Chapter 6
The Missing What of Ethnographic Studies

Abstract In this chapter we look at how various kinds of ethnographic studies done within social science and systems design have tended to generate 'scenic descriptions' of action and interaction. Scenic description orients us to grossly observable features of action and interaction without examining the 'just how' of its doing, i.e., just how what was done was done so as to pull it off as the thing that grossly observable is. This concern with the *absence* of the lived orderliness of action and interaction is framed in terms of the discussion of 'work' as it is understood within ethnomethodological studies. The critical thing to note here is that 'work' is not restricted to what goes on in the workplace but is a *generic feature* of interaction. It draws attention to the fact that action and interaction, wherever it takes place, is always an *achievement*. The work of interaction is all too often missed in ethnographic studies, resulting in descriptions of human activity that have the character of 'X did this, and Y did that', without lifting the lid on *how* it is done as an organised interactional accomplishment. The problem here is that if ethnography resides at a scenic level of description, detailing merely observed behaviour that anyone can see, it can and will *misdirect* designers' understanding of the foundational relationship between ethnography and systems design and what designers can hope to take away from ethnographic studies.

6.1 Scenic Description

We have been examining the idea of ethnography as it developed in anthropology and have attempted to track through some of the consequences of rendering understandings of culture and society through theoretical interpretation. In doing so we have attempted to explain our concerns about the return to traditional anthropological practices introduced by 'new' approaches to ethnography in systems design, and the confusions about the nature of the social and its investigation that accompany them, which have been problematic within the social sciences since their inception. However, another closely associated social science – sociology – has also developed a strong ethnographic character, so much so that the boundaries between anthropology and sociology have become increasingly blurred and mainstream ethnography has come to reflect the innate tendencies of both disciplines. Like anthropology,

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ethnography in sociology has been mainly associated with disciplinary interest in culture and social structure. In turning to 'new' approaches to ethnography from the social sciences, we are also worried then about the way in which many ethnographies conducted for the purposes of systems design incorporate problematic characteristics associated with ethnography in sociology, particularly the production of *scenic* in distinction to *analytic* descriptions of human action (Button 2000).

While the call for 'new' ethnography is, in part, articulated by a break with empirical interest in the social that has been associated with design's engagement with ethnography to date (Dourish 2006), other approaches to ethnography hold on to the idea of empirical investigation but view this as involving matters other than those associated with previous 'studies of work'. Before we turn to these it is worth reminding the reader that we are not trying to legislate as to what aspects of the social systems design could or should be interested in, or how it should be interested in it. Rather, we are concerned to make visible what it is that systems design is buying into should it take up alternate approaches to ethnography. Our concerns here can be elaborated by considering the kinds of problems that have been encountered by sociologists in the study of work and occupations. As noted above, we have so far been examining how traditional anthropological approaches to ethnography are being tracked into design and the problems this raises. We will proceed in the same way with respect to our examination of more empirically-based approaches, first examining problems inherent in early empirical studies of work in sociology, and then finding those problems in ethnographies done for design purposes, both in work and non-work settings or under the auspices of work or non-work interests.

The history of sociology's interest in work has been crafted in the investigations of many and varied people and perspectives over a great many years. The different theoretical auspices they have laboured under and the different characterisations of the nature of work they have produced can be invoked for the purposes of presenting catalogues of, and introductions to, the sociology of work. Despite profound differences in perspective, there is sufficient similarity in the sociological orientation to the study of work to allow the following remarks made by Anslem Strauss and his colleagues (1985) to ring true.

> … remarkably little writing in the sociology of work begins with the work itself (except descriptively, not analytically) but focuses on the division of labour, on work roles, role relationships, careers, and the like. A concerted *analytic examination of work itself* ought to provide a needed corrective to more traditional approaches, which, however effective, still leave important issues untouched or unresolved.

An examination of Keith Grint's popular introduction to the sociology of work (Grint 1991) would seem to bear out Strauss et al.'s remarks. If the sociology of work were concerned with the 'work itself' then we might find in an introduction such as Grint's descriptions of work activity, how work is done, what distinguishes some work from other work, or what makes some work similar to other work in terms of how it is done, and the like. However, instead of introducing the sociology of work in these *analytic* terms, Grint introduces it in *definitional* terms by considering both sociological and cultural characterisations of work. Grint highlights

problems in the definition of work. He points to the way in which work is traditionally defined in terms of paid employment within western industrial societies, and argues that such definitions can make some work invisible, such as the domestic labour of 'housework' which although unpaid is nevertheless work. Grint goes on to consider the classical theories of work presented by Marx, Weber and Durkheim, and the contemporary theories of post-modernism and actor-network theory. His final move is to then use work as a structure from which to view social stratification, examining phenomena such as conflict, class, gender, patriarchy, resistance, race, ethnicity, markets, and politics.

