3 ACQUIRING KNOWLEDGE OF INFORMATION SYSTEMS: RESEARCH IN A METHODOLOGICAL QUAGMIRE

Hans-Erik Nissen
University of Lund
Sweden

Abstract

Information systems development and use involves people in action. Still, information systems research is dominated by ideals largely fetched from Newtonian physics. This leaves no room for human innovative action. Its only role becomes a disturbing factor in the general laws aimed at. The problem is, it largely ruins the possibilities of finding important general laws. A far bigger problem would follow from imposing such general laws on the majority of people in order to smooth the development and use of information systems. To avoid these problems new research methods, conducive to the study of human action as such, have to be added to the traditional ones. Here I intend to open a debate on meeting the challenge of truly human centered information systems research.

The Purpose of this Paper

The advocates of information technology take its beneficial effects for granted. They point to the earlier industrial revolution and how it improved the standard of living. To them, research to get the benefits of the electronic revolution is research, which *produces* new information technology and *introduces* it through practical applications.

Creating knowledge in physics, chemistry, etc., for technological development contributes, in a sense, to the emancipation of man. It frees him/her from believing in unexplainable forces of nature and enables him/her to gain control over natural

phenomena. The emancipatory function of science and technology cannot be taken for granted. Whether it becomes emancipatory or not, and if so for whom, depends upon how such knowledge is used in developing our societies (Apel 1976, pp. 145-146).

Information technology and its applications are not based on knowledge from the natural sciences alone. They are also based on human reasoning developed within the fields of logic and mathematics. They further comprise human use of language for arguing in everyday life, including working life, in order to influence human action. Using such a technology inevitably involves taking control over people (Apel 1980, p. 145). Students of such phenomena, however, have to understand the goals and values involved. Such studies will not replace traditional ones. They represent, however, a supplement to these.

Poor application of information technology leads to bad results in enough cases to make these shortcomings a problem. The conclusion from this generally is that more knowledge, and hence more research, is needed. There are different opinions as to the kind of knowledge required. Here I will argue for the need for studies in which we let real people become visible, both individually and as groups.

In these studies, the questions not raised in traditional research should be attacked. These will center on the fact that information technologies are used by real people in organizations which are going concerns. The answers should be of considerable practical importance both at an individual and at an organizational level. By elucidating problems at these levels, light will also be shed on the aggregated effects at societal level.

Abandoning traditional questions, formulated to suit traditional methods of research, will cause doubts as to the scientific quality of the research. Still, the ability to investigate relevant questions is to me more important than relying exclusively on well established methods. I am not alone in making this choice. The task of directing research towards new questions, calling for major methodological developments, is a big one. It demands the collaborative effort of many researchers. The purpose of this paper is to indicate some possibilities and problems in doing this. It hopefully will get me into contact with fellow researchers, both those sharing my dissatisfaction and those prepared to advocate traditional studies as the only scientific ones. Its aim is to start a debate on the goals and methods of information systems research.

Traditional Methods of Research

What do I recognize as traditional methods of empirical research? Let me quote from an article on paradigms in computer science:

> ...although the phenomena of computer science are created by man they can be studied using empirical techniques of the natural sciences.... (Wegner 1976).

Wegner recognizes the phenomena of computer science as man-made. But he settles for methods taken from the natural sciences, only supplemented by methods from mathematics. In studying development and use of information systems as human action, it is inappropriate only to imitate the ideals of Newtonian physics. Such research may end up by recommending most people to be handled like billiard balls.

What are the main characteristics of a scientific method based on the natural sciences and viewing classical physics as the science par excellence? Let me summarize what I have found in common text on research methods:

- Trying to formulate general laws, starting from hypotheses formulated by researchers specialized in the area of study.
- Trying to corroborate or falsify hypotheses by testing them in repeatable experiments devised by the researchers.

