



Doing interpretive research

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Abstract

Interpretive research in information systems (IS) is now a well-established part of the field. However, there is a need for more material on how to carry out such work from inception to publication. I published a paper a decade ago (Walsham, 1995) which addressed the nature of interpretive IS case studies and methods for doing such research. The current paper extends this earlier contribution, with a widened scope of all interpretive research in IS, and through further material on carrying out fieldwork, using theory and analysing data. In addition, new topics are discussed on constructing and justifying a research contribution, and on ethical issues and tensions in the conduct of interpretive work. The primary target audience for the paper is less-experienced IS researchers, but I hope that the paper will also stimulate reflection for the more-experienced IS researcher and be of relevance to interpretive researchers in other social science fields.

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Introduction

In my book on interpretive research in information systems (IS) published over a decade ago (Walsham, 1993), I argued that interpretive methods of research start from the position that our knowledge of reality, including the domain of human action, is a social construction by human actors. Our theories concerning reality are ways of making sense of the world, and shared meanings are a form of intersubjectivity rather than objectivity. I have similar epistemological views now, although I am happy to accept the plausibility of the ontological position of the critical realist (Mingers, 2004) that there is an objective reality. Indeed, I see critical realism as one possible philosophical position underpinning interpretive research, along with others such as phenomenology and hermeneutics. With respect to the theme of this paper, namely the conduct of interpretive research, Geertz (1973) summarized an interpretive view of the data we collect in such studies in the following memorable sentence:

What we call our data are really our own constructions of other people's constructions of what they and their compatriots are up to (p. 9).

Interpretive research has clearly become much more important in the IS field than it was in the early 1990s when Orlikowski and Baroudi (1991) noted that it represented a very small proportion of the published IS literature in mainstream US-based publications. Mingers (2003) calculated that 17% of papers in six well-known US and European-based journals in the period 1993–2000 were interpretive. This ranged from only 8% of papers in *Information Systems Research* to 18% in *MIS Quarterly* to 35% in *Accounting, Management and Information Technologies* (now renamed *Information and Organization*). With respect to the *European Journal of*

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Information Systems (EJIS), Mingers calculated the figure as 12% in the period 1993–2000, and EJIS has continued to publish a significant number of interpretive papers since then (e.g. McGrath, 2002; Sarker and Sahay, 2004; Lin and Silva, 2005), including a whole special issue in 2004 (Bhattacharjee and Paul, 2004).

So, many IS journals are now publishing interpretive studies, and interpretive researchers can find several examples of such papers in any of the potential outlets for their work. However, do such papers give enough information on how interpretive research should be carried out and written up? The answer, in my view, is no. Authors of interpretive studies, and I am one of them, devote much of their allotted space to the substantive contents of their research topic, and only a little to their own research conduct and method. This is understandable, and indeed desirable, but it does leave a gap, particularly for less-experienced researchers. For example, I find that Ph.D. students everywhere want more on the subject of 'how to do it'. It is true that substantial books on methodology in social science research have been published for many years (for example, Layder, 1993 or Bryman, 2001). I would argue that such books are valuable to IS researchers but they do not provide specific material and examples directly concerned with interpretive research on IS.

In response to this perceived need for practical advice for interpretive IS researchers, I wrote a paper (Walsham, 1995) on the nature of interpretive case studies in IS research and methods for carrying it out. It was a relatively short and straightforward piece, but it has been widely cited in the literature (87 citations by July 2005, according to the Web of Science). In addition, the average number of citations of the paper per year shows an upward trend to date, supporting my argument that there is a continuing demand for specific, practical advice on the form and conduct of interpretive IS research. However, I wrote the 1995 article paper 10 years ago, and I feel that I, and the IS field, have moved on since then. The purpose of the current paper is to try to synthesize some of the further insights I have gained on carrying out interpretive research over the last decade, learning from both my own work and that of others.

The paper aims to be relatively short and direct like its predecessor. However, the list of topics is not the same, as I wish to emphasize some things which were not in the previous paper at all, and wish to take a new look at some old topics. The sections below address the following: carrying out fieldwork; theory and data analysis; constructing and justifying a contribution; and ethical issues and tensions. My previous paper dealt solely with interpretive case studies, whereas the current paper is aimed at interpretive research more generally, including ethnographies (e.g. Schultze, 2000), and action research with an interpretive style (e.g. Braa *et al.*, 2004). I believe that interpretive research can also be critical (see Walsham, 2005 for a fuller exposition of my position

on this), so the current paper is also relevant to critical IS research based on an interpretive approach.

Carrying out fieldwork

The setting up and carrying out of fieldwork is the fundamental basis of any interpretive study. I will tackle the following four elements in this section: choosing a style of involvement; gaining and maintaining access; collecting field data; and working in different countries. Although I describe this paper as being of the 'how to do it' variety, the material in this section and the rest of the paper does not provide neat prescriptions or recipes for success. All fieldwork is context-dependent and requires difficult choices to be made. My aim is to provide some criteria for choice and some evaluation of these criteria from my own perspective. However, the individual researcher or research team must make their own choices in the light of their own context, preferences, opportunities and constraints.

