RESEARCH METHODS IN INFORMATION SYSTEMS
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11 SELECTION OF A RESEARCH METHOD

Lyn Antill
South Bank Polytechnic
England

Abstract

This paper describes the situation of the would-be researcher in Information Systems and the difficulties in selecting a research method for projects designed to enhance our knowledge of the information systems design process. Several conventional assumptions about research designs are examined, concluding with a defense of Action Research for studying the design process as it happens on a live case. The author then describes the background (including a great many previous and on-going live designs by the author) to a particular set of Action Research projects where the researcher, a systems analyst by profession, takes the role first of advisor to the user, and then as the actual problem owner when someone else is implementing a system on her behalf.

Introduction

Information Processing is a hybrid activity. It involves technical, personal, organizational, philosophical, linguistic and mathematical matters. This list is long but could well turn out to be longer because we are only at the beginning of studying Information Processing, and are sure neither of what it involves, nor yet of where to draw a boundary round it. It is not a well defined discipline— indeed it may never become one. It may split up into many or it may be subsumed into some greater whole. Perhaps we will come to take a more wholistic view of the discipline we have created, returning with greater knowledge and perhaps even with some wisdom, to the simplistic view of earlier days. Perhaps we will develop the intellectual tools to handle this great complexity within a coherent framework. But this framework would be on a meta-level and we would then be driven to the meta-meta level and so on (Hofstadter 1980).

An academic discipline is an intellectual convenience not a reality. It is a "frame" through which to look. It is also an environment in which the academic researcher must work. Research projects have to be planned, funds bid for, reports written and evaluations given by one's peers or supervisors or the editors of learned journals. The researcher may unthinkingly use paradigms borrowed from a parent discipline (few indeed of us have a first degree in Information Systems), or may be in doubt about the appropriate way of proceeding in a particular investigation. Alternatively the researcher confident in his or her own approach may have difficulty in pointing to precedents or arguments to defend that approach against evaluators from different backgrounds.

For the moment, we must tackle the problems at hand for the researcher in the field of Information Processing. This has become acute because of the upsurge of both interest and funds for academic research in this area. Up until now, research has often been a pre-requisite for, or a spin off from, the development of particular applications, but now there are a rash of Ph.D. proposals, as well as a large number of active academics, and we need to ask ourselves what constitutes good research.

Checkland (1981) says

> the relevance of information to be provided will derive from both the basic logic of the activity and the meanings which the actors concerned with the purposeful activity attribute to it: therefore we need ways of exploring both the basic logic of the activity and the processes of meaning attribution by which actors make sense of their worlds.

The main problem for the researcher is that any situation under investigation will combine aspects from several disciplines which would normally have been tackled by different research methods. Supervisors in Science faculties are asking for experiments to test hypotheses, others for methods appropriate to the Social Sciences, etc. As Information Processing has traditionally been regarded as a practical discipline, and practitioners take great pride in that fact, many experienced teachers have never been called upon to consider matters of research methodology and are ill-equipped to lead their students through what is now being seen as a minefield (witness the debate at IFIP 8.2 in Minnesota, August 1983). The fact is that the hybrid nature of Information Processing requires that the researcher be particularly careful in the selection of an appropriate research method for a particular situation, and in the interpretation of the results in the light of the method chosen.

There follows a discussion of several research paradigms, methods and assumptions and the implications of these for research into Information Processing. Some of the issues discussed may refer more to the mythology of research, as subscribed to by the academic community, than to the philosophy of science as expounded.

Repeatability of Results Versus the Uniqueness of an Information System

By the very act of installing an information system, one is changing the situation into which it is installed. Therefore no particular "experiment" can be repeated. This is disturbing for the scientifically trained researcher because repeatability is one of the cornerstones of scientific method. It may be possible to look at similar situations but no two organizations will ever be identical even if the only difference is that the second one knows that a system has been installed for the first with whatever results and effects on their expectations. (This also reflects the fact that the people within an IS are also capable of functioning in some way at the observer level. There are no double blind tests here.)