If we wanted a synoptic view of work from a sociological point of view then it is clear that work is treated as an *object for theoretical definition* and a vehicle for apprehending the varied structural forces postulated by sociology, as opposed to a *situated practical undertaking* that is available for analytic study. These remarks are not particularly intended as a criticism of Grint. Our point is that he does indeed do a commendable job of introducing the sociology of work, but that in so doing he provides a characterisation of what the sociology of work amounts to that bears out Strauss's remarks. The sociology of work is not so much about work itself but about the social and organisational conditions under which work, whatever that might be, is conducted and the social characteristics that may be attributed to cohorts who conduct it.

It is also the case that the particular conditions and the particular characteristics that any particular sociological study of work elaborates derive from the particular sociological theory under whose auspices the study is conducted. Thus, for example, Braverman's (1974) depiction of the dehumanising conditions of work results from his confronting Taylorism with a Marxist examination of monopoly capitalism, while Firestone's (1970) depiction of patriarchal subordination implicit in domestic labour derives from a radical feminist theory of cultural reproduction. The sociology of work would thus seems preoccupied with '*scenic*' features of work – i.e., observable and reportable features of the social world that are drawn upon to frame and set the stage upon which work is conducted. Gender distribution is, for example, a scenic feature of work. No doubt such a statement will raise some hackles. However, take the following example before crucifying us. Statistics on the number of women engineers may be used as evidence for a number of social inferences such as the way in which women are viewed in society, or the way in which particular types of work are viewed in society, or the challenges facing women in what are traditionally male roles, or the organisation of the education system, etc. All of these matters might be socially interesting and important in their own right, however, interesting as such inferences may be, they do not inform us as to what it is to *do*, for example, engineering work, whether it is done by men or by women. The doing of work is taken for granted and ignored, and it is in that respect that the personal attributes of the engineer, that they are a man, a woman, tall, short, black or white, heterosexual or GLBTI, is a scenic feature of the work of engineering.

One way in which Strauss's comments can be read – a 'lite' version if you will – is to read them not so much as a critique of scenic description but that the very *doing*

of work could and should be a proper sociological topic. However, the difference between the sociology of work as it is depicted in (for example) Grint's rendition, and the sociology of work as gestured at by Strauss is, we believe, about more than extending the remit of the sociology of work. If it were just about extending the remit then it might be possible, on the lite reading at least, for sociologists to shrug their shoulders and permit the realisation of Strauss's ambitions within the arena of sociology's traditional concerns. It would be possible for the sociology of work to continue to spin out its definitions and theoretical formulations and to also take up Strauss's invitation to study the actual composition of work. On this lite reading, Strauss is not challenging the foundations of the sociology of work by putting the very ways in which it formulates an understanding of its subject matter into question. He is merely proposing a further domain of interest: the doing of work itself. However, a stronger reading of Strauss is possible if we consider the implications of scenic description – that the actual organised conduct of work is *absent* and *will always be absent* from such accounts. On this stronger reading, Strauss's comments may indeed be read as criticising the sociology of work, not merely providing it with further investigatory opportunities.

Though only gestured at in Grint's introduction, there is a body of investigations in the sociology of work, and one with which Strauss is strongly associated, that *has* attempted to address what it is that people actually *do*. This work originated in the 1920s and is primarily associated with the Chicago School of Sociology. To name the anthropologists and sociologists associated with the Chicago School is to make a roll call of some of the most influential researchers in the social sciences: Anderson, Burgess, Frazier, Hughes, Mead, McKenzie, Park, Sutherland, Thomas, Wirth, and Znaniecki, to name but a few. Their research marked a step change in ethnographic interest, shifting it from something preoccupied with non-western societies and cultures to focus on life much closer to home. The Chicago School took the city as its subject matter, and through numerous extensive and detailed ethnographic examinations of urban life subjected the city to an order of examination previously reserved for 'other' societies and cultures. Indeed, early work reflected previous anthropological interests in the slums of Mexico, for example, and resulted in a host of pioneering ethnographic studies of life in the western industrial ghetto (see, for example, Thrasher 1927; Wirth 1928; Zorbaugh 1929).