The currently dominating outlook on science and scientific method in this area can be illuminated by some quotations from Bunge (1967):

> The scientific method and the goal to which it is applied (objective knowledge of the world) make all the difference between science and non-science....A science is a discipline using the scientific method for the purpose of finding general patterns (laws).
>
> Those disciplines which have no occasion to use the scientific method—e.g., because they limit themselves to data gathering—are not sciences, although they may supply science with raw material; this is the case of geography. Nor are sciences those doctrines and practices which, like psychoanalysis, refuse to employ scientific method....
>
> Scientific techniques may be classed into *conceptual* and *empirical*. Among the former we may mention the tactics for the precise stating of problems and conjectures of a certain kind, and the procedures (algorithms) for deducing consequences from the hypotheses....(Mathematics provides, of course, the richest set of powerful tactics for stating problems and hypotheses in an accurate way, for deducing consequences from assumptions, and for checking solutions. But it is of little help in finding problems or in conceiving the nuclei of new hypotheses in the factual sciences. Besides, in the more backward sciences our ideas are not yet clear enough to be susceptible to mathematical translation. Otherwise there is no limitation in principle to the application of mathematical concepts, theories and techniques in factual science...) (Bunge 1967, pp. 12-15).

Suitable mathematical concepts for many factual sciences, however, still seem to await their development/discovery. This constitutes a definite limitation to the application of mathematical concepts and formulae in social sciences.

Delimiting the Field and Purpose of Information System Studies

Information systems have two essential features: (1) They include as a rule information technology. (2) Those associated with them are capable, acting as self-steering systems (Aulin 1982). Examples of such systems are man and human communities. Some notable properties of such systems are:

> In addition to self will and non-manipulability, a self-steering system as a system that seeks and follows its own goals has some other notable properties:
>
> 1. creative goal-formation,
> 2. multiple goals and many-level goals, and even
> 3. contradictory goals (Aulin 1982, p. 63).

Using the two features mentioned in order to delimit the research area deserves a few comments. First, studying the development of information systems, it is possible to end up without any technology. Studies of development where the use of information technology is ruled out at the beginning are excluded. So are studies of information systems in use without any computer support. Second, assuming that people are capable of acting as self-steering systems does not mean they will always take advantage of that capability. The extent to which they do this has to be determined by empirical studies.

I have not excluded the case of someone in the future designing artificial self-steering systems. Should someone create such an artifact, its status in society will become a social problem and not a technical one. Introducing artifacts generally creates social problems, i.e., problems of how we relate to them and how this influences the ways we relate to other people and to ourselves.

One purpose of research is to acquire systematic knowledge. But an important question is: In whose interest is this knowledge acquired and systematized? The question emanates from Habermas (1966). It could also be stated as: Who is to use this knowledge? I believe Habermas has an important point. To ignore it would mean not helping people with a weak position in the development and use of information systems to acquire knowledge strengthening it. By a weak position, I mean a poor chance of protecting their own best interests.

Studies of information systems should be undertaken in the interest of all the parties involved. This may sometimes demand several studies of the same information system from different perspectives. When discussing methodological problems, I propose at least four main groups to be distinguished.

1. Those with vested interests in the production and distribution of information technology, including transformation services.
2. Those with vested interests in using information technology with the intention of improving the work and action of others, e.g., managers.
3. Those actually using information technology to support their own work or everyday life.
4. People indirectly affected by the use of information technology.

The first group are the producers of information technology. The two next are consumers with rather different interests. Until now, the first of these two has been seen by the producers as their customers. The last group represents a third party.

One group of people interested in the results of research is missing, i.e., other researchers. Yet the results of research are usually presented in a way making them accessible only to other researchers. The dialogue between researchers is a part of the

process of validating results and generating ideas for further research. The groups I distinguish above can be called the end users of research.

In this paper, I will focus the discussion of research on what can be called micro theories, i.e., theories for development, choice and use of information systems in particular applications. This requires a research task of establishing a taxonomy. Such a taxonomy has to be *independent of the technical support employed*. It will have to be based on the intended purpose of the application and on its human and organizational contexts. Categories could include information systems to control the work of others, to improve the work of others, and to improve one's own work.

Finally, a comment on terminology. What I have called an "information system" generally comprises both the use of information technology *and* the use of information channels free of such technology, e.g., face-to-face communication. In, for example, data base contexts, only the computerized parts tend to be called "information systems." Employing this usage, I would say: The field of study has to cover the information system, the people informing and informed by it, as well as the other channels and sources they use to keep themselves and each other informed.