Thus the first explicit research paradigm that we are taught at school—that results must be repeatable —cannot be applied to the investigation of actual IS implementations.

Repeatability may be achievable for certain tests in certain situations, e.g., getting several groups of students to do a thing or observing an installation in several different branches of the same company, demonstrating that the predicted effects occur each time. This is not quite the same as the most obvious sort of scientific repeatability—that the same results should be obtained by any researcher in any laboratory anywhere in the world. The individuals and the problems are different. What the researcher must demonstrate is the variables which are held to be controlled and identical in all cases, and the fact that it is these variables which are the significant ones.

A limitation on the value of repeatable experiments is expressed by Knorr-Cetina (1981):

> [C]onstant conjunctions of events result from laboratory work which creates closed systems in which unambiguous results are possible and repeatable. But in practice such constant conjunctions are the rare exceptions—as is predictive success. Consequently the laws proposed by science are trans-factual and rule-like, rather than descriptively adequate. Thus the scientist's ability to analyze situations as a whole, to think on several different levels at once, to recognize clues, and to piece together disparate bits of information, than upon the laws themselves [i.e., he must ask himself whether particular laws would operate in the situation in which he finds himself].

What should be repeated are instances of some general principle which can be abstracted from particular observations, e.g., we can do surveys to show that the use of participative design methods increase the chances of successful design, but we don't yet know enough to be able to say, "Use ethics and you will get good user acceptance." Other factors would have to be taken into account. However, some specific subset of that statement could be framed in such a way as to be testable, e.g., a particular participative framework could be set up, along with a working definition of user acceptance.

The hypothesis may then be something like, "Groups who were involved in the X participation process make significantly more use of the Y Decision Support System than groups who had the system installed for them." This then comes within the province of statistical methods research as to what is or is not significant. The test is potentially repeatable if similar situations can be found, and may be generalizable to other types of system (although this would still have to be proved). However, it cannot be used as a guarantee of the performance of any one future group because it does not claim to have controlled all the variables. One would particularly want to know what variables the researcher thought had been controlled in order to do further research or to draw conclusions for practice.

Repeatable experiments are to test specific hypotheses. Very little research in Information Processing has yet reached the point where testable hypotheses are being put forward. Such as there are seem often to be shallow. This is not so much a reflection on the research itself but on the difficulties of being explicit about controlled and uncontrolled variables in a novel type of situation. In the author's opinion, this difficulty is caused when researchers are constrained into using a pseudo-scientific method without examining its implications. The researcher must attempt to guide the reader to see when the results may be relied upon in real-life situations.

Impartiality of the Observer Versus The Experience of the Designer

Another research assumption is that the researcher is an impartial observer of reality.

It is not only the situation which is changed by the installation of an Information System. Considerable R&D is required for the installation of any system. This adds to the knowledge of all concerned, and all have amassed experiences—good, bad, or neutral. Thus they will interpret any new situation in terms of what has gone before. No observer who is also a participant can remain impartial. This statement is worth less than it seems, at least according to the phenomenalist viewpoint, which holds that reality is what we interpret it as being. "Impartial" is thus a relative term.

It is possible for an impartial researcher (i.e., one who is not otherwise involved in the situation) to come in and make observations. These may be directly observable—e.g., who uses which programs and when—but are more likely to be reports of what people say about the system or the way in which it was designed and implemented. This has the effect of putting the observer at a double remove from the live situation. He is observing people observing their situation. Of course, there are techniques for doing attitude surveys. These might clarify the issues but they do not remove them.