The Chicago School gave rise to whole new branches of social science, developing *urban sociology* for example, which examines how major themes of sociology such as deviancy, power, class, status, race, gender and the like are played out in the city, and *human ecology*, which has now become an interdisciplinary concern focusing on the relationship between human behaviour and the built environment. Of particular relevance to our concerns in this book is the development of *symbolic interactionism*, a term coined by one of George Herbert Mead's PhD students, Herbert Blumer, in 1937 (Prus 1996) to reflect growing disciplinary interest in the social order as the ongoing accomplishment of human interaction (Blumer 1969). Spurred on by the pioneering efforts of Everrett C. Hughes, who saw the ordering of society as "very much a matter of man's (sic) relation to the world of work" and who fostered the study of "the orderly course of man's work life", symbolic

interactionists built up a wide array of studies of a heterogeneous range of occupations (Hughes 1958). As Shaffir and Pawluch (2003) put it,

> Hughes sent his students into the city to study the janitor, the cab driver, the doctor, the union official, the factory worker, the musician, and others. Such studies helped lay the groundwork for the qualitative tradition in sociology and furthered our understanding of how workers organised their work and saw themselves.

Despite the achievements of the symbolic interactionists it is a startling feature of the sociology of work that the immense amount of empirical investigations they conducted seem to be largely ignored in reviews and introductions to the subject, let alone being an influence upon contemporary concerns in the sociological study of work. Only one of these studies, Donald Roy's *Banana Time* (1959) makes it into Grint's introduction, for example. Yet, despite the fact that their existence is mainly ignored in contemporary sociology, the Chicago School studies were the first serious attempt to engage in the *analysis of work itself* and to do so by investigating it 'from the inside', i.e., from point of view of the actors and the interaction actually involved in *doing* it. Donald Roy epitomises the point. He didn't just 'hang out' with workers in a garment factory in New York, conducting informal interviews and observations, he actually *did* the job in order to understand, as he puts it himself,

> … how one group of machine operators kept from 'going nuts' in a situation of monotonous work activity … (ibid.)

The monotonous activity in question took place in the 'clicking room' of a New York garment factory, where a handful of operators hammered out small pieces of material for garment assembly from sheets with dies in mechanical presses – simple, repetitive work as Roy describes it, conducted in isolation from the other employees in the factory. Roy's introduction to the job consisted of "an all-time minimum of training" – he was given a brief demonstration and told to keep his hands clear of the hammer and, after a similarly short period of practise, he was put to work.

So how did the machine operators stop themselves from going nuts in such a monotonous and isolated work situation? Roy first of all elaborates how he made clicking into a game to help pass the time – developing a "continuous sequence of short-range production goals with achievement rewards in the form of activity change" – but this is not how his colleagues coped with a nullifying situation of work day-in-and-day-out. Rather, Roy found that his co-workers, George, Ike and Sammy, had developed an "informal structure" of workplace interaction to make the monotonously long working day "liveable". This informal structure was manifest in what Roy called "times" and "themes", which shaped interaction. The notion of times refers to the *temporal punctuation* of clicking work, not only through the exchange of sheets and moving of boxes, or lavatory and lunch breaks, but through other brief interruptions as well. These interruptions occurred almost hourly. They included the consumption of food and drink outside of the official lunch break – which the workers referred to as coffee time, peach time, banana time, fish time, coke time, etc. – and other kinds of interruption that Roy called window time, pickup time, and quitting time. Sitting alongside and weaving through such distinct

temporal punctuations were themes or *verbal interplays*, which became "standardised in their repetition" – serious themes, kidding themes, chatter themes, the poom poom theme, the professor theme, etc. Thus, through the daily round of times and themes the work of clicking was sustained and made into a "satisfying" job of work by the machine operators through an informal structure of social activity and horseplay that was "in constant flow".

