The ethnographic tradition and design

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This paper reflects upon the emerging uses of ethnography in engineering and systems design. Although ethnography is often equated simply with fieldwork*, a driving force in the early development of* classical ethnography *was to provide accounts for what is observed in terms of* a priori *anthropological and sociological theories. Recent studies of collaborative work systems informed by* ethnomethodology *have exposed the shortcomings of classical ethnography, and have shifted the emphasis of fieldwork towards describing the accountable practices through which those in work constitute and organise their joint activity. However, a second wave of fieldwork-based studies of work systems and design is now gaining force that threatens to dilute this analytic emphasis. Using examples drawn from the production printing industry, I argue that recent examples of* scenic fieldwork—*fieldwork that merely describes and codifies what relevant persons do in the workplace—may well be missing out on the constitutive practices of* how they do what they do*, the 'interactional what' of their activities. Rather than ethnography, or even fieldwork itself, it is the explication of* members' knowledge—*what people have to know to do work, and how that knowledge is deployed in the ordering and organisation of work— that provides the key to understanding the contribution of sociology to engineering and design.* © 2000 Elsevier Science Ltd. All rights reserved

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The ethnographic tradition of social investigation has been developed mainly within the human science disciplines of anthropology and sociology. One of its contributions to social investigation has been the demonstration that culture can be viewed from 'inside' its arrangements and relationships. Both anthropology and sociology, however, have a debunking streak, and ethnography has also attempted to reveal the fact that these social arrangements may not be as they are portrayed in the received versions of social structure or society. For example, founding studies in the development of ethnography in sociology were

1 Hughes, E C *The Sociological Eye: Selected Papers* Transaction Publishers, New Brunswick (1993)

2 Anderson, R J, Button, G and Sharrock, W W 'Supporting the design process within an organisational context' in **G de Michelis, C Simone and K Schmidt** (eds) *Proceedings of the Third Conference on Computer-Supported Co-operative Work-ECSCW '93* Kluwer, Dordretch (1993) pp 47–59

3 Anderson, R J, Button, G and Sharrock, W W 'Getting the design job done: notes on the social organisation of technical work' *Journal Of Intelligent Systems* Vol 3 (1993) pp 319–344

4 Bucciarelli, L L *Designing Engineers* MIT Press, Cambridge, Mass (1994)

5 Button, G and Sharrock, W W 'Occasioned practices in the work of software engineers' in **M Jirotka, and J A Goguen** (eds) *Requirement Engineering* Academic Press, New York (1994) pp 217–240.

6 Button, G and Sharrock, W W 'Practices in the work of ordering software development' in **A Firth** (ed) *Negotiations In The Workplace: Studies of Language in the Workplace* Pergamon New York, (1995) pp 159–180

7 Button, G and Sharrock, W W 'Project work: the organisation of collaborative design and development in software engineering' *Computer Supported Cooperative Work (CSCW)*, Vol 5 (1997)

8 Button, G and Sharrock, W W 'The production of order and the order of production' in **J Hughes, W Prinz, T Rodden and K Schmidt** (eds) *Proceedings of the Fifth European Conference on Computer Supported Cooperative Work* Kluwer, Dordrecht (1997) pp 1–16

9 Button, G and Sharrock, WW 'The organisational accountability of technological work' *Social Studies of Science* Vol 28 (1998) pp 73–102

10 Grinter, R 'Using a configuration management tool to co-ordinate software development' in **N Comstock, and C Ellis** (eds) *Proceedings of ACM Conference on Organisational Computing Systems* ACM Press, (1995) pp 168–177

11 Grinter, R 'Supporting articulation work using configuration management systems'

often concerned with groups considered to be different from the rest of society—bums, hobos, hookers, pimps, and gamblers—and conducted in odd locations—the street, public toilets, whore houses, and mental institutions. Received wisdom was that these marginal groups lead desperate, chaotic lives, outside of the ordered and regulated ways of the rest of society. However, ethnographic studies succeeded in revealing just how regulated, mundane and ordinary were the lives of people in these groups. In the light of what for some will be the tawdry origins of ethnography in sociology, readers of Design Studies might well ask: "what possible interest could such a tradition of research have for me?"