Choosing a style of involvement

I made the distinction in Walsham (1995) between an 'outside researcher' and an 'involved researcher'. I saw the former as, for example, a researcher carrying out a study mainly through formal interviews, with no direct involvement in action in the field or in providing significant feedback to field participants. I saw the latter as a participant observer or action researcher. Although the above distinction still seems useful to me, I would view involvement now as more of a spectrum, and as changing often over time. At one end of the spectrum is the 'neutral' observer, but this does not mean unbiased. We are all biased by our own background, knowledge and prejudices to see things in certain ways and not others. I mean by neutral that the people in the field situation do not perceive the researcher as being aligned with a particular individual or group within the organization, or being concerned with making money as consultants are for example, or having strong prior views of specific people, systems or processes based on previous work in the organization. At the other end of the involvement spectrum is the full action researcher, trying consciously and explicitly to change things in the way that they feel best. A recent special issue of *MIS Quarterly* (Baskerville and Myers, 2004) provides details of six IS action research studies.

What are the advantages of close involvement? It is good for in-depth access to people, issues, and data. It enables observation or participation in action, rather than merely accessing opinions as is the case in an interview-only study. Some positive benefits can often be gained because the field participants see the researcher as trying to make a valid contribution to the field site itself, rather than taking the data away and writing it up solely for the literature. For example, action research represents a highly involved type of research study and, as noted by Baskerville and Myers (2004), can be considered a direct

response in the IS field to the frequent calls for IS researchers to make their work more relevant to practice.

However, there are some potential disadvantages of close involvement. An ethnographic or action research study is very time-consuming, and opportunity costs are incurred in this. For example, the time saved by a less-involved study could be used instead to carry out a second full case study. A second disadvantage of close involvement is that field subjects may be less open and honest with the researcher in cases where he or she is perceived to have a vested interest. There is a further danger that the closely involved field researcher becomes socialized to the views of the people in the field and loses the benefit of a fresh outlook on the situation. With respect to action research, there is a risk that researchers may lose critical distance on the value of their own contribution, and perhaps represent it in too positive a light.

So, it is possible to weigh up the advantages and disadvantages and make a choice, but only to some extent. In some cases, close involvement is not possible, as the organization does not permit it. A subtler point in terms of freedom of choice for the researcher is that continued involvement with the field situation, regardless of one's starting position, can push the researcher towards a more involved stance. We faced such a situation, described in Walsham and Sahay (1999), when we spent 3 years working on a longitudinal field study of GIS implementation for district-level administration in India. Although we started off as relatively neutral observers, we felt the need to offer direct advice and help as time went on, as this was expected by the field personnel in return for the time and effort they put in for us. Indeed, we argued that we felt a particular moral imperative to do so in this case, because a refusal to offer constructive suggestions would reflect a lack of concern for the people in Indian districts, whose economic prosperity is among the lowest in the world.

Gaining and maintaining access

Whatever the style of involvement, interpretive researchers need to gain and maintain good access to appropriate organizations for their fieldwork. I say to Ph.D. students that the most important attributes that they need for this are good social skills, but I cannot teach them how to acquire them! More seriously, individuals can work to improve their social skills if they have the courage to confront their current position in this respect, through self-reflection and with input from others such as friends, colleagues and supervisors. A second characteristic that a field researcher needs is a willingness to accept 'no' for an answer but the persistence to try elsewhere. The first three organizations that one approaches may say no to access, but the fourth may say yes. In some cases, it is possible to obtain access by approaching the research from different angles. For example, access to the London Insurance Market was initially denied to us by certain central market participants, but we succeeded in getting

access through independent brokers and underwriting firms, resulting, in the end, in an in-depth study of the market as a whole (Barrett and Walsham, 1999).

The process of gaining access, as outlined above, entails strong elements of chance, luck and serendipity. However, this does not mean that any organization is OK. For example, if the research topic is ERP implementation, it is essential that the researcher gains access to an organization or organizations that have actually implemented ERP in a substantial way. If all approaches to such organizations fail, then the researcher must try again or change their research topic. Does access to a limited set of organizations, or even one organization only, necessarily remove the possibility of generalizability? My answer to this is a clear no, as I stated in my 1995 paper, as generalizations can take the form of concepts, theories, specific implications or rich insights. Lee and Baskerville (2003) have done a much more comprehensive demolition job than I did in my earlier paper on those who say 'you can't generalize from a single case study'. They describe a generalizability framework with four components: from data to description; from description to theory; from theory to description; and from concepts to theory. All of these are feasible from a single case study or a small set of case studies.

In terms of maintaining access, I again tell Ph.D. students that good social skills are essential. One should be either liked or respected by the field personnel, and preferably both. As an example, consider the issue of time and length of an interview. Staff in contemporary organizations, as we all know, are normally very busy and pressured. It is essential to be sensitive to these time pressures in fixing a suitable interview time and then not overstaying one's welcome during the interview. Better to finish interviews and lose some interaction time, if interviewees are clearly pressured, than to irritate them by taking too much of their time. Stories about researchers pass rapidly around an organization, and being a nuisance is not only a morally dubious practice, but is instrumentally foolish.