Observers are never actually impartial—we all have expectations about the sorts of things we are looking for and are likely to miss clues of a different sort. This is why some of the most astonishing discoveries have been made by people who followed up what others might have regarded as random errors, e.g., the discovery of penicillin. Observers of one-off scientific experiments such as the Voyager space probes had a few ideas about what they expected to see based on their hypotheses about what the planets were like, although they were quite prepared to be surprised. Although massive volumes of data were recorded, they were of very limited types—light, X rays, radio waves, heat, etc. The directions in which these observations were made were also tightly controlled. This meant that only certain types of observations were possible. However, it is not so many years ago that astronomers were amazed to discover that they had to record anything other than light.

In Information Processing, we are only beginning to experiment to see which factors we should observe. At the moment, we all have our unstated favorites. We should always try to take account of known bias but cannot remove hidden assumptions. They will inevitably cause us to look out for some types of observation and miss others, and also to put certain types of interpretation on what we see.

Non-participant observation is most likely done through surveys and questionnaires although here the observer's assumptions about what questions are worth asking (let alone how they should be phrased) may produce more bias than the personality of the participant observer whose existing beliefs are at least exposed interactively to large doses of "reality."

What would be lost by the impartial observer is all the expertness acquired by the participants in the design exercise. This cannot simply be captured by the observer who has not shared in the experience. The expert may or may not be able to tap his own expertness to pass on the information to the researcher (Johnson 1983). New work on Expert Systems may provide other ways of formalizing expertness or experience.

Analysis of Data Versus Hypothesis Testing

A popular belief in the philosophy (mythology) of science claimed that scientists were analyzing hard facts in order to discover the underlying principles—looking to bring order out of apparent chaos. Looking for the patterns in things is after all how we believe that a new born baby makes sense out of the world, and we must still do it ourselves in situations which are entirely unfamiliar. The approach is still observable in the work of some aspiring social scientists who amass large volumes of data and then subject them to a variety of statistical techniques until correlations appear. This is in spite of Popper's (1963) assertion that scientists in fact use intuition to develop hypotheses which are then tested empirically. He claims that "Induction...is a myth...the actual procedure of science is to...jump to conclusions....Repeated observation...function ...as tests of our conjectures...the mistaken belief in induction is falsified by the need for a clear line of demarcation" around what is covered by the theories, which is not provided by the observations themselves. The faith in induction is also in spite of the teachings of statisticians about the absolute importance of the assumptions underlying the collection of the data in the first place.

This is not to say that amassing and analyzing data are not valuable research techniques, only to point out that data collection is a highly selecting process and that you will not find the right correlations if you have not collected the right data in the first place. The fact of the matter is that, in Information Processing, the data that most of us have available is our own experience as practitioners. Only in very widespread and uniform IS activity would statistical data be valuable, e.g., reactions of VDU users to certain types of physical layout or dialogue design, willingness of managers to use certain types of DSS, reliability of products created by structured techniques, user acceptance of systems designed participatively. Most of us are only drawing very tentative and superficial hypotheses from this and few of us have any real ideas for experiments which would decisively support or reject our hypotheses.

CRIS (Olle et al. 1982, 1983) was an attempt to collect all the data on available systems design methodologies in order to discover the underlying principles. What it discovered was a confusing and increasing diversity which suggested that, if there were any general principles of systems design, they lay at a deeper level. CRIS was a valuable exercise in looking for patterns even if it did not find the ones it was looking for. Much of the effort was directed to creating a framework for comparison. Researchers can now make a more informed decision about whether to amass more data of the same sort, or to proceed further on the design of the analytical framework. Probably much more of both is needed.

What most of us feel in need of are the conceptual and linguistic tools for analyzing our experiences and those of our fellow workers in order to see the patterns from which we can draw the general principles. Perhaps we can only feel our way towards these by amassing data according to some plan (preferably stated) and testing out different ideas about the tools which might prove useful.

Research on Our Own Systems Designs

Probably the first reaction of the IS researcher is to draw conclusions from the systems which he or she has been involved in developing. One, nameless, reader in an Engineering Faculty regarded this as "hopelessly unscientific." However, a framework for this type of research has been developed and called Action Research. The version described here is that of the Systems group at Lancaster as described by Peter Checkland.