That Roy's study is still mentioned in introductory texts to the sociology of work goes some way to mark the impact of his work. *Banana Time* raised a range of theoretical considerations of relevance to sociological inquiry into the behaviour of small groups, particularly in factories. Roy's study suggests that, counter to rational theories of action, such groups are not generically 'instrumental' in nature but that their ecological situation drives local socio-cultural systems marked by 'consumatory' interaction – i.e., interaction done freely for the pleasure of it, rather than done to achieve some specific instrumental goal. Furthermore, in the course of working together the group's members produce distinctive sub-cultures having their own distinctive social structures that provide for the 'equilibrium' of the group. This does not increase productivity, but it does bring job satisfaction or "at least job endurance" to work situations that are largely bereft of creative experience.

Roy's studies are but an example of symbolic interactionist studies of work, which stand in stark contrast to those of the mainstream sociology of work in terms of their emphasis on the interactional context and situation, and in examining work not so much in terms of the social characteristics of those involved but in emphasising an examination of the *interactional milieu*. In mainstream studies of work in sociology the actual work that is done seems to mysteriously *vanish*. It is taken for granted and so disappears from view. Take, for example, a very influential book from the 1950s *Coal is Our Life* (Dennis 1956), which provided a penetrating description of a way of life now passed of close-knit communities working together and supporting each other which were bound together by 'the pit'. Although evocative of a mining culture, a culture dominated by the fact that men worked "down't pit", the actual doing of the hewing of coal, an actual description of the very wellspring of community life – the time spent underground with machinery extracting coal and bringing it to the surface, the actual interactional accomplishment of that job of work – is *assumed* and *in its place* is put an account of the ways in which economic forces structure social relations.

Similarly, the recognition by feminist sociology that 'housework' is unrecognised work but nonetheless work for that, unpaid and undervalued and constituted in a patriarchal social structure. However, again, the actual work of 'housework' is assumed and taken for granted. We might say that housework involves ironing, for example, but how is ironing done? How is a stack of clothes in a laundry basket ironed and moved into its storage spaces ready to wear? Are decisions made about what to iron and what not to iron, and if so how are they made? Is the first thing on the top of the pile ironed, or is the pile ordered into categories of ironing? How are judgements made about the temperature of the iron, are the temperature dials to be trusted, or are past experiences of ironing triumphs and mishaps taken into account? These and similar matters may appear to be trivial and of little interest to the

sociologist who wants to talk about grand social structural matters, such as patriarchy or the economic drivers of social relationships, but to those who are doing the ironing as an everyday concern they are matters that enter into the very *doing* of the ironing which can, as the tradition of symbolic interactionism has demonstrated, be characterised in different ways for sociological purposes: in ways that seek to elaborate the *interactional ordering* of work.

However, it is with respect to the interactional ordering *of work* that interactionist studies are themselves problematic. While mainstream sociologists have simply ignored it, and continue to ignore it, symbolic interactionist studies, despite the promise they seem to hold, have not actually got to grips with it themselves. *Where*, for example, *is* the interactional order of work in Roy's study of clickers in the New York garment factory? We can read in Roy's account of the work that it involves the monotonous, repetitive, order 'click – move die', that new sheets to be clicked have to substituted for old ones that have been clicked by someone (who, and what their job is, we do not know) and that boxes of finished work have to be moved and empty ones put in their place, and we can read too that there is a clicker room leadman who coordinates daily with the superintendent and communicates workloads to the clickermen, but how are any of these things actually ordered in interaction? How are the monotonous, repetitive actions 'click – move die' actually done: what do 'click' or 'move die' actually consist of *as* repetitive actions? What other actions are implicated in the achievement of clicking work? How do the sheets get into and out of position for 'click – move die' to take place? What happens when there are no sheets left to perform the actions on? How are sheets made available to the clickers to work in a timely fashion? How is the movement of boxes paced to ensure the smooth flow of work? How do the clickers manage and coordinate the day's workload? How do they know if they are on target, or if they need to slow down or speed up, etc.?

Roy provides a fascinating study of how people stop themselves going nuts in banal work situations, elaborating the social interactions that enable them to endure the working day, but he does *not* elaborate the interactional work and interactional order of *doing the job*. While symbolic interactionism has made interaction into a key analytic topic, it is the interaction 'going on around the work' that is of analytic interest, *and* what can be made of certain aspects of it for mainstream sociological consideration: the nature of small group formation in the workplace, the dynamics of group interaction, the development of sub-cultures and social structures, etc. – considerations that can, as Roy (1959) puts it, "be *abstracted* from the total existential flow of observable doings and sayings" (our emphasis).