A further point on maintaining access is that organizations will normally respond well to the offer and delivery of various forms of feedback. I believe that interpretive IS researchers should offer feedback of some sort if they are asked to do so, even if they are adopting the role of neutral observers. The form of the feedback can involve a single presentation to a group of participants from the field site to a more intensive workshop involving a wider range of participants over a longer time period. We undertook such a workshop in our GIS study referenced earlier (Walsham and Sahay, 1999) in order to try to give something of value back to those who had participated in the research study, and to Indian Government GIS practice more generally. A report is another type of feedback, although I tend not to use this form much these days for two reasons. Firstly, a presentation or workshop involves a two-way interactive approach from which both participants and researchers can learn,

whereas a report by itself is a rather static object. Secondly, it is harder to say controversial or critical things in a tactful way in a report. I will return to this point later in the section on ethical issues.

Collecting field data

Interviews are a part of most interpretive studies as a key way of accessing the interpretations of informants in the field. I have commented above on the need to maintain good timekeeping during an interview, and I discussed the important balance between passivity and over-direction in Walsham (1995). One other point that I would add about interviewing is that it is crucial to try to reassure the interviewee at the start about your purpose and about confidentiality. One tactic in this respect, particularly if the interviewee appears nervous, is for the researcher to do most of the talking for the first few minutes. The interviewer may 'lose' some precious interview time, but if it succeeds in its purpose of getting the interviewee to relax, then the quality of the rest of the interview is likely to be much higher, in terms of honesty of response for example.

This brings me to the perennial topic of tape-recording of interviews. I will simply distil my thoughts on this topic into a list of advantages and disadvantages. One advantage is a truer record of what was *said* compared with the taking of notes during the interview, no matter how extensive. It is possible to return to the transcript later for alternative forms of analysis, and it is useful for picking out direct quotes when writing up. It frees the researcher to concentrate on engaging with the interviewee. Finally, it is popular with neo-positivist reviewers in some establishment journals. Against this, it is very time-consuming and/or expensive to do transcriptions and then to extract themes, and this time could be spent elsewhere, for example, on more interviews or analysis. A crucial disadvantage for me is that tape-recording may make the interviewee less open or less truthful. Finally, tape-recording does not capture the tacit, non-verbal elements of an interview, which are crucial aspects of the experience for the researcher. We may not know exactly how we assess people, as human cognition remains something of a mystery, but we do know that we do not judge people's views or attitudes solely on what they *say*.

Interviews should be supplemented by other forms of field data in an interpretive study, and these may include press, media and other publications on the sectoral context of the organizations being studied. Internal documents, if made available, may include strategies, plans and evaluations. Direct observation or participant observation of action is a further data source. Web-based data from e-mails, websites or chat rooms can be very valuable. Finally, surveys can be a useful complement to other data sources.

This last point can be generalized to the comment that interpretive does not equal qualitative. Quantitative data, from surveys or elsewhere, are perfectly valid inputs for

an interpretive study. This does not mean that they are needed in all interpretive research projects. However, they should be considered as part of the possible portfolio of methods for any specific study. Mingers (2003) provided useful data on the frequency of multi-method research in the published IS literature, showing an overall incidence of around 20% in the six journals surveyed over the period 1993–2000. An author who has long advocated the combination of quantitative and qualitative methods is Kaplan (e.g. Kaplan and Duchon, 1988). In a recent study, Kaplan *et al.* (2003) describe the use of a combination of quantitative surveys and ethnographic interviews to evaluate a telephone-linked health-care system. The survey demonstrated overall positive responses to the technology, complementing the ethnographic interviews that 'indicated more subtle and surprising reactions', notably that some individuals read 'personal relationships' into their technological interaction.

Working in different countries

I have carried out interpretive IS field research in about 16 different countries and four continents (see Walsham, 2001 for some examples). With respect to similarities, my experience suggests to me that interviewee responsiveness rests on similar bases throughout the world. People are normally willing to talk about themselves, their work and their life, with reasonable openness and honesty, provided that they perceive the researcher's sincerity of interest, feel that they understand the researcher's agenda, and trust the researcher's statements on confidentiality. This is not to say that ways of achieving this mutual trust are the same in all contexts, nor that I, or anyone else for that matter, can always succeed in gaining that trust with everyone.

However, in addition to basic similarities, there are also differences between countries. So, if you plan to do some field research in a particular country, do plenty of homework about the country beforehand and during the research. Much can be learned about the history, politics, religion and ways of living in a country by reading from both fictional and non-fictional sources. The myth that travel broadens the mind is a persistent one, but many aspects of international tourism demonstrate the exact opposite, where people travel in body but not in spirit. The researcher should try to be 'there' in both body and spirit. This reminds me of Ciborra's (2004) call for a redefinition of situatedness to include emotions and feelings, not merely cognitive processes. By the way, all of us can learn a lot about our own country, and, therefore, how to carry out research in it through similar processes of reading, reflection and attempts at emotional empathy. The view that one 'knows' one's own country in any full sense is clearly wrong.