Part of the answer to this question resides in the way in which the ethnographic tradition in sociology has also involved the study of *work*. As early as the mid-1930s Everett Hughes, drawing on his anthropological training, initiated an ethnographically-based sociology of work, subjecting work, occupations, and the professions to sociological scrutiny[1]. Since then a considerable body of literature has developed that depicts various work related themes such as the value of work, changing perceptions of work, the management of social relations, and the formal and informal structures of work. Although the study of work is not pursued as ardently within sociology as once it was, within recent years there have been a number of studies that have used ethnographic fieldwork techniques to study work domains associated with the so-called generation of knowledge: science and technology. A number of these have concentrated upon engineering and design work[2–13]. In addition to these, there have also been a number of fieldwork studies of work domains unrelated to engineering and design, but which have been undertaken for the purpose of informing the design and engineering communities as they develop computer systems to be used in those domains of work[14–16].

There are many aspects of these recent developments that resonate with the ethnographic tradition in anthropology and sociology, especially the use of fieldwork techniques. However, there are also some ways in which these recent developments display serious departures from that tradition. First, some have distinct motivations which differ from those that have informed the ethnographic tradition. This tradition, as it has been played out in sociology, was primarily concerned with informing other sociologists, and to some extent, those who create and implement social policy. In this respect, ethnography was theoretically, methodologically and politically motivated. However, a characteristic of some recent ethnographies of design is that they have clearly *practical* goals, and often are carried out on behalf of the workgroups under study, or particularly in the area of computer-supported collaborative work (CSCW), to inform systems

designers hoping to introduce computer technology into those work domains. As some of the papers in this special issue suggest, studying engineering and design work may also be used to inform the very organisation of that work.

A second divergence between the recent studies associated with engineering and design is that the disciplinary background of those undertaking the studies has widened beyond anthropology and sociology. Although the initial ethnographic orientation in sociology developed many themes in common with social psychology, mainstream psychology has not trodden the ethnographic path. Recent ethnographic studies, however, have not only been undertaken by sociologists and anthropologists but also by psychologists, and interestingly some computer scientists have also engaged in this type of research. The third divergence is that a great deal of this work, though certainly by no means all of it, is not in fact informed by an ethnographic tradition at all but by a completely distinct and very small domain of sociology: *ethnomethodology*.

In this paper, I want to reflect upon one of the new emergent uses of ethnography in engineering and design. In the main, the other papers in this special issue focus upon the study of engineering design as work itself, with the intentions of contributing to the organisation of that work either through the design of support tools, or through the design of the work process. This paper, however, is set slightly apart from the others in that it focuses upon what it is to work with designers in the development of the object of their design and engineering, in this case the use of ethnography in the design of software systems. In considering this difference, I will attempt to extrapolate some of the arguments so as to reflect upon the ambitions of those who are studying engineers and designers. I want first, though, to consider the ethnographic tradition in the human sciences in more detail.

1 An ethnographic tradition

A first point that needs to be established is that when the term ethnography is being bandied about either in the study of engineering and design work, or with respect to the study of work for engineering purposes, it is mainly being used as a proxy for *fieldwork*. Often when ethnography is being explained, heralded, feted, or condemned, the champions or detractors are not talking or writing about ethnography but about field-work: the ethnographic tradition and what it has involved is an overarching presence, vaguely felt but never directly confronted in many of these examinations. The founding of ethnography by the anthropologist Malinowski[17] certainly involved fieldwork techniques. Thus his foundational series of studies of