Thus, and despite the turn to interaction, interest in the work of a setting and its accomplished order is subordinate to theoretical interests in the sociology of work. Little wonder, then, that interactionist studies are strangely mute about the actual interactional accomplishment *of the work* they are concerned with. In interactionist studies of work, although we are given descriptions of what people do when working, in the sense that we are given shallow characterisations of *what* machine operators do for example, we still do not know *how* those things are done, and in *missing* that order of detail much of the organisation of work itself remains untouched.

In place of the orderliness of work, we are offered the orderliness of social interactions surrounding the work and sociological abstractions on the nature of work and society. So although interactionist studies focus on the importance of interaction and the interactional milieu, Strauss's hoped for *analytic* emphasis is not realised. We are returned instead to scenic descriptions – to anything in the world *but* the actual doing of work as an interactionally ordered and accomplished achievement.

6.2 The Missing Interactional What

The issue of the 'missing interactional what' of interactionist studies was highlighted by Garfinkel in his consideration of a study of jazz musicians done by another of Hughes' protégés and a leading figure in the Chicago School, Howard Becker. Just as Roy's study of clicking work in the New York garment factory was conducted through participant observation, so too were Becker's studies of the playing of jazz music in Chicago dance halls.[1] Just as Roy's studies elaborated the interaction that surrounds the work of clicking, so too Becker's studies elaborated the interaction that surrounds the playing of jazz. Becker's studies showed that what was at the time considered to be a somewhat deviant culture, a view reinforced by jazz musician's express rejection of social norms, was nevertheless a highly organised occupation ordered through a distinctive set of occupational values embodied in the "colleague code". His studies elaborated how 'the code' shaped the career structure of the jazz musician, the fraternal organisation of work it gave rise to, the pressures of work and playing to the audience, the dilemma of commercialism versus prestige, and the impact of family on the musician's life and the conflict it generates (Becker 1951).

Becker's ground-breaking studies shed light on the hitherto unrecognised social *and* moral order of so-called 'deviant' cultures (Becker 1963). As illuminating as they are, they spurred Harold Garfinkel to make the following observations.

> Harvey Sacks speaks of a curiosity in the work and history of the social sciences: the 'missing interactional what' in lay and professional studies … For convenience we shall speak interchangeably of the 'missing what', 'missed what', or 'missed orderliness'. David Sudnow epitomises the issue as follows. On the basis of his studies of the gestural organisation of ensemble musical play (Sudnow 1978) he speaks of the 'Howard Becker phenomenon' in sociologists' studies of jazz. (Garfinkel unpublished manuscript)

The Howard Becker phenomenon is comprised of two parts. First, Sudnow observed of Becker's work that we come to understand where jazz musicians work, what they earn, who they work with and such like. However, and this is the second part, *how*, with the particular assembly of people to hand, within the particular circumstances in which they are playing, they pull off making music together is not available in Becker's account.

[1] Becker was a jazz musician, had been since the age of 15, and he complemented his own observations with informal interviews of other jazz musicians on the Chicago circuit.

> A curiosity of the reportage, Sudnow points out, is that Becker's articles speak of musicians' work and do so by omitting entirely and exactly the practices that for those engaged in them makes of what they are doing, makes up the recognisably just so, just what, just this going on: making music. Not music of a certain type, but this music; not music accomplished via behaviours in motivated compliance with valued and normative practices, (except perhaps in the work's own established terms) but music done as, and consisting of certain, local, witnessed, practically objective and practically observed materially particular musicians' practices. *That* is omitted from Becker's account, it cannot be recovered from the account … it is completely and essentially missing … Sudnow points out that even though it was written by a jazz musician, it is an *appreciation* of the work of jazz musicians. By an appreciation is meant that no reading that could be made of that published article will provide the *what* … … … … we wish to emphasize as a positive feature of the *missing what* the absence of a descriptive literature. The absence of such a literature is not restricted to the work of jazz musicians. A descriptive literature on occupational praxis is absent to the entire field of the sociology of occupations. It is nowhere to be found. (ibid.)