The Unity of Scientific Endeavor—On What Basis?

Important knowledge for the consumers of information services cannot be acquired adhering to the methods of traditional science, e.g., knowledge of new kinds of actions, when user interests are in conflict with those of the producers or of other consumers. This is equally valid for groups of people indirectly affected by information systems. The reason is simple. This kind of knowledge neither belongs to the "general law" category nor can it be tested by repeatable experiments devised by "outside observers."

It is my belief that even some knowledge of interest to producers cannot be acquired by methods of traditional science. For the natural sciences and the empirical-analytical social sciences, the traditional methods are appropriate. Different techniques are used in different sciences. Natural science techniques are also still being developed (Bunge 1967). Sometimes these technical differences are presented as a multitude of research methods. They do, however, not address the fundamental difference of treating human action as such or only as a stimulus-response reaction.

There have been and are many advocates of the unity of scientific method. This supports the belief that there is something essential that they all are aiming at. One way to express this aim is to say they are looking for an underlying unity in all scientific endeavors. Popper (1963) has posed this problem as a search for a line of demarcation between science and non-science. I will not discuss his theory of falsifiability as a criterion of such a line of demarcation. I focus on his aim to find what unites all scientific knowledge. My thesis is, if a unity of scientific endeavor is to be established, this has to be on a different basis from those tried so far.

Researchers following traditional methods of science look for causal laws. But Ryan (1970) stresses the rule-following behavior of people as characteristic of the fields of social science:

> The claim which so to speak permeates Winch's argument is that social behavior is to be understood as *rule-following* behavior, and not as *causally regular* behavior....only if we can describe all events, especially those events which are human actions in social situations, in such a way that they are not logically or conceptually linked to any other events, could we begin to establish mechanical causal laws of human behavior. A causal psychology...requires us to be able to identify the components of action in such a way that they acquire the necessary logical independence, and thus permit us to establish causal connections between them. And essentially, what Winch argues is that we cannot do this without the total loss of the very subject-matter we set out to investigate in the first place. What Winch claims is that the connections which hold between the actions are *conceptual* connections, and that the terminology which we employ in talking about actions is indispensable to our identifying actions as actions—rather than mere bodily happenings, physiological events....Human actions, on this view, are meaningful, and meaning is not a category open to causal analysis; thus, so long as meaningful actions form the subject-matter of social inquiry, the most important category for our understanding of social life will not be that of cause and effect, but that of meaningfulness and rule-guidedness.... (Ryan 1970, pp. 128-131).

Rules can be broken, albeit often at a price for the rule breaker, in a way established natural laws cannot. The price depends upon whether breaking a rule is undertaken individually or collectively. Breaking a rule may also constitute a step in trying to change it. The rule breaker may, for example, perceive the rule as unjust or inappropriate. Whether a rule will be broken in a particular case depends upon the meaning assigned to such an action by the people involved, not on any invariant causal law. The quotation from Ryan provides one argument why studies in social sciences have to differ in a fundamental way from studies in natural sciences.

The most thorough analysis of the fundamental difference between research covering the study of people and other research is to be found in the writings of the German philosopher Karl-Otto Apel. What, according to him, makes the position of traditional unified science untenable is:

> ...the presupposition—hardly reflected upon by the logic of science—that the tidy *separation of the subject and object of science* is not only to be justifiably upheld in the realm of natural science but also in the social sciences too (Apel 1980, p. 141).

As soon as one consciously crosses this rubicon, a number of fundamental implications follow:

1 First of all, there already exists a fundamental distinction in the *identification* of the objects of science at the level of so called *description*. This distinction is based on the question as to

whether the so called "data" can be made "available" through repeatable experiments as instances of possible explanations by means of laws—or can at least be subsumed under class concepts—or whether they should be treated as spatio-temporal individualized elements of the totality of an irreversible historical process that is itself mediated through these elements. We can derive *two completely different concepts of experience* from this. Only the former opens up the transcendental horizon for such entities as contingent "laws" or inductive corroboration in the sense of the "logic of science." In contrast, the latter opens up the transcendental horizon for an experience...which includes not only inductive corroboration or falsification but, above all, the qualitative revision of its conceptual presuppositions through self-reflection (Apel 1980, pp. 141-142).