12 Grinter, R 'Doing software development: occasions for automation and formalisation' in **J Hughes, W Prinz, T Rodden and K Schmidt** (eds) *Proceedings of the Fifth European Conference on Computer Supported Co-operative Work* Kluwer, Dordrecht (1997) pp 173–188
13 Sharrock, W W and Button, G 'Engineering investigations: practical sociological reasoning in the work of engineers' in **G Bowker, L Gasser, S Leigh and B Turner** (eds) *Social Science, Technical Systems and Co-operative Work* Lawrence Erlbaum, New Jersey (1997)
14 Heath, Luff, Hughes, Randall and Shapiro, in **L Bannon, M Robinson and K Schmidt** (eds) *Proceedings of the Second European Conference on Computer-Supported Co-operative Work* Kluwer, Dordrecht (1991)
15 Hughes, Randall and Shapiro; Bentley, Rodden, Sawyer, Sommerville, Hughes, Randall and Shapiro; Luff, Heath and Greatbatch; Harper, in **J Turner and R, Kraut** (eds) *Sharing Perspectives CSCW '92* ACM Press (1992)
16 Anderson, Button and Sharrock; Heath, Jirotka, Luff and Hindmarsh; Murray in **G de Michelis, C Simone and K Schmidt** (eds) *Proceedings of the Third European Conference on Computer Supported Co-operative Work* Kluwer, Dortrecht (1993)
17 Malinowski, B *Argonauts of the Western Pacific* Routledge, London (1967)

the Trobriand islanders involved him in living amongst them, learning their language, participating in their customs and collecting first hand recollections of their way of life, including their myths, magical incantations, history, folk-lore and stories. This immersion in the society or the tribe pioneered by Malinowski became a prerequisite for most subsequent anthropological studies, and anthropology established field work techniques as part of the methodological menagerie that makes up the human sciences.

Anderson[18], however, makes the point that there is more to ethnography than fieldwork: it also involves an 'analytic mentality'. Thus the object of ethnography, as it was established by Malinowski, is not just a matter of bringing back the myths of a culture, its folk-lore and the descriptions of activities or ceremonies that take place within it, and then presenting them in some literal fashion in terms of, for example, a vernacular description of what the anthropologist has witnessed or was told. Rather than providing a literal rendition of such matters, the anthropologist was involved in constructing an account or an interpretation of the role they play in the life of the tribe or the society. Thus Anderson reminds us that Malinowski does not simply describe, as he witnessed it and had told to him, the 'Kula Ring'. Instead, he explains it in terms of its role in Trobriander society: Kula functions as a method of social cohesion by promoting social integration. In this explanation, Malinowski is rendering a *functionalist* account of society, a social theory that was also being deployed by sociologists in accounts of their own societies, but without reference to fieldwork. Ethnography, as established by Malinowski and practised by a generation of anthropologists, consequently involves collecting 'data' about a collectivity through first hand participation in it, and then organising this data in terms of a social theory, in Malinowski's case, functionalism.

A driving force in the development of ethnography in anthropology has thus been to provide explanatory frameworks that will account for what is observed. This, however, gives a cast to observation. Ethnography in anthropology is not a two step process, the anthropologist first describing what he or she witnesses, and then separately offering an explanation of this information. The reason for this is that the explanatory framework conditions the observation or the description in the first place. Thus Malinowski's description of Kula is marbled through with the terms of his explanatory framework. It is described from the point of view of social cohesion, not from the point of view of the islanders themselves. How the Trobrianders apply the category Kula to their own activities is not Malinowski's concern: his concern is with the anthropological application of the concept of social cohesion to Kula.

18 Anderson, R J 'Work, ethnography and system design' in *The Encyclopaedia of MicroComputing* (1996)

2 *Problems in sociological description*

A second question that could then be raised in the context of the design and engineering community concerns the interest that a theory of society can have when applied to them, or to other occupational communities with which they may be professionally engaged. However, though this may be a legitimate question, there is one that as a sociologist I would want to ask first. This concerns the extent to which the generation of and application of theories of society to observations about the workings of society is the appropriate way in which sociology, itself, should proceed. A problem can be raised with the types of sociology that are conducted by applying theories of society to particular witnessed activities or groups, a problem that goes to the very heart of what it is to engage in 'professional' social description or inquiry. It is obviously not possible, nor relevant; to fully explore the dimensions or the consequences of this problem here, but allusion to it is important for examining the relationship between ethnography and design. The problem has two dimensions to it. First, the relationship between descriptions of social action produced by human scientists and the descriptions produced by those who perform them, and second, the loss of the phenomena.