Today the situation has still not changed with respect to the sociology of jazz. In 2009, along with Robert Faulkner, another sociologist and jazz musician, Howard Becker published *Do You Know …? The Jazz Repertoire in Action*. On reading the table of contents it might be expected that the missing interactional what has been addressed: chapter one is called 'how musicians make music together', two 'repertoire as activity', three 'learning songs and building an individual repertoire', four 'the skills you need …', and so on. However, on actually reading the text it is clear that the interactional what of making music together is still *missing* (see Faulkner and Becker 2009).

The interactional what *of work* is still missing in ethnographic studies more generally. Not only in mainstream ethnographies of work, but also in symbolic interactionist studies and a great many ethnographic studies conducted for the purposes of systems design as well. The latter may well produce findings of interest, but like the studies of the symbolic interactionists they nevertheless treat interaction at the scenic level. The result is that an ethnographic study may at first glance appear to be taking on an examination *of work itself* in furnishing first-hand 'insider' accounts of interaction, but on closer inspection it transpires that the work is missing, supplanted by accounts of the interaction that surrounds work and what can be abstracted from it for the purposes of systems design. In sociology and design alike, Strauss et al.'s dismay at the lack of attention paid to work itself has largely gone unheeded. Nonetheless, if we view interactionist studies as an attempt to implement an interest in work itself, we still have to conclude that even when it seems to actually pay explicit attention to work it is more concerned to produce descriptions, as the sociologist Wes Sharrock puts it, of what people are unwittingly doing when they are wittingly working.

The failure to get to grips with the interactional what of work is reflected in recent calls for systems design to turn to the 'European field study tradition' (Bannon et al. 2011), as exemplified by studies of work undertaken in Germany and France. While the authors recognise the contribution ethnomethodology has made to design's understanding of work through its interactional studies of the what of work itself, they go on to say that design can learn so much more than is provided for by

such studies, particularly about the ergonomics of work activity and the conditions of its undertaking. We find it ironic that this should be viewed as a step forward. Bannon, Schmidt and Wagner are returning us to traditional ways of apprehending work in sociology, for when the studies that we are exhorted to attend to are examined we find two very familiar sociological gambits at play. The first is that, again, the actual work as it is done by the parties to it is not available; like the symbolic interactionist studies before them, the what of work itself is missing. The second is that in place of the work itself we encounter, particularly in the arguments recommending these studies, not descriptions of work but *sociological definitions* of work. That is to say work becomes a matter not so much to be apprehended through the agency of its doing, but through the social scientist who first tells us what work is before we actually encounter it. These definitions do not so much provide a cultural lens, as we have seen in the context of ubiquitous computing in Chap. 4, rather they are prescriptions as to what work is and how it is 'shaped'. The return to the European traditions of field study, defined by Francophone ergonomics and German industrial sociology, is a return to the wastelands of sociological definitions and prescriptions, and leaves us with the traditional sociological business of at best producing scenic descriptions.

Scenic description orients us to anything in the world but the work that people do: the social attributes that frame it, the social interactions that surround it, the conditions that shape it, etc. At best, scenic descriptions of the kind symbolic interactionists provide put us onto *what* the work consists of, but leave the practices involved in actually pulling off the work untouched. Such studies are commonplace and fail, in Strauss's terms, to *analyse the ways in which work is done*. Now a response to this, from someone involved in design at least, might well be: "Who cares about work anyway? Systems design has moved on. We're into leisure, and play, and all kinds of new and interesting stuff. This is just old hat." Such a response would, however, fail to recognise a number of things. First, that the design of workplace systems does *still* occupy industrial design and research. Second, that the missing interactional what is relevant to ethnographic studies of all colours, shades and hues, even ethnographies in academic design and research, no matter the domain of inquiry or whatever new topics absorb the field. It was this very issue that we sought to convey in a CHI paper called *Ethnography Considered Harmful* (Crabtree et al. 2009). Our argument was, and is, that the turn to new domains and topics of interest does not mean that 'old' approaches to ethnography that focused on work should be so readily supplanted with 'new' ones as design continues to move out of the workplace or, at least, that systems designers should do so *with caution*, in cognisance of what it is they are buying into and casting off.

