different accounts and what is the significance for IS that there were two different accounts? In this study we have concluded that IS research may have underestimated the role of emotion in decision-making and the communication/ reception of the decision. Objectivity in such a case lies more in a determination to present the several accounts, not going beyond the reported views and signaling one's own views clearly, than in only reporting findings which can be agreed by any "reasonable" person.

Reliability is a more subtle question. Given the choice of question, and the same framing process, there is no reason to believe that other researchers would have elicited different answers. Where our own interpretations of a situation are used as data, for example notes taken at meetings which also have a set of minutes, these are clearly signaled in the reports on the cases. These are evidently subjective, in that sense "unreliable" and signaled as such.

But the question of the reliability and corroboration of the inferences from the documentation as a whole, including tapes, transcripts, documents etc. is in doubt, not just for this study, but for all case studies. Undoubtedly, one is left at the end with documentation as the only extant record. Case studies finish with interpretation of texts and the question is then how this material is first coded and then analyzed. We used an informal coding within a systematic framework to derive grounded theories. A more formal scheme, as one might do for a content analysis would (with hindsight and more resources) have led to more reliable evidence. It is a limitation on the study and, as we have said we would welcome further analysis by other people as well as ourselves.

Action research

Our reading in practice of Action Research was largely dictated by the conditions of access and by the degree of trust we were able to establish between ourselves and the people with whom we were working.

The conditions of access, i.e. the terms on which we were invited in to do the research, affected what we could actually do (c/f Newman, 1993). In one organization it was understood that we were allowed to attend meetings, but not intervene in the discussion unless specifically asked by the Chair to do so. One initially invited us in to give immediate advice on a pressing decision. When that advice proved sound we were allowed to stay to carry out our "own" research. They saw it as a respectable bargain. In one organization our primary role was as an "objective" sounding board and in many, if not all, the imprimatur of the "University of Bath" was expected to give extra legitimacy to decisions or strategies or political positions. Our function was to look wise and use the University logo.

Trust takes much longer than access and is easily destroyed. One of the primary benefits of the longitudinal study was that it gave us the time to build trust, so that people were more open in what they said, and more willing to introduce us within their organizations than might have been the case if there had been one interview, or several, but within a short space of time.

Trust was also a key feature in getting people to reflect on their own work and share the reflections with us. It is in fact a mutual trust: we had to let go some of the separateness of being an "objective outsider" and believe that our subjects could in fact reflect for themselves on what they did.

Confidentiality was an issue of trust for some of the organizations. The credibility of the researchers and the right of continued access depended on their trust in us not to reveal their identity and, in one case, not to seek a competitor organization as co-partners (fortunately the problem did not occur in practice or we would have had a difficult choice to make). For the researchers confidentiality was not only an issue of trust, but also an ethical issue. The policy we adopted was based on the consideration that the research should not act deliberately to hurt our hosts, either individually or as organizations. For example all internal reports would go to the person who invited us in to the organization and we asked them to be responsible for distribution within the organization. Our experience was that where they did not like what they read, they would simply ignore the report, rather than, say, leak it selectively. This must introduce a bias into the effects of the research but it is

hard to see how to get round this without gross discourtesy. It is a downside of the method. Second, we obtained formal agreement that all published work would be anonymous at sector level - "a financial organization ...".

The actions which we were researching were those involved in the decision-making processes with respect to information systems investment. The principal methodological issues were the degree to which we were allowed to participate in the action / decision-making, the extent to which our collaborators were willing to reflect critically on their own actions, how we should interpret the reflections and the ways in which their reflections and our own should feed back into the processes.

Intervention and participation

Active participation and intervention in the organizations that are being researched raises numbers of methodological questions. In each case there was a "passive/ reactive" intervention. We were present at meetings and taking notes, we did interviews. Active interventions included:

- advising the cancellation of a project
- presentation of a tutorial paper at(IT) professional conference arranged by the organization for its members
- active encouragement and calm words at a critical, nerve-wracking stage of the project
- advice on the form of appraisal to include whole-life costing
- carrying out appraisals (feed-forward and feed-back - Canavet, 1996)
- participation in an evaluation panel
- internal reports

Clearly we were not detached and did influence outcomes. Equally clearly the justification and validity of the research cannot rest on a claim to objectivity or even neutrality. In Action Research theory justification and validity come in the improvement of practice, the "liberation" of the subject of the research, i.e. the practitioner, or the authentic insight.