2.1 *Professional and lay sociology*

In Malinowski's account of The Kula Ring we are presented with a professional description of social activity: The Kula Ring is an exercise in social solidarity. However, if we were to ask the Trobrianders what it is they are doing it is highly unlikely, even if Malinowski were correct, that they would answer in this fashion. Perhaps more relevant for present concerns is the fact that within the humans sciences there is, following Heidegger, a general move to regard technology and technologists in a sceptical light. Thus, technologists are regarded as *agents of power*. However, if a particular engineer or group of engineers were asked in the course of their work what they were engaged in doing, it is highly improbable that they would answer in this fashion, but rather would describe the work task in hand. Thus, for example, the engineering designer on a project developing a new digital printer may well respond to such an enquiry by describing how he was, at that moment, generating a representation of the machine's architecture. It would be highly remarkable if he were to respond with the description of what he was doing as 'contributing to world domination', unless he were kidding the poor sociologist observing him. The point is not that he is *not* contributing to world domination in doing his work—he may or may not be. However, the description that he is contributing to world domination is dependant upon, yet does not make visible, the 'lay' description of his activities as work tasks. This description remains absent

from the human science account, yet is presupposed by it. The social science account is, in this sense, a secondary account.

In the science and engineering disciplines, the scientist and the engineer can produce their accounts of matter in motion free of these kinds of worries. They need not concern themselves with the fact that the subject matter of their endeavours may dispute their accounts: molecules simply do not talk back. In the human sciences, however, members of society have well-organised descriptions of their own doings. Traditionally, the human sciences have proceeded by regarding these descriptions as truncated, partial, *ad hoc*, ideological, mere common sense, or in some other way deficient when set against the human scientists' accounts, which are deemed superior in kind due to their theoretical and methodological rigour. However, the terms of sociological description are very much the terms of common-sense description. Thus, to instance a founding study of sociology, Durkheim's examination of suicide[19], which was an attempt to establish that the scientific frame of reference could be applied not only to matter in motion but also to social doings, actually embodies a common sense understanding of suicide, in spite of Durheim's intentions. Although he relates suicide to social cohesion, as opposed to states of mind, climatic conditions, or other presumed causes of suicide prevalent at the time he was writing, he nevertheless uses the same understanding of suicide that is common in society. The reason for this is that he draws his data of the frequency of suicide from statistics produced by the coroner's office, and these statistics had been generated through the common-sense reasoning that was involved in applying the category 'suicide' to a particular case. Despite the exotic appearance of the task, the coroner employs common-sense reasoning in dealing with a mundane matter of assigning a category from a range of categories to the case in hand. After all, dead bodies are not found with labels on them. It is the work of the coroner to provide for the application of one of a range of labels to the case in hand. Has the dead body been produced through suicide, murder, misadventure, accident? The coroner must assemble and organise the application of one of these labels. Thus the social fact, the very *facticity* of the suicide, is not given, it has been produced through a social process. The terms of the professional account are, consequently, dependent upon the common-sense landscape: they are secondary descriptions.

Harvey Sacks[20] has proposed that the more interesting issue for sociology might be the investigation of how primary descriptions are assembled, how the category of suicide, for example, is assembled and deployed in local circumstances, rather than placing a sociological veneer over those activities. "An investigation of how it is that a decision that a suicide occurred

19 Durkheim, E *Suicide* Lowe and Brydone, London (1957)
20 Sacks, H 'Sociological description' *Berkley Journal of Sociology* Vol 8 (1964) pp 1–16

is assembled, and an investigation of how an object must be conceived in order to talk of it as 'committing suicide', these are the preliminary problems for sociology. Having produced procedural descriptions of the assembly of a suicide classification it may turn out that it is the category and the methodology for applying it that constitutes the interesting sociological problems[20]." Sacks' proposal here is very much in keeping with the domain of sociology known as ethnomethodology, which was founded by Harold Garfinkel[21]. Despite the similarity in name, ethnomethodology owes nothing to classical ethnography, and the two should not be confused. Ethnomethodology is a radical re-specification of the foundations of the human sciences, foundations shared by the classical ethnographers of anthropology. It shifts the emphasis away from the production of sociological accounts and theories of social doings to an emphasis upon the description of the accountable practices involved in the production of naturally organised phenomena[22].