The team designed a methodology for tackling complex organizational problems. The test of the methodology was to be whether it enabled the team and their students to produce solutions which were acceptable to the problem owner. Indeed the idea of the "problem owner" was an insight which emerged during the iterations of methodology testing. It is not enough to say that problems have been solved using the methodology—impartial observers can often get to the bottom of a difficult situation better than actors embroiled in it. However, consistency in getting results is a good indicator. Other confirmations are required such as whether the methodology gave problem owners and problem solvers a useful insight into what was happening, and whether the same types of results could be achieved by others using the same methodology. (How much of the insight is gained sitting at the feet of the master—or in the offices of IBM?) Cross-checking with other research methods is also required.

The analogy between Checkland's work and research in IS is a good one in the way they both deal with organizational complexity, although in IS there is a good deal more of the Action (i.e., implementation) to be taken into account. An IS has a good many stages of problem solving to go through, of which Checkland's Human Activity Systems is only the first part. Episkopou and Wood-Harper (1983) suggest there are five stages:

— Analysis of the human activity system
— Analysis of entities, functions and events
— Analysis and design of socio-technical system
— Design of user man-machine interface
— Design of the technical sub-system

If a system has failed, one would have to be very sure which one (or combination) of these had gone wrong before drawing any conclusions about the effectiveness of the methodology or its applicability in that case.

The main purpose of Action Research is to build up useful insights, expertise and case law. AR starts with an attempt to build a "rich picture" of the problem situation. Perhaps what we should be trying to do is to build up a rich picture of what is actually going on in IS—what methodologies are being used, what analysts and users feel about them, what happens at the various stages that they go through, what are the results. Individual analyst/researchers (participant observers in Checkland's terminology) will record their own insights and try to suggest generalizations from their own experiences. These generalizations would have to be tested against what was found in other situations written up within the same framework.

All IS teachers bemoan the lack of good case study material. This is also a significant handicap for researchers who are then driven to invent their own experiments, generalize from their own experiences, or give way to "unbridled intuition." One valuable service to IS research would be the setting up of a framework in which analysts could record their own activities.

Making Use of Other People's Systems Design Work

Following directly from Action Research, where the researcher is commenting on his own systems design methods, is the Case Study, where the researcher is recording the work of others. This provides several problems as a research tool, and it is probably the lack of any concerted analysis of these problems that has lead to the severe shortage of good case study material, and to the unhappiness of many teachers and researchers with what material there is.

The first difficulty is in describing the organization in a way in which the readers can get a good feel for it. Mant (1977) says that "the organization is what you perceive it to be." This phenomenological approach not only explains why different members of it have different perceptions, it could also explain why the writer's feeling for the organization (usually said to reside in the guts!) cannot readily be passed across to the reader, at least not unless the writer has the literary skills to evoke that feeling of perception in the reader. This also depends on whether the reader has the experience of something sufficiently similar. The best way round this is where readers and writer agree on the essential features to be described and on the words used in the description. This may turn out to presuppose that they have a similar view of the nature of organizations—perhaps scientists will never be able to swap case studies with politicians. Thus we need a framework for describing organizations. This may well be provided by something like Checkland's Human Activity System.

The next difficulty is similar in that the writer must select salient points to describe in the various stages of analysis, design and implementation. Here there is likely to be too much information of certain sorts (e.g., interview forms or layout charts), barely enough, and that rather sanitized, in the way of minutes of decision making meetings, and nothing, except retrospectively, of the thinking processes of the designers. Readers may come to assume either that which is well documented is that which is considered important, or conversely that the real secrets of the system have been hidden from view.

There are other problems with case studies such as the desire to protect a client's confidence. In one current study by the author, which involves a networking system between independent organizations, the study itself is beset by the problems of respecting the confidences of the individual clients from each other!