One of the key things we tried to get across in that paper was that the notion of 'work' in ethnomethodological studies is not restricted to what goes on in the workplace but is *generic* in that it draws attention to the fact that action and interaction, wherever it takes place, and whether it involves payment or not, is always an *achievement*. The idea of work in ethnomethodology recognises that people are involved in *doing* some activity, that they are involved in *making* it happen and *bringing* it about. The complaint about the missing interactional what is a complaint

that the courses of action and interaction whereby clicking work, or making music together, or *any* other human activity is actually done are missing from ethnographic accounts, and indeed the accounts of social science as a whole. With it go the procedures, methods, or practices that members accountably employ to organise the activities they are engaged in (making music together, etc.). Two of us have made separate attempts to clarify this (Button 2000, 2012; Crabtree et al. 2005), however, the generic idea of work articulated in ethnomethodological studies continues to cause trouble and Schmidt (2010) goes some way to point this out:

> … to argue that just because we [can] use the word 'work' … for all sorts of phenomena, then all these phenomena are *of the same kind* and can be studied as more or less the same phenomenon … is the classical nominalist fallacy.

Taking his inspiration from Gilbert Ryle, Schmidt lays out an argument to demonstrate that the concept of work is a 'polymorphous' concept.

> If asked 'What does working consist of?' we should quickly object that there was no general answer … … … There is nothing which must be going on in one piece of work which need be going on in another. Nothing answers to the general description 'what work consists of'. (Ryle 1971)

Schmidt then proceeds, somewhat strangely given Ryle's comments, to make an argument about 'finite provinces of meaning' (Schutz 1962) – different kinds of social action (work, play, leisure, etc.) – to distinguish and justify a normative conception of work. His argument is that work as articulated in ethnomethodological studies in the way in which we have described it above is a derivative use, and that ordinarily speaking we mean by it the idea embodied in a 'job of work', which for Schmidt is its primary reference. However, it is important to appreciate the *irrelevance* of any 'primary sense' of work, in distinction to a generic technical conception of work, to ethnographic studies of social action. Nevertheless, Schmidt argues that the word 'work' *does* have a primary sense, providing a number of ordinary examples of its use to demonstrate this: how people complain that meetings get in the way of their work; or they are interrupted in their work by telephone calls; or enough talk, lets get to work. However, we can equally well offer examples of the ordinary and perfectly understandable use of the word outside of workplace contexts: it takes work to get out of bed in the morning; it's fun but hard work; I'm really having to work at not getting angry with him, etc. These are perfectly intelligible uses of the word 'work' and it is only by *fiat* that we would call some uses primary and other's derivative. It is the action done through the use of the word that counts, not a definition applied to it by a social scientist. But then Schmidt, as we have seen in his call for design to turn to the European field study tradition, sets much store by imposing particular meanings on words, however they are ordinarily understood and used.

Of course what Schmidt is really worried about is that the ubiquitous use of the term 'work' undermines the field of Computer Supported Cooperative Work, which for him is 'naturally' about what goes on in workplaces. However, there is really no concern here. In clarifying the way in which work has been used in ethnomethodology, an alternative conception of work is not being offered for CSCW or any other

discipline: we are drawing on and making use of ordinary language not to specify what work is or what it consists of but to specify a *study policy* and to provide an *instruction* to fieldworkers and others involved in the design of interactive systems: attend to the missing interactional what of lay and professional studies, elaborate the interactional work that human activities consists of and get done through. Thus, we can as equally well elaborate the interactional work involved in doing and coordinating paid labour as we can elaborate the interactional work involved in doing and coordinating a host of unpaid activities. The annals of CSCW stand testimony to that, and CSCW has not disintegrated as a result of this.

Ironically, the problem that others have had with the ethnomethodological conception of work, and one that motivates calls for 'new' approaches to ethnography in systems design in particular, is the polar opposite of Schmidt's – champions of the 'new' think that we are *only* interested in, and only fit to study, what goes on in workplaces (see Crabtree et al. 2009). This is an unfortunate misunderstanding that is, perhaps, a consequence, as we mentioned in Chap. 1, of systems design's history and ethnomethodology's peculiar nomenclature. Historically, systems design's engagement with ethnography was motivated by a concern to develop technology for the workplace. This historical contingency gave rise to various phrases to describe ethnographic studies: studies of work, work practice studies and even workplace studies. It is easy to see how, on the face of it, ethnomethodologically-informed ethnography could be about understanding what goes on in workplaces, and that as design began to move out of the workplace 'new' or alternative approaches would obviously be required. Ethnomethodology's terminology does nothing to help matters here. The terms 'studies of work' and 'work practice studies' are of ethnomethodology's making. They were coined in a sociological context to reflect the disciplinary interests that ethnomethodology has in the production of social order.