A final practical matter in terms of field research away from one's own country is that of language. It is clearly better to be able to speak the local language fluently in order to carry out field research there. However, this is

not possible in all cases. For example, in terms of field research that I have carried out in India, only a small proportion of Indians speak English, mainly the highly educated. There is no common other language either, although Hindi is widely spoken in the north of India. But field research is possible through translators. For example, I have carried out field studies in the south of India with a research collaborator who spoke the local language. I have also conducted interviews in Japan using a member of the software outsourcing organization I was researching who spoke Japanese. I have not personally used professional translators, who would be expensive and not necessarily knowledgeable about the particular topic of the research, but I would not wish to rule them out. My main point is that one should not be irrevocably deterred from talking to people by not speaking their language.

Theory and data analysis

I have given methodology talks to doctoral student groups in many institutions and countries, and the topic of theory always comes up as something perceived to be essential but difficult. I addressed this topic to some extent in Walsham (1995) where I argued that theory can be used as an initial guide to design and data collection, as part of an iterative process of data collection and analysis, or as a final product of the research. I stand by these distinctions now, but in this paper I go a little further by describing and analysing the role of theory in four specific case studies published over the last 10 years or so. I then discuss the issue of how to choose a suitable theory. Finally, in this section, I examine the related topic of data analysis, something that I did not treat directly in my earlier paper.

Role of theory

I will start with an early paper by Orlikowski (1993) on the use of CASE tools, based on her field research in two different organizations. Orlikowski described the use of grounded theory (Glaser and Strauss, 1967), including the techniques of open and axial coding, to generate a set of concepts from the field data. She brought these concepts together into a theoretical framework for conceptualizing the organizational issues around the adoption and use of CASE tools. Interestingly, Orlikowski connected the grounded theory derived from the data with aspects of 'existing formal theory', in this case the distinction between incremental and radical types of change from the innovation literature. We do not learn why Orlikowski chose this latter theory and, indeed, the researcher herself does not figure in the account of the case study. The study, published over 10 years ago, has a rather 'hard' feel to it, with the data analysis techniques sounding highly 'scientific', and with nothing on the subjectivity of the researcher.

Moving on in time, Walsham and Sahay (1999) described the use of actor-network theory (cf. Callon, 1986; Latour, 1987) to analyse the case study of GIS

implementation in India mentioned earlier in this paper. The authors stated that the theoretical basis of their study 'evolved over time' in response both to the field data and to reading further about theory in related fields. In the privileged position of one of the authors of this paper, I can say that our use of actor-network theory was a relatively late idea based on an increasing interest in the theory as a specific way of conceptualizing the role of ICTs. We did describe our own role as researchers in some detail in the paper, but this description of researcher subjectivity was a late addition at the request of one of the paper's reviewers.

In contrast to the papers discussed above, Schultze (2000) put the researcher herself on centre-stage in her 'confessional' account (Van Maanen, 1988) of an 8-month ethnography about knowledge work. She studied three groups of knowledge workers in a particular organization, and derived a synthesis of their work practices as involving expressing, monitoring and translating. She argued that a common feature of their work was their balancing of subjectivity and objectivity. The confessional element to the paper is that Schultze compares herself to her research subjects, arguing that she also, as a knowledge worker, engaged in similar knowledge tasks and needed to balance her subjectivity and objectivity. In terms of use of theory in this study, grounded concepts were generated from the field data, but the theoretical insights about the researcher's role appeared to derive largely from Schultze's study of the anthropology literature.

Rolland and Monteiro (2002) described a case study of the attempts of a maritime classification company, with headquarters in Norway, to develop an infrastructural information system to support the surveying of ships globally in over 100 countries. Their use of theory was much looser in style than the papers discussed above. The authors described elements of their 'theoretical grounding' as coming from two main bodies of literature: the first arguing the benefits of standardized solutions; and the second emphasizing local variation and the need to design for local specificity. Rolland and Monteiro described and analysed how standardized solutions and local resources were moulded and meshed fluently in the ongoing use of the information system, but this was not a costless process. Some elements of the researchers' role are briefly outlined in the paper. This included the statement that their approach to research was 'inspired by actor-network theory', although this received only a passing mention.

Choosing theory

The papers discussed above can be used to illustrate a number of points about the choice of theory. Firstly, there was considerable diversity in the theories chosen, and they were chosen at different stages of the research. Secondly, theory can be used in lighter or tighter ways, both of which have their merits. But a crucial third point for me is that the choice of theory is essentially

subjective. Even in the papers where the researcher's role is discussed in detail, which tend to be the later ones, there is a missing ingredient. So *why* did Orlikowski choose theory from the innovation literature, Walsham and Sahay choose actor-network theory, Schultze choose a confessional account, and Rolland and Monteiro use both literatures on standardization and localization? The answer lies in the researcher's own experiences, background and interests. They chose a particular theory because it 'spoke' to them. Of course, the authors then had to justify that choice in their papers by arguing how it was relevant to the research topic and the field data, but nevertheless the choice was their own.