Perhaps the major hurdle for the aspiring researcher using case studies is the impossibility of "proving" anything. We have no test, other than gut feeling, for what constitutes a good piece of case study research in IS. Here we must learn from the social sciences. The object of the study must be justified and placed in context with other studies and more theoretical work. There must be a clear framework for the collection of data and for its representation in the description. The validity and usefulness of all the data must be discussed, along with likely implications for the usefulness of the study. The background, attitude and expectations of the researcher should be made clear in order to help the reader reconstruct his own perception. Conclusions should be drawn about the methods used as well as about the object of study.

Comments on the Philosophy of Science

I commented in the introduction that the working researcher had often to deal with the mythology rather than the philosophy of science. What is accepted as a good piece of research at the time is, unless its results are startling, often what the researcher's peers or supervisors feel comfortable working with.

It is interesting to note that much of the writing on the philosophy of science is concerned with the nature of hypothesis testing and refutation, and very little about the nature of observation. In epistemology there is much debate about the nature of facts. "To the objectivist, the world is composed of facts and the goal of knowledge is to provide a literal account of what the world is like—then an inquiry into the 'real' becomes an investigation of how the logic of scientific accounts preserves the law-like structure of the real." To the anti-objectivist Feyeraband, "science is nothing but one family of beliefs equal to any other." However, much of the practical work in individual disciplines is concerned with what constitutes good observation in that discipline. This seems ironic.

General Conclusions

There are a whole variety of research methods which can be used in different circumstances in IS, and to establish different things.

(1) Observe in detail and perhaps over a period of time one particular installation where the researcher is not directly involved in the installation process either as user or designer. The researcher must have a bona fide position with respect to the installation in order to have adequate access to all relevant information and to be accepted and tolerated by all parties. Pettigrew (1973), who did such a study, claimed a "belief in the value of longitudinal research designs for highlighting social

processes in organizations." He did not use one method alone: "Where practicable the present study has employed multiple methods, multiple data sources, multiple observers, and multiple levels of analysis."

This is a fact finding method of high value to those who follow on the research in that the researcher has access to a great many facets of the reality of the installation, but gives only the flimsiest grounds for formulating general hypotheses, and no way of testing them.

(2) Survey a variety of installations with which the researcher is not directly involved. This gives a more authoritative ground for generalization but the type of data gathered will be more limited by the preconceptions of the survey designer, and to the types of response that can be gained via the impersonal media likely to be necessary, and bearing in mind that there will not be the rapport which could be built in type (1). Andy Friedman at Bristol has just done a large survey of DP installations in this way.

(3) Action Research in which the researcher sets out to investigate a particular approach to a particular situation. This has the advantage that the researcher has personal access to the realities of the situation, but brings attendant problems of personal bias. This is probably not greater than that of the researcher who consciously or otherwise wants to make a point, or who is simply blind to alternatives. The problem is different in that the personality and competence of the researcher enter into the problem solving activity. In other respects, a single piece of action research is similar to a single case study—a good source of data but insufficient grounds for generalization. The approach would need to be tested in a variety of situations in order to build up knowledge and confidence in the applicability of that knowledge.

(4) Validation of limited hypotheses by surveys of a particular facet, e.g., in Brittan-White's experiment, a particular measure of personality type was used in analyzing the makeup of the project teams, and their success was measured in a particular type of project. This lead to confidence in prediction of the success of certain student groups on their project, and indicated that this might be worth following up with other studies on other project groups, or other personality factors. The problematical issues here are the limits on the hypotheses from the controlled and uncontrolled variables, and the usefulness and reliability of the measurements of those variables. This leads to more or less confidence in the generalizations which can be made from those limited hypotheses.