2 Even "more doubtful" than this implication of our step over the rubicon of the modern logic of science for the theory of experience is the associated *transcendence of the concept of value-free science* that Max Weber also made obligatory for the social sciences. The recognition of what Weber termed "purposive-rational understanding" as a "good reason essay" which, as such, cannot be reduced to a causal motivational explanation already necessitates a critical evaluation of human behavior, even if the latter remains confined to the normative *standard of instrumental rationality* and seeks to understand the goals in question without evaluating them (Apel 1980, p. 142).

3 The necessity for evaluation in the critical social sciences outlined above indicates the final and most radical consequence that is implicit in the theory of science's recognition of society as the subject and object of science: namely, the *distinction between theory and practice*....a methodologically pure theory with a practice in the sense of technical utilization is valid *a fortiori* with reference to the social-technological function of the so-called empirical-analytical social sciences. That this is the case, is manifested in the mostly quite naively formulated demand that the perfection of scientific progress in modem industrial society must lie in the extension of man's natural scientific control over nature through the social-scientific *control* of man *over man*. But this demand is not merely and obviously of practical relevance, but as a practical relevant demand it is deeply ambiguous. If, in terms of empirical-analytical unified science, the separation of the subject and object of science is also to be maintained in the social sciences, then this demand could only signify that society must be split up into those who are controlled and those who control. This practical consequence of this ambiguous demand today finds its

> methodological recognition and explication, it would appear, in a functionalist systems theory of society which thereby places itself at the service of "technocracy".... (Apel 1980, p. 145).

Apel replaces the presupposition of "the tidy separation of the subject and object of science" by an *a priori* presupposition for all science of "an unlimited communication community to be realized in a concrete society." This, by the way, "... must be presupposed in order for any argument to have meaning ...." The crucial point of this presupposition lies in the fact that the ideal communication community is to be realized in concrete society. As Apel points out himself:

> Thus, this transcendental presupposition of science is neither idealistic in the sense of traditional philosophies of consciousness nor materialistic in the sense of an ontological official "dialectic materialism" or a scientistic, positivistic objectivism that conceals its ontological implications. Rather, it will be concerned with a genuine *dialectical conception that lies on this side of idealism and materialism*....dialectical ...in so far as it "mediates" from the very start the opposition between transcendental idealism and a "historical materialism" that is related to society (Apel 1980, p. 140).

The position of Apel excludes neither natural science nor social science and technologies based upon these. It clarifies that (1) all people have to become the recipients of scientific results, and (2) supplementing critical social sciences is needed. The latter support all people, of any historical society, to acquire knowledge. This knowledge is to make them well-informed as actors in forming the tomorrow of their societies.

If there is a unity of scientific endeavors to be regained after leaving the traditional interpretation of unified science, it is my conjecture, it has to include in its base Apel's a priori or some similar presupposition.

Some Consequences for Acquiring Knowledge of Information Systems

What has been achieved by the discussion so far? The concept of the scientific endeavor and its goal has been broadened. Traditionally this goal was expressed as producing objective knowledge of the world. Here "objective" was understood as "intersubjectively, and provisionally agreed upon by a closed community of scientists." New members were admitted into this community only by its old members. My conclusion is this means a scientifically knowable world, which excludes most human actors.

The new goal for scientific endeavors advocated here can be stated thus: To acquire intersubjectively agreed knowledge of the world *and* deepened self-understanding of all men—individually and collectively—as actors in concrete, historical societies. What represents knowledge and deeper self-understanding has to be agreed in an open, unoppressed, global communication community to be realized in historical time. The

scientifically knowable world now includes human actors as actors. The scientific endeavor by this change of its goal now also comprises the acquisition of knowledge conducive to improved mutual and self-understanding of human actors.

What implications follow from this shift for information systems research? Shifts in the objects of study and in the kinds of knowledge acquired can be expected. The former will result in more studies of information systems design as a process closely integrated with organizational redesign. Field studies and experiments will increase. Studies of information systems in actual, organizational use will supplement studies of systems development. Such studies are needed to know how the different actors involved understand and actually use information systems.