So, my first piece of advice for new researchers is for them to choose theories which *they* feel are insightful to *them*. Do not choose a theory because it is fashionable, or because your supervisor likes it, if it does not really engage you. You will be required by reviewers and others to defend the reasons why your theory is a good one for the purpose, and perhaps why it is not one of the current 'hot theories' in the field. You must have convinced yourself first if you are to have any chance of convincing others. For example, Sundeep Sahay and I chose to write a paper based on actor-network theory because it seemed to fit so nicely with the Indian field data that we had, and offered some insights that other theories did not. Whether we convinced readers of our paper is another matter, but at least we believed it ourselves.

Although choice of theory is subjective, I would like to end this sub-section with a few general points that can perhaps help the choice process. Do not fix on one theory and then read nothing else. Do not leave theory generation to the end of your study. I will comment on this below in terms of data analysis. Do not dismiss a theory's value until you have read about it in some depth. On the positive side, do listen to what others find valuable as a source of possible inspiration for yourself. Finally, do read widely on different theories, as this will offer you a broader basis on which to choose, and more likelihood that you will find a theory or theories which inspire you, and enable you to gain good insights from your field data.

Data analysis

Theory provides one of the ways in which your data can be analysed. For example, suppose that you have used actor-network theory as a guide for data collection in your field study. You would have tried to follow the actors and networks, and attempted to understand issues such as inscription, translation and other processes associated with the theory. This means that you would have already carried out a data-theory link, and could of course choose to write material based on this. Alternatively, you may decide that other theories are potentially valuable ways to view your data. In which case, try to develop the data-theory link in some explicit form, a presentation or working paper, and ask colleagues and others to comment on this material.

In terms of learning from the data itself, grounded theory offers one way of doing this, although the 'coding' is a subjective process to some extent, because the researcher chooses the concepts to focus on. I tend to use a looser approach myself, where I write impressions during the research, after each interview, for example. I generate more organized sets of themes and issues after a group of interviews or a major field visit. I then try to think about what I have learnt so far from my field data. If this sounds a rather subjective and relatively unplanned process, well it is. I believe that the researcher's best tool for analysis is his or her own mind, supplemented by the minds of others when work and ideas are exposed to them.

This brings me to qualitative data analysis techniques through packages such as Nudist. Such software can be a useful method to link themes to specific pieces of text in your notes or transcripts. However, the method has some major disadvantages. It is very time consuming and is sometimes a form of displacement activity, in place of the harder work of generating themes in the first place, or making data-theory links. The software does not remove the need for thought, as the choice of themes remains the responsibility of the researcher. A final disadvantage that I observed when one of my own Ph.D. students used the method was a tendency, owing to the large effort of linking data to themes, to get 'locked in' to the themes as the only way to look at the data.

Constructing and justifying a contribution

Let us now suppose that the fieldwork has been completed, data have been analysed, and theories have been selected. Now is the time to communicate your work to others, although of course you should have been doing this at stages throughout the research. Nevertheless, the writing of a Ph.D. thesis, articles, conference papers or books should be the culmination of any serious piece of academic work. The focus of this section is on the processes of constructing a contribution and writing in order to achieve recognition of merit by others. The goal may be to gain a Ph.D., to get a paper accepted for a conference or journal or, more difficult, to have your work cited by others at a future time. I will start by discussing approaches to justifying the research work that has been carried out.

Justifying your approach

There has been some interest in recent years in the interpretive IS research community on appropriate ways to justify the methodological approach adopted in a particular study. For example, papers were solicited for a special issue of *MIS Quarterly* on intensive research, and one of the requirements was that the papers should state explicitly their criteria for methodological appropriateness. The paper by Walsham and Sahay (1999) was one of the papers accepted for the issue, and we used the three criteria by Golden-Biddle and Locke (1993) of authenticity, plausibility and criticality. Authenticity concerns the

ability of the text to show that the authors have 'been there', by conveying the vitality of life in the field. Plausibility focuses on how well the text connects to the personal and professional experience of the reader. Criticality concerns the way in which the text probes readers to consider their taken-for-granted ideas and beliefs. We give detailed examples of how we tried to achieve these goals in our article.

A rather more comprehensive approach to methodological justification of interpretive field studies in IS was provided by the seven criteria in Klein and Myers (1999). The criteria were derived from the study of the anthropology literature, and the philosophy of phenomenology and hermeneutics. In very brief terms, the criteria involved demonstrating that the researchers have applied a hermeneutic approach with critical reflection to the social and historical background of the study and their own role in it; they have demonstrated multiple interpretations of the participants and shown how data findings sometimes contradict earlier theory, and related the findings to theory, showing sensitivity to biases and distortions. Klein and Myers then evaluate three interpretive IS papers from the literature to see how well they stand up to scrutiny through the criteria.