Our preferred justification is that there is a mutual learning between researchers and researched (with the boundaries breaking down the closer one moves towards co-operative inquiry). In the terms of Evaluation Research, research becomes formative, not summative (for discussions of formative evaluation in IS see the work of Kaplan, for example Kaplan, 1996).

More selfishly, by becoming part of the scene, part of the scenery almost, our appreciation of the unfolding projects was much closer to that of an insider than it might otherwise have been. This was a contributory factor, for example, to our recognition of the role of emotion in decision-making. It was something we had experienced ourselves. At first the reaction was to try to step aside, to be for example de-briefed by other members of the team when we felt we were becoming too emotionally involved. Late in the day we realized that the emotion was a critical part of decision making and management and therefore a phenomenon worth recording.

A second phenomenon, which we would not have picked up on, was the dense nature of the relationships between people and among groups. The realization that relationships were, as Gellner

(1991) has it, "multi-stranded", that people relate to one another in different roles and in a multitude of organizational contexts does not necessarily come out of single interviews or short term encounters and certainly not out of organization charts.

Did we learn more than if we had stood aside? Possibly, possibly not. One would hazard that we learned different things. Did we leave a footprint? Academics are disturbances in an organizational landscape. Some of the interventions have taken root, some will take a while so to do, others will make no difference at all.

The use of other people's reflections

All the research methods used informants within the organizations studied. Most were keen to inform us of their own views and interested in helping the research for its own sake. Inevitably, though they had their own agenda. Their motives could also be seen as political and instrumental.

Political moves included using the research to convey messages to the rest of the organization. This applied to individuals who hoped that what they said would reach the ears of their allies, or opponents. It was also true of the research as a whole. Reports are political weapons.

One was sometimes aware of informants attempting to convey a good impression, of themselves or of the department to which they belonged or of the organization (for outside consumption). One was aware too of overt or covert attempts at involving the researcher in organizational politics. A third reason for humouring the researcher came from some who were hoping to get access to the results before anyone else in the organization.

The political use of academic research within organizations presumably stems from the perception that it is external, or rational, or at any rate not subject to the same pressures as internal research. It is seen as summative, i.e. a means of calling people to account, and authoritative. This leaves open the question as to whether action research, or any of the methodologies followed can carry that weight of opinion and from that, how researchers can properly manage expectations so as to say "This part is objective research, but that part is not. You will learn from both, but differently".

Instrumental responses to the research(er) included people responding to questions with the answer they think you want to hear. The only way round this is to constantly crosscheck with other people's accounts of the same thing. This is (and was) not always practical and then becomes a matter of experience and judgement.

An interview is often an occasion for people to pause and reflect, taking the opportunity to contemplate what they are doing. Action Research involves precisely that as its *raison d'être*. Most of the time people are so absorbed by the day-to-day pressures, and minutiae of organizational life that they cannot actually think about what they are doing. They just do it.

In this project as on many others the interviewer was sometimes seen as a counsellor. In this project this was gently discouraged. It can be useful, but clearly raises ethical issues, particularly when the confidences are personal and not immediately work-related.

This is different from using the interviewer as a sounding board, a frequent occurrence. And neither are the same as taking advantage of the interviewer's perceived expertise - sometimes the researchers felt that they were valued as providers of consultancy at zero cost.

Due recognition of the informant's agenda and managing it over the period of time implicit in a longitudinal study is important for the quality of the research. It is the sharpest point of departure from the structured, pre-determined questioning that is the hallmark of so much good research in the social as well as the physical sciences, or from the single interview, although the questions occur in a mild form even in a single interview. As such it requires development of new criteria for the validity of research, in the emergent situation of a longitudinal study.

Instrumentality in the researcher

If the subjects of the research sometimes appeared to have a private agenda - the instrumental responses - we too had an agenda which impacted on the research.

First, the decisions as to what or was not interesting will have been influenced by the likely response of our peer group. It is more rewarding in terms of fame and fortune to produce something new, and dress it up in the overheated language of management schools, than to do something scholarly, but boring. Publication has become a touchstone of academic excellence, as has utility. Novelty is more exciting than depth and both University funding and the researchers own career argue for excitement.

Second, there is a strong temptation to emphasize results that are acceptable to the subject of the research and hide results that might offend them.

Third, there are major problems with pre-conceptions. It is only too easy to select data and interpretations to fit one's preconceptions and to reject those that do not. Certainly the problem is aggravated by departure from the "gold standards" of scientific method, replicability and rationality. We are in the territory of the irrational and "we cannot think that our thought is wrong" (source unknown). But the problem is there for pure scientists too. There is a sociology of science no less than other activities (see for example Collins (1990)).