The latter will shift the kind of knowledge acquired to comprise:

- knowledge on which to base modifications of information technology in order to make it and its use more transparent to all the people affected by it,
- knowledge on which to base new architectures of information systems, which integrate formal and informal channels of information,
- knowledge to support people affected to evaluate information technology from their own professional viewpoint,
- knowledge to enable everyone affected to gain real influence over how information technology is put to use in our society.

The scientific endeavor belongs to those human activities in which people develop techniques in order to inform themselves. Any fundamental shift in our way of understanding the scientific endeavor induces potential new ways of looking at information systems, their development and use. This has already been discussed to some extent, e.g., by Churchman (1970) and by Ivanov (1972). Table 1, summarizing two ways of doing research, can also be studied as two ways of looking at and using information systems. This indicates a connection between our primary field of study and investigations into our methods of research.

What could be gained by a shift in information systems research? It represents the only way of developing and using information technology without creating a technocratic society. Such a society furthers the control of the majority of people by a minority. A changed direction in information systems research is not enough to avoid a technocratic society. Still it represents that section of the front line on which we as researchers can take part in the ongoing battle for the transformation of our societies.

This battle is about the goals and values of society. The use of technology is not at stake. What is at stake is the kind of technology we develop and the way in which we put it to use.

To me, the arguments so far are good reasons for a shift in research in general and for a shift in information systems research in particular. Shifting the goal of research in a fundamental way means that the traditional methods of research can no longer be taken for granted. In the realm of natural sciences and of the formal sciences, they remain unassailed. In the social sciences and in all kinds of applied sciences, their use has to be critically evaluated in relation to the new goal. Many of them may still be used though in modified ways and they will have to be supplemented by methods fetched from hermeneutics and the *Geisteswissenschaften* (Apel 1980, pp. 46-76).

Table 1. Two Different Ways of Looking Upon Research and Upon Development and Use of Information Systems

Description (objective/i.e., inter-subjectively agreed)	**Description** (subjective/objective/historical)
Explanation by specialists (by means of general laws on which to base technologies and of observable starting conditions)	**Understanding** by actors (of actors in an environment and of the environment)
Prediction given by specialists (based on general laws and observation of starting conditions)	Support well grounded **expectations** formed by actors based o knowledge acquired and on values they are prepared to work for
-o-	-o-
Traditional research	Research aware of critical social sciences
Information systems controlling actors (as if they were mechanisms)	Information systems supporting actors

Some of the methodological problems created by this reevaluation of traditional methods and introduction of new ones will be discussed in the next three sections. They will be treated as problems of theory building and of empirical studies.

Problems with Building and Applying Theories

Suppose social science research does not try to create knowledge systems in the form of general laws about man and human action. One apparent risk is such research does only result in a sequence of loosely related case stories. Are there any kinds of theoretical constructs which can be built short of general, causal explanations?

Case stories are traditionally looked upon only as heuristic devices which suggest ideas for hypotheses to be tested by repeatable experiments. Those which are not falsified may be used as building blocks for general theories. The conceptualizations employed in these theories are those of the researchers looking upon themselves as outside observers. At best the researchers see themselves as minimally intervening observers on the border of their field of study. As pointed out in the quotations from Ryan, human actions cannot even sensibly be described without severe distortion from such a research perspective.

In the research proposed here, among the primary research data are the concepts employed by the actors in the field of study. These concepts will cover both their work and life situations, how they make sense of these and of their actions as well as of the actions of others. These conceptualizations will, in a sense, be subjective. In another sense they will be intersubjectively shared. This is largely due to the use of language for

interaction in social reality (Berger and Luckman 1967). Compared with total subjectivity, interpreted as one conceptualization per person involved, there will be restraints found, i.e., the subjective material will exhibit some form of pattern. Still, there will often be several conceptualizations of what to an outside observer would look as one "objective" phenomenon. How an actor sees his action, how other people see it, and what constitutes the actor's intersubjectively agreed overt behavior will often differ systematically. Argyris (1982) has found differences between what he calls "espoused theories of action" and "theories in use." The former correspond to how the actors explain their actions. The latter correspond to how other people explain these same actions.