I do think that it is valuable for IS researchers to think about their own work in relation to the Klein/Myers or Golden-Biddle/Locke criteria. I would, however, like to offer a 'health warning' on their use. As noted by Klein and Myers themselves, a particular study could illustrate all the suggested principles and *still not come up with interesting results*. It is essential that researchers are not misled to confuse process with outcome. So it is insufficient to say that 'I have applied the principles'. It is essential to say 'Here are my interesting results'. I now turn to approaches to constructing such results.

Constructing a contribution

I sometimes say to students that their writing looks as if they are addressing themselves, or in other words they are not thinking enough about potential readers. Now we do not normally know who all our readers will be when we write something, but we can at least construct our piece to aim at a particular type of *audience* or audiences. In addition, we can ask to what *literature* we are aiming to contribute. Thirdly, what does the piece of written work *claim to offer* that is new to the audience and the literature? Finally, how should others *use* the work? These questions may look very simple and obvious. However, I review many prospective journal articles and conference papers every year, and a large proportion of them do not address at least one of the above questions at all, and a significant proportion do not seriously address any of them.

Let me use Klein and Myers (1999) again, but for a different purpose, because it provides a nice illustration of a paper that answers all the above questions in a very clear way. Firstly, the key audience for the paper is interpretive IS researchers who want to reflect on their

methodological approach and defend it in their writing. Secondly, the literature that Klein and Myers wish to contribute to is the interpretive IS literature in general, and that dealing with methodology in particular. They claim to offer a set of principles, based on their reading of anthropology and philosophy, which can be used to evaluate the methodological approach of an interpretive IS study, and they illustrate their claim by applying the principles to three such articles. Finally, on use by others, Klein and Myers say that 'authors may find it useful to refer to the principles when their work is submitted for peer review'. If readers act on this advice, Klein and Myers will have a well-cited paper in the years to come!

The above questions provide a broad framework for how to construct a contribution but they do not go into the finer details. I tried to address one aspect of this in Walsham (1995) in discussing how to generalize from interpretive studies, as outlined earlier in this paper. In a later work (Barrett and Walsham, 2004), we use these ideas and others to examine how papers aim to make contributions by examining processes of article construction by authors and use in the literature by citing papers, illustrated by a detailed examination of one particular paper, namely Star and Ruhleder (1996). We discuss, for example, ways of both reflecting the existing literature but identifying gaps; and framing material for particular audiences, highlighting what they should find interesting. We hope that such ideas are helpful to prospective authors, but we recognize that they do not do the actual writing work for them. I turn now to this crucial topic.

Writing

I quoted Van Maanen (1989) in Walsham (1995) as reminding us that that writing is an act of persuasion that is as much about rhetorical flair as it is about care in matters of method and theory. It is possible for any intelligent person to learn to write grammatically, but it is much harder to develop an interesting and readable style of writing. Practice and more practice is the only way in which to improve. Responding to constructive criticism is a good way to learn, for example, by working with co-authors who write well. If you are not a native English speaker, but are required to write in English, you will often need additional help. However, do not hide behind this to justify poor writing. If you can write well in your native language, and are fluent in spoken English, you can learn to write well in English.

Although I can not teach readers how to write well through the medium of this article alone, I wish to offer some thoughts on the organization and conduct of writing based on my own experience of it, and of reading the writing of many other people. I will start with structure. For example, suppose one is planning to co-author a paper with a colleague. The approach that I adopt is to create a detailed outline, including sections and sub-sections. I estimate the length of the various sections in words, so as to try to create a balanced use of the space available. I write some brief material on each of

the sections and sub-sections to outline their contents and how they will connect. I then discuss the outline with the colleague, possibly going through further iterations to create an agreed approach. Now, plans do not always match what happens, so the paper normally evolves during the writing. However, this is not an argument to have no plan. I create an outline, even when I am the sole author. If the work is a thesis or a book, rather than an article or conference paper, then a good outline is absolutely essential, and often required in the case of book publishers.

I will now briefly comment on the various elements of a journal paper. The *title* should be sharp, and focused on your contribution. Look at titles from well-cited authors such as the paper by Orlikowski (1993) discussed earlier: 'CASE tools as organizational change: investigating incremental and radical change in systems development'. The *abstract* should summarize the whole paper – it is not an introduction. The abstract should say why the topic is important, what you have done and what are your key conclusions. Write the abstract early in the writing process. It is a good test as to whether you know what you are trying to say in the paper and why.

The *introduction* to the paper should not, in my view, be very long. It should say why the topic is important, and how you are planning to develop your contribution in the paper. It should outline carefully the structure of the rest of the paper. The *literature review* should not be a listing of what you have read from others on the topic. It should be a structured review of interesting aspects of the literature, but showing why your paper is needed. You can include references to work that you don't like, to make a point, but be polite! With respect to the *methodology* section, reporting on 'soft' human issues is not an excuse for sloppiness, as noted in Walsham (1995). I gave some guidelines there as to what should be included, and I would add the need to describe the researcher's own role in the process. You won't get a paper published in a good journal just because you have a well-written methodology section. However, you might get a paper rejected by a good journal because your methodology section is weak.