(5) Statistical surveys of the type "structured programs are more maintainable than unstructured ones." These differ from those of the previous type in that they are not intended to be refutable by specific instances, e.g., an unmaintainable structured program would not affect the usefulness of the research. Results are intended to encourage prudence and good practice, leaving analysis of why to later research.

Proposal for a Paradigm of Action Research For the IS Practitioner

This is an adaptation of Checkland's paradigm to reflect the way in which understanding gained through practice is at the heart of this approach to the generation of new knowledge. It is shown in Figure 1.

At any point in time, the practitioner/researcher has gained an amount of experience, come by some amount of anecdotal evidence, and read some amount of literature both of theories and of cases. Absorption from these sources is a continuous process for any practitioners who are not totally set in their ways.

The practitioner who thinks about his practice is continuously designing and trying a better practice. This may or may not be formulated in some way. A practitioner of any influence or seniority will have his methods copied, consciously or unconsciously, by those who observe his work. Working alongside people you also learn from their activities. This all feeds back into the improvement in the practice.

Where the practitioner consciously moves into research, the design and trial of better practice is formulated in terms of new theories. These theories will involve some or all of the theory, case study, experience or anecdote. They may well be theories about the practice and its improvement and will be fed back into the practice of their author or others, and thus build up the body of experiential or anecdotal data. They may be in the form of surveys or experiments which will feed back into the literature for others to read and work on in other ways.