The interpretation of findings: choosing a vantage point

The interpretation of findings has been the most challenging part of the project. After three years not much is left to the researcher but piles of text, tapes and memories, by now unreliable. This is the part of the project when the researcher is most sharply aware of Bateson's words: 'data' are not events or objects but always records or descriptions or memories of events or objects (Bateson, 1972). Or of Hampden-Turner's remark "these are better described as 'capta' than 'data' ". There must be shaping, prioritizing and letting go, but choosing which threads to follow requires a final

leap of imagination which is not only difficult but frightening. Even in the best run projects there are thousands of bits and in our own experience some small bit, left out in a spirit of consolidation and thematic arrangement, suddenly comes back as being highly significant in another context. The snatches of evidence on emotion were a good example within our own work as was the realization of the multi-stranded nature of relationships in an organization.

There are helpful and systematic procedures for coping with volumes of research data. Classification, coding and theory building can for example be handled mechanically via systems such as NUDIST. Grounded Theory (Glaser and Strauss, 1967) contains a systematic way of extracting theory from observation. Many others exist. Textual analysis is hardly new (for examples in systems theory and IS see Introna, 1993, Farbey 1985).

However, we found that as the case histories were pieced together, mosaic like, from aspects suggested by the coding frame and the theories contained within it, richer explanations and insights seemed possible, if only we had a deeper knowledge of one or other or all of the theories and a way of presentation that could bring out the multiple facets of each story. Increasingly presenting the case "in the round" became more important and more difficult.

To combat this we are currently exploring the area of post-modernism (Achterberg et al, 1990). For example an approach which might work would be that adopted by Braudel (as described in Lechte, 1994) in which the same phenomenon is described from many viewpoints. There are echoes of this in our own insistence on the concept of "searchlights": that the issues in IS investment are multi-faceted and often best illuminated by focusing the bright searchlights of more than one discipline on the problem in hand.

A second approach adopts, or rather would seek to adapt as analogy or reference the work of post-modernist thinkers in other fields, most immediately post-modern painters. As an illustration of the kinds of questions that arise from this approach and the kinds of argument that can be made one can look at a popular book on the history of art, Hughes's *The Shock of the New*. The analogy follows an historical line of argument on perspective.

Following Hughes one could imagine "traditional" IS research, like painting since the Renaissance, as providing "... an ideal view, imagined as being seen by a one-eyed, motionless person who is clearly detached from what he sees. It makes a god of the spectator, who becomes the person on whom the whole world converges, the Unmoved Onlooker". The traditional detached researcher, the "(social) scientist", is in this sense the spectator - god-like qualities being very acceptable to most.

Noting that science has moved on, that "the idea that the looker affects the sight is taken for granted in most fields of scientific enquiry today" and that this "does not mean that no clear or truthful statements can be made", Hughes writes that one artist above all came to a similar insight without being aware of the movement in science - Cezanne. Cezanne, Hughes argues, expressed in his painting not only particular motifs: rock, grasses etc., but the relationships between them and these "blossomed into an infinity of small but equally worthy and interesting truths each time the old man moved his easel or his head". The sensation of confronting an infinity of small observations (whether

or not they are worthy) is easily recognizable to the qualitative researcher. The focus on relationships is key to the work we have done. The process of interpretation and reporting is on this argument, more like composition than narration.

Hughes argues further that Cezanne's paintings are also about the process of seeing. Again this is a problem for the qualitative IS researcher: how to take the reader through the process of research so that they are better able to judge not only its validity, but its meaning and significance. Where there are accepted disciplines the problem falls away: one looks to the accepted procedures. IS does not have that luxury.

Cubism moved the argument on. Cubism sought to "represent the fact that our knowledge of an object is made up of all possible views of it: top, sides, front and back. They wanted to compress this inspection, which takes time, into one moment - one synthesized view". Research reports demand no less.

Cubists used the art of other cultures. Hughes's appraisal of this use is not kind. He sees it as exploitative, a "cultural plunder" by people who knew too little about the people and civilizations that had created it. Do IS researchers use other reference disciplines in a similar way? Do we study or teach other disciplines in our courses, or do we only use them instrumentally in our research?