Thus ethnography as a mode of social investigation, although seemingly articulating the arrangements of society from within, in fact demotes those as the primary object of concern in favour of secondary description. On this argument, classical ethnography as the application of social theories is then a dubious exercise. The interest that designers or engineers could have in ethnography is therefore put into question by what I am contending is the very problematic status and value that ethnography has for the human sciences themselves.

2.2 *The vanishing phenomenon*

What, then, of the *fieldwork* that is associated with ethnography? To a very real extent, investigations that currently call themselves ethnographies are not cast in the mode of classical anthropological ethnographies, as they do not necessarily propose the application of a favoured social theory to the particular group or work domain under study. However, the alternative to theorising proposed by Garfinkel and Sacks in the development of ethnomethodology could be based upon the fieldwork techniques associated with ethnography. Materials gathered from relevant settings, such as coroners' courts, could be used to answer the question posed by Sacks as to how the category of suicide is assembled and applied. Consequently, fieldwork could be used in the service of a sociology that does not fall foul of the problems of sociological description that have just been identified with ethnography. The question of the relevance or interest that ethnography might have for designers or engineers would be more appropriate and to the point if it were transformed into a question of the interest or relevance that *fieldwork* could have for them. However, even then the issue is clouded

21 Garfinkel, H *Studies in Ethnomethodology* Prenctice-Hall, Englewood Cliffs NJ (1967)
22 Heritage, J C *Garfinkel and Ethnomethodology* Polity Press, Cambridge (1984)

by a prior question that arises from the use of fieldwork in the human sciences.

I have been arguing that the use of ethnography in engineering and design is not the point, and now extend the argument by suggesting also that the point is also not just about fieldwork. An example drawn from one of the recent studies of the work of scientists illustrates this. Fieldwork has been used extensively to develop new understandings of the work of the scientist. However, although drawing on fieldwork materials, the studies that have emerged radically diverge from one another. Compare two of the seminal studies of laboratory settings, Mike Lynch's *Art and Artefact in Laboratory Science*[23], and Bruno Latour's and Steve Woolgar's *Laboratory Life*[24]. Both of these books are based upon materials drawn directly from observations of scientists working in their laboratories, observing what they were doing, and by participating in the daily round of the scientists' working lives. However, if we examine both books it is possible to find radical differences that show that just doing fieldwork evidently does not yield the same results. The reason for this divergence, however, is not that the two studies were examining different laboratory settings, but because they were done under different *analytic auspices*. For example, Lynch, operating under an ethnomethodological influence was attempting to describe the accountable practices of the lived work of scientists, whilst Latour and Woolgar were developing a divergent constructionist argument concerning literary inscription. Although both studies examined the issue of the role of *agreement* in science and both gathered instances of agreement from their observations in the laboratories they were studying, they emphasised different issues relating to agreement. Latour and Woolgar were interested in how facticity is a constructed phenomena in reaching agreement, whilst Lynch was interested in the production of agreement, and the mechanism through which agreement is organised. I have chosen this example because on the face of it both books may appear to be very close to one another. They are both based upon fieldwork, all the investigators engaged in participatory observation, and they both examine the same type of work site. Yet because their analytic orientations were divergent, then no matter that they may have used the same method, their analyses are of a different order with radically and analytically consequential differences.