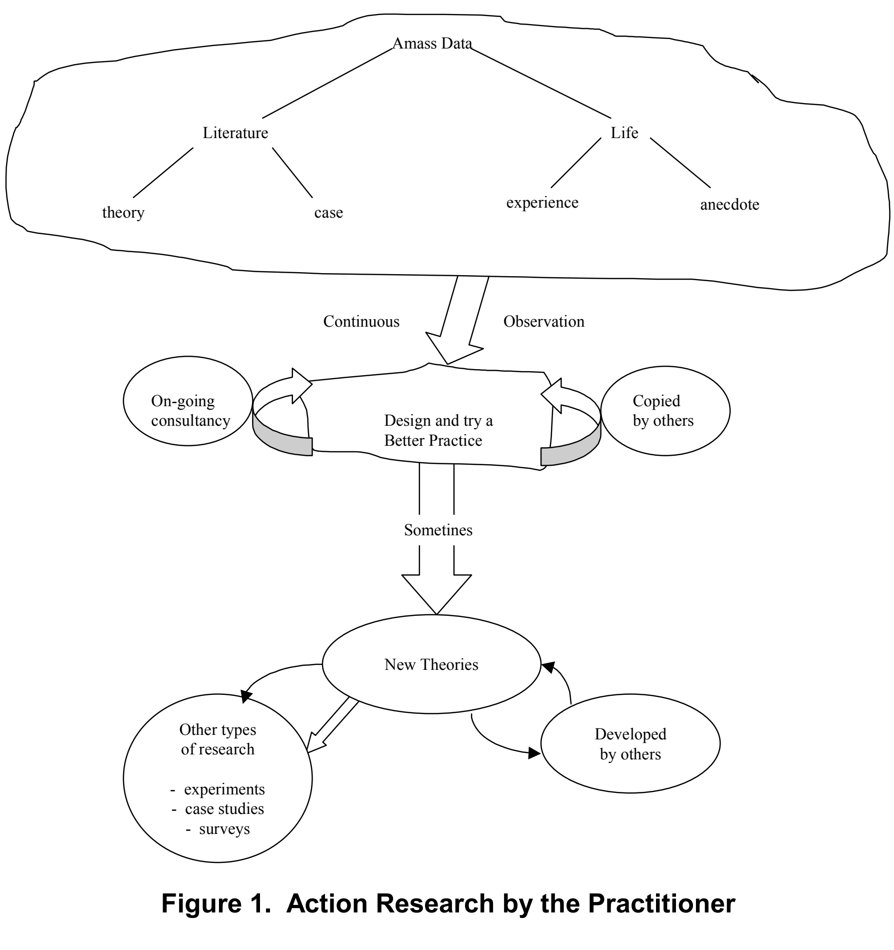

Figure 1. Action Research by the Practitioner

The Author's Current Research Program

This project started as from the statement of a problem—viz that users were too often not getting what they wanted from the installation of a computer system—and from the assumption that a better methodology of systems analysis, design and implementation would help to alleviate that problem. Many commercial companies have tackled this problem as a piece of R&D and perhaps the author's experience in the computer industry would have encouraged this approach as it certainly was responsible for homing in on the topic. However, in keeping with the author's move into academia, and anticipating the current initiative to coordinate industrial and academic research, it was decided to cast this problem into the nature of a Ph.D. project.

It was quickly discovered that there was no obvious way to formulate the project! A project to "devise a better methodology" is too open ended and nebulous, it also poses the question of how do you test whether what you had proposed was a better methodology, or indeed what you meant by better. There was also the difficulty of working out whether it was the methodology that was better, or whether experience was helping to develop a better analyst.

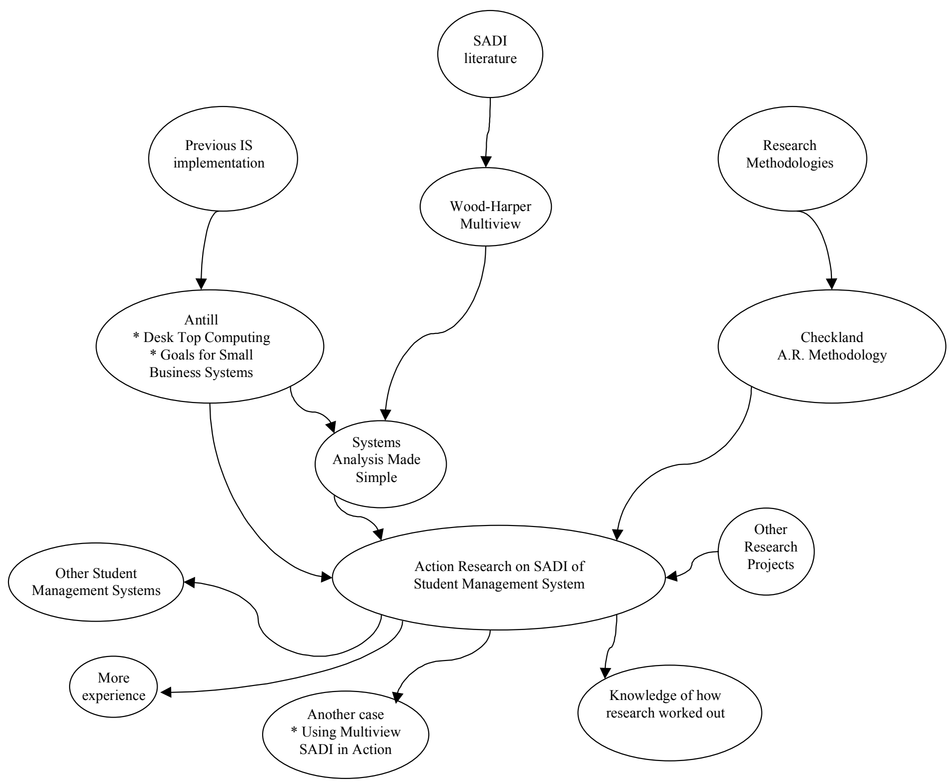

Figure 2. Threads in the Research Project

The research continued along two apparently separate strands: a study of what had been written about methodologies, and continued consultancy with end users and system owners about their requirements for small business systems. When no satisfactory project plan emerged as a natural result of the work, a third strand was undertaken: a study of research methodologies.

Here is a very brief outline of the three strands.