Cubism gave way to other movements. Futurism for example battled with images of the machine and movement and how to capture that movement in paint. But after 1914, in Hughes's words "machinery was turned on its inventors and their children ... the worst war in history cancelled the faith in good technology ... [t]he myth of the Future went into shock, and European art moved into its years of irony, disgust and protest " (c/f Angell, 1996) [1]

In recent years, the idea of post-modernism has attracted much attention in the arts, and some in IS. Hughes does not argue the post-modernist case. He sees its relationship to the later movements in art: as kitsch, parody, ungrammatical collections of bits from random sources and does not approve.

We do see some helpful parallels in post-modernism, as helpful as any in the preceding paragraphs and also by way of analogy, that is, not necessarily taking up or defending the whole intellectual stance of the post-modernists (Harvey, 1990; Bradbury 1995). Post modernism suggests:

- the absence of a single meta-narrative or grand unifying theory. Indeed the often sought for fundamental theorem of IS may well be akin to the alchemists philosophers stone. This opens the possibility of accepting the "other" in research, a contingent approach, different forms of research including principles of knowing, discovering, reporting, judging.
- the idea of pastiche. "Pastiche is, like parody, the imitation of a peculiar or unique style, the wearing of a stylistic mask, speech in a dead language ... Pastiche is parody that has lost its sense of humour" (Jameson, 1983) . Are IS researchers doing other than wearing stylistic masks by using the techniques of other disciplines? How many papers are submitted to journals with a statistical gloss including the uneducated use of SPSS with absolutely no sense of humour? Do we do any better at sociology or philosophy?

- the idea of the museum. Papers (research reports) are written for the journals, not the public [2]. Journals are the public face of the discipline, and the arbiters of correctness. Papers take their value from the journals in which they are published, their juxtaposition within journals affects how they are read. Are the journals the museums of research? Are papers coming to the point where they have meaning only in a museum, like Carl Andre's pile of bricks in the Tate Gallery? That is how the funding bodies see them.
- the idea of value as sign and as symbol. Do systems have different values:
 - a) value as object of exchange: (for an in-house, tailored system this is virtually nil because of the specific nature of the application, apart perhaps from the second-hand value of the machines)
 - b) value in use: this is what we have studied
 - c) value as sign: this is not a first-order value. A Decision Support System, which is a system of signs, needs interpretation before value can accrue to the organization
 - d) value as symbol: for example, in this study and in a much earlier one, we found managers putting the computers within sight of clients so as to signify the organization's entry into the brave new world of IT. In a similar way we found in one case study that the organization had mimicked the systems of their customers, to symbolize commitment. EDI is often taken up for the same reason: a badge of solidarity with a powerful customer.

In considering the post-modern critique of conventional scientific methods it is worth reminding ourselves of the values espoused by Popper (Popper, 1974). Can we give up the notion of the need for refutability in erecting scientific principles? Do we need to heed his distinction between science and metaphysics? We believe we do, with the consequent question: Are we in IS a step too far from the "gold standard"?

Epilogue

All this is to say three things. First that as systems of informing and communicating which are inescapably social, it is likely that there will be thought-provoking analogies to be made with other social systems of informing and communicating: art, architecture, literature, drama, music. In particular we should have much in common with those that are predicated on empathetic observation and representation of the human condition. Some have already been taken up (for example Lee 1990). A watching brief would seem to be necessary, much as we maintain one on management ideas and technological advance.

Second, IS is not immune from the intellectual currents of our time. We have briefly looked into ideas from post-modernism to help interpret what we have seen and examine how we went about the research. Others look to, say, Social Theory, Critical Theory (Hirschheim and Klein, 1989 among others) or psychoanalysis (Seeley, 1997). In view of what we have learned from Hughes on exploiting other disciplines, we would like these brief excursions to be construed not as clumsy attempts to colonize but as an invitation to philosophers and others to join us.

Third, IS is the community most deeply engaged in the conjunction of information technology with the personal, social and industrial context and consequences of the deployment of the technology. The technology and its deployment are of great consequence, probably as great as that of machines in the industrial revolution. We have a grave responsibility to observe, participate, learn and report back. How we do that is a hugely important question. The searchlights matter. But we will do no good by smashing up each other's lenses. The community and the project in which it is engaged will not be progressed by descending into warring factions. The extreme views of euphoria and despair about the technology are not helpful, and we believe, wrong. We need a pluralism that takes account of the limitation of all method and we need the collage built from many pieces. As Louis MacNeice has it:

These, as being themselves, are apart from not each other
But from such as being false are merely other,
So these are apart as parts within a pattern
Not merged, nor yet excluded ...