The differences could have been predicted from remarks made by Harold Garfinkel about problems associated with some of the early ethnographies of work. One of the classic and influential ethnographies of work is a fieldwork study of jazz musicians, conducted by Howard Becker[25]. He gives a very detailed account of their working, based upon direct observation and participation in the daily round of the jazz musician. He fur-

23 Lynch, M *Art and Artefact in Laboratory Science: A Study of Shop Work and Shop Talk in a Research Laboratory* Routledge and Kegan Paul, London (1985)
24 Latour, B and Woolgar, S *Laboratory Life: The Social Construction of Scientific Facts* Sage, London (1979)
25 Becker, H S *Outsiders: Studies in the Sociology of Deviance* The Free Press, New York (1973)

nishes intimate information on the sort of music they play, the way they differentiate themselves from the rest of society—'the squares'—their habits, clothes, lifestyle, and the rest. However, in all the wealth of detail there is no account of how they make music together, of the interactional and improvisational 'work' of playing together. Garfinkel acknowledges an observation of Sacks', which is that studies of occupation miss the interactional 'what' of the particular occupation studied. As Lynch put it: "studies of bureaucratic case workers 'miss' how such officials constitute the specifications of a 'case' over the course of a series of interactions with a stream of clients; studies in medical sociology 'miss' how diagnostic categories are constituted during clinical encounters; and studies of the military 'miss' just how stable ranks and lines of communication are articulated in and as interactional work[26]."

Garfinkel's ethnomethodology proposes a domain of study that is concerned with the 'missing what' of the ordered and organised properties of activities, such as complexes of work activities. Such studies may well draw on fieldwork, but in as much as other studies miss the essential issue of the constitution of the work under study, fieldwork is not the point. The point concerns the analytic auspices that are brought to bear, and whether they preserve the practices through which those involved in work interactionally pull it off.

3 Design and studies of work

How do these issues play out in the world of design? I want to consider this question with respect to one domain of software design: collaborative work systems. It is fair to say that the idea of collaborative software systems has arisen in the CSCW field. It is also fair to say that CSCW originated in a reaction to the dominance of cognitive science in the area of human-computer interaction, with its emphasis upon individual behaviour and interface design. CSCW, however, focused less upon individual behaviour and more upon the collaboration: less upon interface design, and more upon how to support collaboration. CSCW also recognised the fact that computer technology is very often placed within work settings and organisational contexts within which complex divisions of labour already exist, and within which work activities are very often highly co-ordinated, and thus began to focus upon systems to support collaboration at work. Inevitably, it is necessary for the designer of collaborative work systems to gain some knowledge of the nature of the work that their systems will support.

26 Lynch, M *Scientific Practice and Ordinary Action: Ethnomethodology and Social Studies of Science* Cambridge University Press, Cambridge (1993)

However, a question arises as to how the designer can gain knowledge of the nature of that collaboration and work. The Scandinavian school of participatory design attempted to involve users in the actual design of sys-

tems and introduce knowledge about work, organisations, interaction, etc. into the design process[27]. At the same time Lucy Suchman, from Xerox PARC, emphasised in her critique of AI and cognition the idea of work practice as a resource in the design of systems to support work and interaction[28]. Both developments set the stage for the participation of sociologists interested in the naturalistic study of interaction, especially since those interactions and that work often involved computer technology. The relevance of a sociological participation was further underscored by the fact that studies of work often furnished evidence of the problematic design of a system, describing how the system detrimentally impacted peoples' work activities and even showing that a system had to be worked around. It is now the case that no CSCW conference seems to be complete without a naturalistic, observational study of work. Although they often describe themselves as ethnographic and while they certainly use fieldwork, these studies of work are not in fact ethnographic in classical terms, and are done primarily under the analytic auspices of ethnomethodology. The first wave of the relationship between 'studies of work' and design in CSCW has then broken. A very small band of sociologists, mainly adopting an ethnomethodological orientation, and a number of CSCW practitioners who were probably unconcerned with the complexion of the sociologists, just the practical outcomes of their investigations, have recognised that they can have a mutual interest in exploring the relationship between fieldwork based studies of work and systems design.

However, a second wave is now gaining force. A distinctive feature of this is that a number of CSCW practitioners are engaging in studies of work themselves, not just appreciating the studies produced by sociologists. These initiatives are, however, in danger of diluting the initial thrust of sociological studies of work for design purposes because although engaging in fieldwork may be important, it is not enough. Fieldwork may provide data about the organisation of work and collaboration at work, and about the use of technology at work. However, the telling issue is how that data is analytically worked. If the first wave could be described as *analytic fieldwork*, using data gleaned from fieldwork as material for analysis, the second wave could be described as *scenic fieldwork*, recording what is to be seen.