Let this suffice to indicate some ways of processing research data gathered during hermeneutic phases of research, i.e., during phases of research attempting to understand the subjective perspective of the actors studied. In the reconstruction of "conceptions in use," the researchers themselves can take part. This processing of primary research data and its results can and has to be checked and criticized by other researchers and by the actors in the field.

The further processing of this kind of research data can follow two lines, which are not mutually exclusive. In the research perspective discussed here, the research data are looked upon as individualized elements from of an irreversible historical process, which is itself mediated through these elements. This process, though irreversible in principle, may during some limited period of time exhibit a stationary pattern. According to traditional ways of theorizing, a hypothesis would have been formed interpreting the pattern as an indication of some invariance on which a general law could be based. This would correspond to what Galtung (1977) has called *science as invariance seeking*.

When people interact in the development and/or use of an information system, any pattern found could rather indicate a set of rules followed fairly consistently for the time being. If the pattern is disclosed to all actors involved, as a set of rules they seemingly follow, this may result in one or more groups of actors changing their actions. This will generally be accompanied by their reframing the situation, i.e., conceiving it in a different way than before. This would correspond to what Galtung has called *science as invariance breaking*. The outcome of such an intervention, particularly how it is interpreted by different groups of actors, often will provide more data on the process studied. In this way, systematized knowledge could be acquired indicating differences between patterns which do break using mode-rate resources to change them and those which do not.

Another way of processing the analyzed data is to look for patterns of a higher order in a number of cases including results of interventions. The reason is, as Apel (1976) has pointed out, that the hermeneutic method is not sufficient alone, as people are transparent neither to themselves nor to each other. When we, in our dialogue with actors in our field of study, fail to understand them, we may temporarily suspend it. Provisionally we look upon them and their interaction as the behavior of a quasi-natural system to be explained. Theories for different such systems constitute a second theoretical product from the kind of research discussed here. These have to be expressed in a conceptual framework of the researcher. Before they are applied, i.e., introduced into the dialogue with different groups of actors, they have to be translated into their language. The researcher, through the hermeneutic phases of his studies, should have acquired a fair knowledge of these languages.

These theories are called "quasi-natural" as they provisionally present cultural phenomena as if they were natural phenomena. Their expressed purpose is:

> Here, the social-scientific "explanations" would have to be so established (and published!) that they did not give power over the ignorant to those who know, but represent a challenge to all to transform causally explicable modes of behavior into intelligible actions by a process of taking stock.... (Apel 1980, p. 72).

In other words, the goal of acquiring and distributing this kind of knowledge is not to create technologies but to deepen the self-understanding of the actors involved.

Tentative Characteristics of Theories Making Sense of Human Action

General, causal laws make good sense of what we know, at some point of time, within the fields of natural sciences. They do, however, not make good sense of human action as such. In order to do so we have to learn from the science of history and use good reason essays (Dray 1967). A good reason essay shows the point of an action, why a person could have acted as he did. Subjective meanings of the actors involved will generally form part of a good reason essay.

If we are to acquire and disseminate knowledge contributing towards improving the self-understanding of various groups of people affected by the use and development of information systems, theories making sense of human actions are needed.

The subjective reasons for the actions performed by actors could be stated as their in-order-to motives in the sense of Schutz (1967). The subjective reasons for their beliefs, i.e., why their chosen way of action will be efficient in realizing their intentions, could be stated as their because-of-motives (Schutz 1967). In this way the subjective beliefs of the actors could be introduced into the theories.

A few things are worth noting about the theories proposed. In order to reconstruct subjective meaning and intentions, they have to be framed in an intentional language. All traditional theories are framed in an extensional language of an invisible, outside observer. The subjective meanings and intentions are those of the actors in the field of study. In the proposed research perspective, the unavoidable intervention of the researcher into the field of study has to be made explicit. It might be worthwhile to observe the fact that the application of any theory always comprises an intervention of some kind. The actors in these theories will be what Max Weber has called *ideal types*. This concept has been elucidated by Schutz.