(The People's Kingdom).
New draft: Reflections

Endnotes

- 1 The loss of the Titanic must also have been a blow to the esteem for technology
- 2 Papers are often written for other arenas where comparison is of the essence, for example for CV's or institutional quality assessments.

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Table 1. Case studies: sector and approximate number of employees

Organisation	Sector	Employees
1	Financial	15000
2 (3 cases)	Telecommunications	> 150,000
3	Computer Services	24,000
4 (3 cases)	Financial	515 (subsidiary)
5	Financial	
6	Defense	>1600
7	Defense	>1500
8	Public Sector (health)	N/a

Figure 1: The research cycle

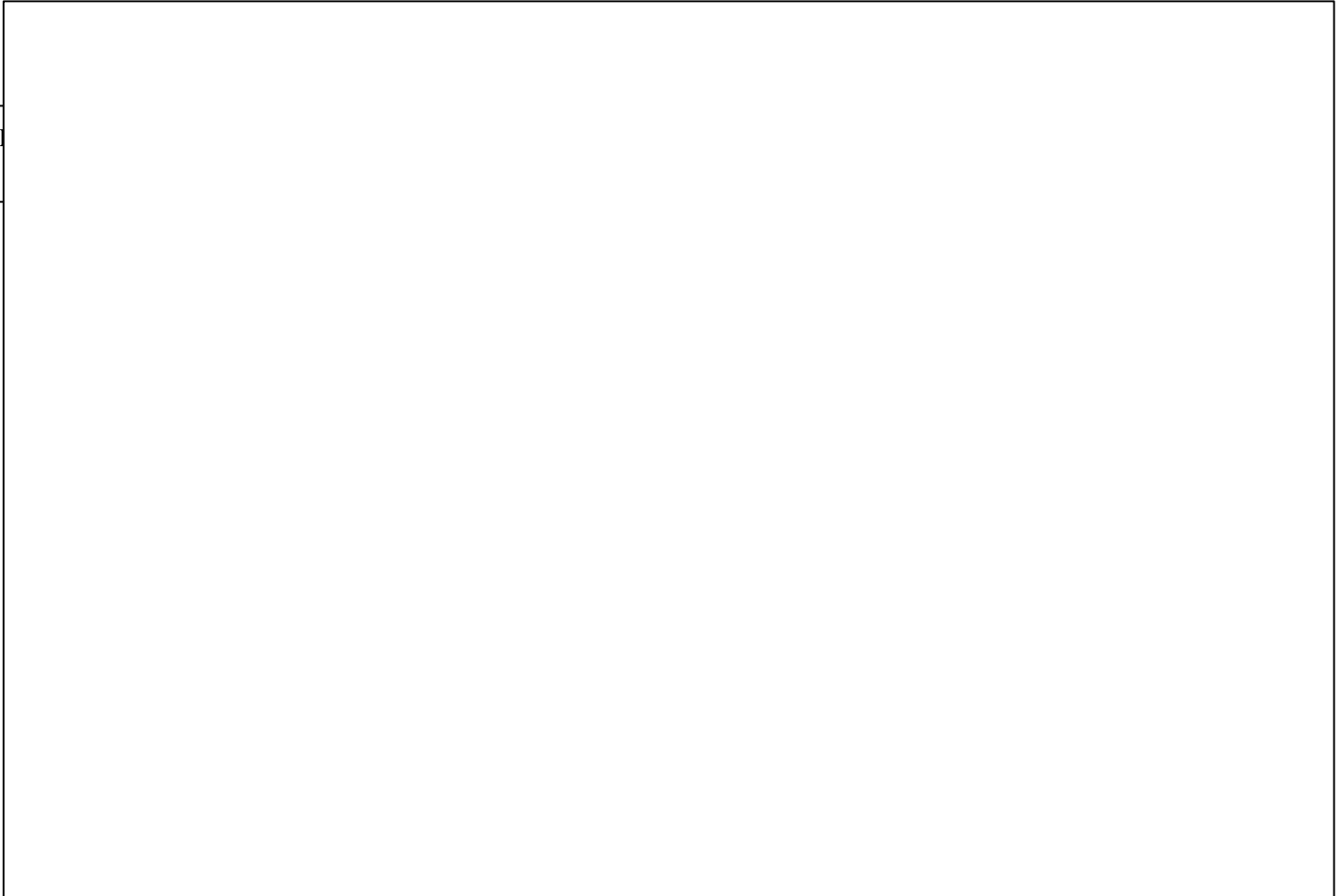


Figure 2 Framework of theories