In describing the *empirical data and analysis*, try to make it a coherent and interesting story for the reader. For a case study for example, it is often helpful to provide an overview before going into details. Use plenty of quotes from respondents, as they can often make a point really sharply and vividly, however, make sure that you have introduced the point you are trying to make first, rather than making the quote 'do the work'. Tables and figures can sometimes be useful to summarize key arguments and models in the text. Finally, with respect to the *discussion and conclusions*, remember to focus on your claimed contributions. How do they advance our knowledge of the research topic? How do they extend the existing literature? To what extent are your results generalizable to other contexts? Pay attention to a good upbeat ending.

Ethical issues and tensions

In the conduct of interpretive research studies over the years, I have experienced a number of ethical issues and tensions. Very little is written in the IS literature on this topic, although I know that other researchers feel similar pressures also. There is a sizeable literature in the social science field in this area, but it does not offer simple answers to ethical questions regarding the conduct of empirical work. For example, Bryman (2001) quotes Diener and Crandall (1978) as identifying four main areas of concern: harm to participants, lack of informed consent, invasion of privacy, and deception. However, while this is a sensible classification, experienced researchers will know that there are many grey areas. One is often unsure about potential harm, cannot always enable fully informed consent, do sometimes invade some elements of privacy, and may 'deceive' about the precise aim of one's research. These 'transgressions' are normally justified on the teleological grounds of getting the research done properly, but there is a fine line to be trodden here.

In order to contribute to the debate on important ethical issues of research conduct, such as those outlined above, I will discuss three specific domains of practice in this section: confidentiality and anonymity; working with the organization; and reporting in the literature. I do not have any simple answers as to what is the right thing to do in each of these domains, but I would like to offer some of my own practical experience on ethical issues in these domains, and how I have attempted to resolve them in my own work.

Confidentiality and anonymity

I always offer confidentiality to individuals I interview. More specifically, I say that I will not identify them by name or by giving their specific position, either in written work or in reporting back to internal organizational sponsors. I would mention two problems that I sometimes encounter here, even though I certainly try to honour my stated position. One is that, when reporting back to organizational sponsors, after a field site visit for example, it may be possible for individual sponsors to make an informed guess as to whom particular views may be attributed, even though the researcher uses no names or positions.

A second ethical tension occurs when reporting in the literature on the views of senior people, namely that a little detective work on the part of the reader may enable them to make a good guess as to who is being discussed. For example, I reported in Walsham (1993) on 'Sky Building Society', a pseudonym, and included much about the CEO of the Society at the time of the research. It would not be too difficult, on the basis of the contextual information provided, to do some research to identify Sky and thus the CEO. Most readers will not do this of course, but nevertheless some breach of confidentiality is potentially possible. Indeed, one post-experience management course that I personally taught

included some individuals who had been involved with 'Sky' and immediately recognized the real company.

This last example relates to the general point as to whether one should give the actual name of the organization when reporting on it in the literature. There was an interesting debate on this topic in the *European Journal of Information Systems* around 10 years ago. Liebenau and Smithson (1993, 1994), at that time the co-editors of *EJIS*, argued the benefits of disclosure, such as being able to provide much better contextual information, and enabling readers to validate the empirical work based on their own knowledge of the organization. Hirschheim and Lyytinen (1994) argued against this, for reasons such as that 'organizational disclosure makes many relevant IS topics unresearchable'.

I became worried myself at this time, stimulated partly by the *EJIS* debate and partly by a call for papers for the European Conference on Information Systems *requiring* disclosure. Therefore, I wrote Walsham (1996) in which I argued a compromise position of organizational non-disclosure only when necessary for access or ethical reasons, and even in those cases good contextual information should be provided. My current position is similar to my views expressed in 1996, but the disclosure issue has not gone away. Indeed, it seems even stronger to me in the highly competitive era in which we are currently located, where organizations are very sensitive about their external image.

Working with the organization

The first ethical tension I will mention during the carrying out of actual fieldwork is the gap which often is there between the expressed purpose of the research, in written or verbal form, and the broader agenda of the field researcher. For example, I never mention power relations as something that I plan to investigate, as power is normally not something which is openly talked about with 'outsiders', almost a dirty word in contemporary organizational life. Yet, power is endemic and crucial to all activity, and thus must be addressed by the interpretive researcher. Indeed, power often provides a valuable basis for analysis, and we are back again to reporting on things that organizations may not want reported. Is it legitimate for the researcher to say that they are researching ERP implementation or whatever, when actually they are focusing on endemic power relations? I tend to think yes, on the grounds that power is a crucial element in systems implementation and must therefore be looked at, but the fact that I do not include it in my written research agenda indicates some personal tension here.

Another ethical issue that I have often encountered in fieldwork is how and whether to give 'bad news' to organizational sponsors. Let us suppose that I have visited a field site in a country that it is a long way away from the headquarters of a multinational organization. I am being given access by IS staff at headquarters who are 'rolling out' a new system worldwide across the company.