The value of the concept of ideal types can be illustrated by information system use in hospitals. In a traditional study, the people affected likely would have been grouped into such classes as doctors, nurses, patients, etc. The heterogeneity of the groups would have been taken as an unavoidable "random error" to be counteracted by the sizes of the groups studied and by repeating the studies. When it comes to variations with respect to human action, my conjecture is, these only rarely are random. Most variations of human action could be accounted for by understanding the subjective meanings attached to situations and the intentions of various actors. As ideal types are ascribed subjective meanings and intentions, the groups of doctors, nurses, and patients can each be subdivided according to differences in this respect.

As reconstructions of the joint experience of actors (and researchers) in the field of study, based on the primary recorded research data, these theories can be criticized by other researchers. Subjective meanings and intentions are expressed *in* these theories. Their adequacy *as* theoretical reconstructions of human experience can, however, be intersubjectively scrutinized.

Even these theories will probably employ some concepts not currently used by the actors in the field of study. However, the essence of the knowledge, condensed in these theories, has to be translated into the everyday language of the actors. This follows from the presupposition to include these among the knowers (subjects) of this knowledge. Care should be taken to facilitate this task, whenever theoretical terms for this kind of theory are chosen. Including subjective meanings and intentions, albeit of ideal types, should also ease the task of making this kind of systematized knowledge accessible to everybody concerned.

Problems in the Realm of Empirical Studies

A fundamental problem introduced by the use of hermeneutic methods of understanding is how to check one has not misunderstood. As Apel (1980, pp. 46-76) has pointed out, it is not sufficient for critical social sciences to use only hermeneutic methods. To do so would presuppose we are, as intentional actors, transparent to ourselves and each other. This is obviously not true. When we meet indications of lack of mutual understanding or in self-understanding, we have two main options.

One is to shift our topic of conversation to a meta-level. There we start explicitly to discuss differences in how we conceive the primary topic, and what we implicitly presume about our mutual relations. We always undertake such a discussion taking a set of rules for granted, but at the same time checking them implicitly in the primary communication. Evidence for some meta-linguistic and meta-communicative processes going on as integral parts of all human communication is given by Bateson (1972, pp. 177-193). Some explicit discussion on these levels may suffice to remove the lack of understanding perceived. If it does not, the second option has to be tried.

The second option (as has been indicated above) is discussing what kind of theories to build. It comprises building or applying earlier quasi-natural explaining theories of human behavior (Apel 1976, pp. 58-68). These are then used in order to restore the understanding in the provisionally suspended conversation. Before such theories can be applied, the conclusions drawn from them may have to be metaphorically transplanted into the history of the organization and/or the biographies of the actors involved. This option may also be used by the researchers as a tool of intervention intended to improve the self-understanding of groups of people.

The necessity of quasi-natural turns of investigations within any critical social science should help to remove a rather common misunderstanding. Some researchers seem to take the position any use of methods or techniques developed within natural science or within objectively explaining social sciences *ipso facto* disqualifies an investigation as a contribution to a critical social science. As I see it, such a conclusion does not follow from the fundamental difference as explicated by Apel. Besides it would create a tremendous burden of methodological development. What has to be considered

is, if methods "imported" are used in order to improve self-understanding for *all* groups of people affected by information systems or not.

In the quasi-natural turns of research on information systems, classifications and various kinds of measurements will be used. Traditionally, which classifications to apply and which measurements to use is argued within a closed community of researchers. In a critical social science what classifications and measurements make sense to the actors in the field of study has to be discussed with them. This follows from recognizing *all* people as the subjects of social science knowledge. If this recognition is not practiced in the research process, the results will not belong to the realm of a critical social science.

In this context, a possible misinterpretation has to be explicitly discussed. The importance of what makes sense to the people in the field of study does not preclude the use of some concepts, classifications and measurements which make sense to the researchers, but, at the time of the investigation, do not make sense to the other actors. In the case of measurements, and the use which is to be made of them, still their acceptability to the people affected has to be checked.

How to Continue the History of Information Systems Research?