4 The 'interactional what' in production print scheduling

An example may be in order. Fieldwork-based studies of production printing reveal that production is managed by a number of people interacting with one another around organisational artefacts such as a production list,

27 **Greenbaum, J and Kyng, M** *Design at Work* Lawrence Erlbaum Associates, Hillsdale NJ (1991)
28 **Suchman, L** *Plans and Situated Action: The Problem of Human-Machine Communication.* Cambridge University Press, Cambridge (1987)

a scheduling board, work tickets and the like[29]. It would be perfectly understandable to produce fieldwork-based descriptions of the work of production printing describing these objects and the fact that they are being used, even how they are used. This sort of description may depend deeply upon the fieldwork, and may be of practical use in the design of production management systems. Thus it is possible to see that production management systems for the print industry available in the market place today are based upon an understanding of production management in commercial printers. Many of these products incorporate electronic representations of the objects that can be found in production settings, objects that fieldwork would bring to attention. The manufacturers of these systems could even claim to have improved upon them by, for example, automating various processes. Thus the scheduling tools on these systems can automatically schedule a job and be used to quickly run different production scenarios.

However, fieldwork that merely describes what relevant persons do may well be missing out on the constitutive practices of *how* they do what they do, the 'interactional what' of their complexes of action. Thus, if we were to use as a starting point of enquiry the fact that managers in the print factory manipulate various objects, such as scheduling boards, and asked the question 'what are they doing in using these objects?' we might begin to see important practices involved in their work that otherwise would remain concealed to us. If we only described what they did, we might well only be touching the surface of their work endeavours, the details of which would remain known only to them, or to others who could do their work. If we do ask these questions then we begin to see that these objects are used as tools in the situated achievement of a production order that is responsive to the contingencies of print production, such as keeping all machines running and hitting agreed deadlines. Certainly, scheduling boards are used to work out answers to the calculations involved in this, and the level of automation offered by current products may well be of value.

However, it is *not* the manipulation of data that is consequential to those involved in using the scheduling board to plan production. On the surface it might appear so, and current systems inevitably assume that it is. Current systems are really little more than databases and methods of presenting the data. What is a consequential feature for those that use scheduling boards is not the ability to grind through production data. Rather, it is that relevant personnel can use them to fix a picture of the production status at a glance. Should a printing press go down, as they often do, a quick glance at the scheduling board reveals in a rough and ready way—but a way that is

29 Button, G and Sharrock, W W 'The production of order and the order of production' in J Hughes, W Prinz, T Rodden and K Schmidt (eds) *Proceedings of the Fifth European Conference on Computer Supported Co-operative Work* Kluwer, Dordrecht (1997) pp 1–16

perfectly adequate for participants' purposes—what will have to be contended with. The scheduling board, as well as other objects, can thus be used as ways of making the order of the work visible at-a-glance. In this respect, it is not inconsequential for its use that the visually available features of the medium are emphasised. Scheduling boards tend to be large, colourful, often involving paper. The material composition of the object is the consequential characteristic of its utility in working a production calculus, not its ability to run calculations. In this respect systems that emphasise the numerical capabilities of scheduling boards would, and do, miss essential requirements. It may be that systems that support the viewing of forward loading boards may be of more value than systems that automate them. Scenic fieldwork may be unable to reveal this requirement.