Work on Methodologies

Most of this is covered within CRIS, but the author is indebted to Wood-Harper and Fitzgerald's paper on the Taxonomy of Methodologies which points out the extent to which different methodologies rest on different paradigms and assumptions. Wood-Harper carried this idea on to produce a framework for comparison of methodologies. The framework is used itself as the basis for a working methodology—Multi-view. This covers all aspects of IS design—human activity systems, information modeling, socio-technical, MMI, technical design.

The author took the criteria for evaluating a methodology by Bjørn-Andersen and used these as the basis of a search for the best methodology for the design of small business systems.

Users and Their Requirements

Most of this research has been in the form of previous and on-going consultancy work in the field of small and medium sized business and administrative systems. The author has taken major responsibility in about 50 installations. Practical experience of trying different design methodologies has lead to a variety of criticisms of their usefulness

- In Desk Top Computing (Antill 1980), the author produced the first attempt to adapt NCC techniques to the needs of the microcomputer user. Neither NCC nor structured techniques give any real help in package selection, which is the main concern of the micro user.
- In many of the consultancy exercises, no methodology based on functional assumptions would have been helpful because the problem was in resolving organizational or socio-technical problems.
- The particular needs of the small business system user are detailed in a previous paper by Antill (1983).

Methodological Research

Much of the thinking has been described above. For the project, particular pieces of action research were to be set into the framework of theory, other people's action research and case study, and the author's extensive experience of the problems.

The three came together in an action research project. This is based on a methodology derived from theory and practice, and is supported by other action research and cases using the same methodology. The Information System to be designed is a Student Management System for the newly formed Distance Learning Unit of the South Bank Polytechnic. The project was particularly interesting in that it involved the creation of a new human activity system and the definition of new roles and tasks. It also provided the author with a novel viewpoint from which to take part in the analysis exercise—for the first time she was cast in the role of user! Two different versions of the management system were to be set up. In the first, the author was consultant to the user. In the second, it was the records of the course for which the author was responsible that were being computerized. This actually represents two different user perspectives.

The project is a live one—the Manpower Services Commission have put large sums of money into the development of Open Learning in technical and vocational subjects. This is a significant expansion of higher education with corresponding potential and problems. The PSB team quickly decided that the only way of handling the extra administration generated by large numbers of students at a distance was to set up a dedicated computer system.

The author's previous attempts at using structured SADI methods within the Polytechnic's administrative system had resulted in the conclusion that you cannot use structured methods to solve problems which are often basically political viz "What is the purpose of this Information System?" or "Which of the various stakeholders was to have a say in the decisions about what the system was to do?" These are questions which people are often unwilling to ask let alone answer but the analyst cannot work without knowing what the answers were. An additional difficulty here was that there was no existing or comparable system to analyze. The exercise was as much to create a new human activity system as to create a computerized records system.

Wood-Harper's Multi-view Methodology was chosen as it fitted most closely with the author's experience of the variety of different problems encountered in the process of SADI. So far, the indications are that it has been most beneficial in helping the users, including the author, to formulate their total requirements and to resolve many of the difficulties that arose.

A corollary of the Multi-view method is that the system has been prototyped. We had originally proposed the design as a paper system and then computerized it, but Synergy have created for us a prototype using an applications generator. This has proved a most successful method of trying out the design in practice to see whether it actually met our requirements. It has also proved invaluable for the clerical staff and those management level users with no previous computer experience. The analysis of the human activity system as the first stage of the method has proved attractive to the non-computerate managers who would otherwise have avoided getting involved with a computerized system because they didn't know how to state their requirements.

A great deal of anecdotal evidence is being generated by the exercise. Without a framework of understanding, this evidence might be lost and certainly could not easily be passed on. The framework can be provided by other project experience, case studies, other action research projects, other published research. The result of this coming together of theory and practice will be an improved understanding of the SADI process and of the exercise of SADI research. The project will be written up in a new text on SADI methods in action being written by Antill and Wood-Harper to be published in 1985 by Blackwells.

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