I visit the field site and discover that there is little local use of the system, and indeed some hostility to it. Apart from the confidentiality issues discussed above in disclosing the views of local staff, a further tension in reporting the bad news to headquarters staff is that they will not like it. It is always difficult to be the bearer of bad news, and the 'shoot the messenger' approach is alive and well in contemporary society. For a research project, this can mean restriction or cessation of future research access.

The above example, as may have occurred to the reader, is not a hypothetical one. Indeed, I encountered it last year. The tendency, because of the above concerns, is to sugar the pill, namely to soften the feedback somewhat and try to phrase it in the language of opportunities rather than problems. However, this has an ethical side to it also, as is it good moral behaviour to shirk from telling the truth as one sees it because of potential future consequences? I have no simple answer to this question, which is related to deep philosophical issues of deontology vs teleology, but it is something that will confront many interpretive IS researchers during their work. As I discussed briefly earlier in the paper, it is probably easier to give 'bad news' in a presentation or workshop than it is in a written report.

Reporting in the literature

I sometimes write articles with practitioners, either from the organizations being researched, or from organizations providing funding for the research. This is a good thing to do in my view, but it does involve some compromise, and related ethical issues. Again, the organizational participants do not want their organization to be portrayed negatively in published works with their name on it. This is understandable, but raises again the moral dilemma of truthful reporting against expedient reporting. We are normally talking shades of grey here, rather than black and white. A relatively light-hearted example from my recent experience is where a co-author from a technology vendor organization did not want me to write about 'problems' with the use of technology, but rather about 'potential disjoints in ICT support'.

A bigger dilemma in my view is the articles that I write as sole author or where I co-author with an academic colleague. I never agree to show *all* my work to sponsoring organizations, whether research sites or funding organizations. This can be justified on the grounds of maintaining academic integrity to write critical pieces in both the academic and common language use of the word 'critical'. The tension which I feel is that some of my research respondents would not only disagree with my interpretations in some cases, but I suspect that they may feel hurt by some of the critical comments, no matter how carefully I write to try to avoid giving unnecessary pain. It could even, in some extreme cases, be regarded as a betrayal of the trust between the researcher and researched.

Now one can try to rationalize this concern in a number of ways. How can we learn from mistakes if everyone writes about organizations as if things were perfect? More generally, the whole project of critical research breaks down if honest reporting is banned. One can also rationalize that respondents are unlikely to read papers in academic journals anyway. In addition, long lead times between article submission and publication mean that the organization will have moved on by the time that the article is in print, and thus critical comments about a past era are likely by then to be less problematic for organizational members. All of these points have some validity in my view, and taken together they make a strong case for the publication of critical studies with suitable anonymity. I intend to continue to do so, but I will no doubt continue to experience the ethical tensions articulated here.

Conclusion

I aimed in this paper to reflect on and extend what I wrote in Walsham (1995) on the nature of interpretive case studies and methods for carrying them out. I have broadened the brief to interpretive research more generally, including, for example, ethnographies and critical research in an interpretive style. I have written more on the conduct of fieldwork and the role of theory, including new topics such as working in different countries and data analysis methods. The material on constructing and justifying a contribution draws on work that has largely been carried out over the last decade since the publication of my earlier article. The section on ethical issues and tensions is completely new, and little has been written to date on this in the IS literature.

I said at the beginning of the paper that it is not a blueprint of 'how to do it', but I hope that it offers some useful areas for reflection for the interpretive IS researcher, and in some cases direct advice. Interpretive research is here to stay in the field of IS. Those of us who identify ourselves with this research tradition need to articulate more carefully how we conduct our work, and how we organize and justify our research contribution. My paper is one input to this continuing process, and it may indeed be useful to interpretive researchers more generally,

About the author

Geoff Walsham is a Professor of Management Studies at Judge Business School, University of Cambridge. His teaching and research is centred on the social and management aspects of the design and use of information and communication technologies, in the

rather than solely to those working in the IS area. I have given talks for a number of years to wider audiences, such as those concerned with interpretive research in management studies, and many of the issues discussed in this paper are relevant to them also.

One of the reviewers of the first submitted version of this paper suggested that I should evaluate my own paper using my criteria for constructing a contribution described earlier. I will end by doing this. The primary *audience* I am aiming at is less-experienced IS researchers, but the paper is also aimed to stimulate reflection by the more-experienced IS researcher, and can also be of relevance to interpretive researchers in other social science fields. The *literature* I am aiming to contribute to is the interpretive IS literature, and more generally, the literature on the conduct of interpretive studies. What do I *claim to offer* that is new to the audience and the literature? A more difficult question to answer, because the answer is partly reader-specific; however, I hope that the detailed examples, insights and reflection from my own interpretive work over two decades offer something new to all readers, but particularly to the less-experienced IS researcher. Finally, I suggest that the paper can be *used* for personal reading and reflection, or discussion in forums such as seminars on research methodology. However, whether you like the paper is your choice, and not within my control.

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context of both industrialized and developing countries. His publications include *'Interpreting Information Systems in Organizations'* (Wiley, 1993), and *'Making a World of Difference: IT in a Global Context'* (Wiley, 2001).

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