Overall, I am not arguing that fieldwork can only be the preserve of the sociologist and that designers and CSCW practitioners should keep away. It seems most perplexing that it could ever have been the case that systems designers or software engineers could ever have thought it unnecessary to observe first hand the work they were attempting to support. It is sensible to urge designers to take some time to go 'into the field'. However, as I have argued, this should involve the caveat of going into the field under particular analytic auspices. I would not, however, argue that designers should first train as ethnomethodologists. Rather, a division of labour between the designer and the work analyst is in order. What this can amount to is a matter to be worked out in practice for particular development undertakings. There seems little point in being prescriptive, and we should really consider this division of labour as a framework within which design and work studies can collaborate. Thus I am offering the suggestion that ethnography can be trailed into the world of design in a harder fashion than our enthusiasm currently permits. It is not classical ethnography that is the remedy here: indeed I am sceptical of classical ethnography in and of itself. Much more relevant is the use of fieldwork, but even here I believe we need to be more rigorous, demanding not merely scenic depictions of settings and doings, but analytic explications of how activities are done and ordered. Analytic explications must reference what Garfinkel terms *members' knowledge*: what people have to know, and how that knowledge is deployed in the ordering and organisation of their work. I believe it is the explication of members' knowledge for the purposes of design that is the point of collaboration between design and sociology in CSCW, certainly not ethnography, and not merely fieldwork.

5 Members' knowledge
How do these issues translate to the concerns articulated in other papers in this special issue, where the purpose of working with engineers is to

contribute to the organisation of their work? In part, the answer turns on the relationship between the knowledge that people have of their own activities, and the descriptions that the fieldworker may give of these activities. The arguments presented within ethnomethodology suggest that it is simply not possible to go beyond members' knowledge, the knowledge that persons have of their own doings. We have seen how thinking that it is possible to do so caused problems within classical ethnography.

The argument that investigations of social life are constrained by members' knowledge would be unacceptable to many branches of sociology. However, it is an argument that fits the collaborative relationship between studies of work and the design of systems. In many ways, the hallmark of a strong study of work would be that it contains nothing new for those who do the work. This is not to say that, as a feature of doing their work, they naturally provide a commentary on it. They may never have thought about producing a systematic account of what they do, and the actual explication of the work may be of little interest to those who do it, and only of interest to the sociologist. However, for the sociologist's account of the work to be systematic, it needs to be recognisable to those who do the work. For the purposes of designing systems to support their work, such an explication may be exemplary. However, from the point of view of those who do the work itself, the explications are irrelevant.

So, what does this imply for the engineering community that may be the subject, rather than the recipient, of ethnographic studies of design work? There is a strong and a weak version of what can be said. The strong version is the following: it simply is not possible to tell engineers anything about how engineering is done and organised that they do not already know. After all is said and done, it is their knowledge, its organisation, and the practices of its deployment, that is the object of study. Consequently, even if welcomed by the engineers themselves, studies of their work will not in practice count in the organisation of engineering work. While such accounts may provide fascinating insights for sociologists, it cannot tell the engineers more about engineering and the organisation of engineering than they already know, for engineering is simply what they do, and they already know all about what they do. An argument may be made, though, that an outsider's point of view can shed light into the dark corners of engineering work. But as it would be nothing more than a catalyst, any point of view would do, and no special merit could be claimed for ethnography or ethnographic fieldwork. A further argument might be that an ethnographic study can act as a mirror, allowing engineers to reflect on their own practices. If so, then again nothing particular could be claimed for ethnographic fieldwork, since *anything* that encouraged reflection would be in order.

We might, however, say that ethnographic fieldwork encourages engineers to reflect *in a certain way*, and this is the weak version of the answer. Laying out the logic of the relationship between various work activities may occasion an opportunity for them to consider how their work endeavours are supported, organisationally and technologically. For example, most industrial engineering companies are subject to some sort of product delivery process (PDP). By studying how this process is applied in practice, the numerous techniques that engineers have to use to make procedures fit the varying contingencies and circumstances they face—but which are not covered by formal PDP descriptions—can be made visible. This is not news to engineers, but making it explicit through studying their work might allow managers to articulate product delivery processes more effectively. For example, if engineers need constantly to refer to the requirements for a product, then those requirements are better presented online than physically located in a distant paper archive. By presenting them online, engineers can make consulting the requirements part and parcel of their work, rather than an act which forces them to break off from their work. Of course, another way in which studies of engineering work may be of value to engineers is to treat the engineering community in the same way as do studies of work in the CSCW field; bringing an understanding of the engineering work into the design and development of engineering support systems. But, this is clearly a matter for further consideration.