During the short history of information systems research, most studies have centered around producing knowledge on which to base methods of design and implementation of such systems. The implicitly intended knowers of such knowledge were mainly persons specializing in information systems analysis and design. The values they supported in their work were predominantly those of their employers. The research methods applied were fetched from natural science and from objectively explaining social sciences.

Some studies of implementation and of evaluation of systems were intended to acquire knowledge for consumers of information technology, particularly managers, and not for producers and their advocates (e.g., Lucas 1976). Some critical studies employing other methods of social science studies than the traditional ones by now have also been undertaken. Still they are very few by comparison and are often looked upon as either unscientific or not belonging to the field of information systems studies.

Within other fields studying man and his societies, a debate has been going on for quite some time on the goals and methods of scientific endeavors. The time seems almost overdue for researchers in the field of information systems to join it. We need to discuss its implications for studies in our field of study. There may be very good reasons for the history of our research so far. This does not free us from the responsibility of reflecting over and debating where we are to go from here. The debate hopefully will improve our self-understanding as researchers in different sub-areas of information systems research. It probably will set us off on a number of routes with a fair mutual understanding of our different choices. Finally, it hopefully will improve our understanding of for whom we are acquiring knowledge in the first place and why.

Acknowledgments

The author wishes to thank E. Mumford, Manchester Business School, R. Hirschheim, London School of Economics and Political Science, A. Olerup and P. Flensburg, University of Lund, for their valuable comments on an earlier draft of this paper. The author also has profited from a number of comments received during the IFIP WG 8.2 Colloquium at Manchester Business School. For any shortcomings of the paper in its current form, of course, the author alone is responsible.

References

Apel, K.-O. *Transformation der Philosophie, Band 2, Das Apriori der Kommunikationsgemeinschaft*, Frankfurt: Suhrkamp, 1976.

Apel, K.-O. *Towards a Transformation of Philosophy*, London: Routledge & Kegan Paul, 1980.

Argyris, C. *Reasoning, Learning, and Action, Individual and Organizational*, San Francisco: Jossey-Bass, 1982.

Aulin, A. *The Cybernetic Laws of Social Progress: Towards a Critical Social Philosophy and a Criticism of Marxism*, Oxford: Pergamon Press, 1982.

Bateson, G. *Steps to an Ecology of Mind: A Revolutionary Approach to Man's Understanding of Himself*, New York: Ballantine Books, 1972.

Berger,P. L.,and Luckmann, T. *The Social Construction of Reality: A Treatise in the Sociology of Knowledge*, Garden City, NY: Anchor Books, Doubleday, 1967.

Bunge, M. *Scientific Research I: The Search for System*, Berlin: Springer Verlag, 1967.

Churchman, C. W. *The Design of Inquiring Systems, Basic Concepts of Systems and Organization*, New York: Basic Books, Inc., 1971.

Dray, W. *Laws and Explanation in History*, Oxford: Oxford University Press, 1957.

Galtung, J. *Methodology and Ideology, Essays in Methodology, Volume One*, Copenhagen: Christian Ejlers, 1977.

Habermas, J. "Knowledge and Interest," *Inquiry* (9), 1966, pp. 285-300. First published as "Erkenntnis und Interesse," *Merkur* (19), 1965, pp. 1139-1153.

Ivanov, K. *Quality-Control of Information: On the Concept of Accuracy of Information in Data-Banks and in Management Information Systems*, Ph. D. thesis, Royal Institute of Technology, Stockholm, Sweden, December 1972. Available as order code PB-219 297 from the National Technical Information Service (NTIS), Springfield, VA 22151 U.S.A.

Lucas Jr., H. C. *Why Information Systems Fail*, New York: Columbia University Press, 1975.

Popper, K. *Conjectures and Refutations, The Growth of Scientific Knowledge*, London: Routledge and Kegan Paul, 1963.

Ryan, A. *The Philosophy of the Social Sciences*, London: Macmillan Press Ltd, 1970.

Schutz, A. *The Phenomenology of the Social World*, Evanston, IL: Northwestern University Press, 1967.

Wegner, P. "Research Paradigms in Computer Science," Second International Conference on Software Engineering, San Francisco, 14-15 October 1976.