In this context, the notions of studies of work and work practice position ethnomethodology's interests in the social order and how this contrasts with mainstream sociological treatments of the phenomenon. Thus, in contrast to practices of sociological theorising, which seek to provide interpretations and explanations of action with reference to the structural forces in society that play upon it and cause or at least shape it, the notion of studies of work and work practice posit an alternative viewpoint. Instead action and interaction is seen to be 'worked on' and 'worked up' by those involved in doing it; action and interaction is, as Garfinkel (1996) puts it, achieved. The invocation of studies of work and work practice orients us to the *achievement of social order* in and through action and interaction then, and thus defines a distinctive disciplinary interest in the production of social order. Concomitant to this is the disciplinary commitment to the idea that there is 'order at all points' (Sacks 1984), which is to say that anything and everything that people do – any and all courses of action and interaction – may be examined to find the ways in which they are socially ordered in their actual doing. Studies of work and work practice orient us as ethnomethodologists to the achieved character of action and interaction, then, and to the ways in which action and interaction is socially ordered *in the course of* its accomplishment (Button and Harper 1996; Crabtree et al. 2012).

Thus ethnomethodology has attempted to make visible the fact that people are not puppets animated by the omni-relevant socio-cultural structures and forces delineated in mainstream sociology's scenic descriptions of action, including those that Schmidt would orient us to. Rather, they are active participants in the construction of the action and interaction they find themselves engaged in, whatever it is and wherever they are, and that, consequently, action and interaction are always and entirely achieved, 'worked at' and 'worked up' matters, put together or assembled in orderly ways. Ethnomethodology has thus referred to the 'work' that people are engaged in, in the doing, indeed *in the design*, of their actions and interactions. This holds true whatever the action or interaction involved, whether it is work in workplaces, or the work involved in doing leisure pursuits, or domestic life, etc. The fact that Garfinkel fostered a program of studies of work that involved detailed examinations of different occupations has perhaps confused matters, making it seem that ethnomethodology only applies to the study of what goes on in workplaces. It does not.

It is a little ironic that ethnomethodology has been characterised as only interested in matters to do with occupations and the workplace, though perhaps indicative of the fact that those doing the characterisation have not properly engaged with that which they criticise. Ethnomethodological studies of work have spanned a broad range of settings and activities that its detractors might well be interested in, including so-called 'ludic pursuits' (Gaver 2001) such as playing the piano (Sudnow 1978), or video games (Sudnow 1983), or hop scotch (Goodwin and Goodwin 2000), even playing with a dog (Goode 2007), doing origami (Livingston 2008), or being drunk (MacAndrew and Edgerton 1969). Indeed two of the current authors, Tolmie and Rouncefield, recently edited a publication called *Ethnomethodology at Play*, which includes studies of cooking, bird identification, fishing, yachting, using music software as a hobby, having a day out in the country, rock-climbing, running, playing music together, line dancing and having a drink in the local pub (Tolmie and Rouncefield 2013). All appalling unworthy topics for mainstream social science with its eyes on the lofty theoretical and methodological issues of the day, such as globalisation and mobility in a massively networked world, which drive the call for 'new' ethnographies because ethnomethodology, with its distinctive focus on work, allegedly cannot handle them.

It is certainly the case that the bulk of ethnomethodological studies done in design to date have focused upon the workplace, but then that is because historically this has been where systems design's interest has lain. It should be appreciated by now, however, that ethnomethodology's interest in work is not restricted to what goes on in workplaces. Even a cursory look at Garfinkel's programmatic text *Ethnomethodological Studies of Work* (Garfinkel 1986) should suffice to make the point clear for here can be found, sitting alongside studies of occupations, studies of kung-fu and the occult, and the corpus of ethnomethodological studies of work further reinforces the point that studies of work are *not* restricted to the study of what goes on in the workplace.

In this respect, the call for 'new' types of ethnography in systems design misses the point about and significance of studies of work and work practice. Ethnomethodology is, no doubt, in part responsible for the fact that the point *can* be

missed, but ethnography should not be tied to the object of study in the sense that one type of ethnography is suitable for some purposes, while another is suitable for others; this is quite a meaningless idea. Ethnography involves the study of social and cultural arrangements inside of their workings, *whatever* the social and cultural arrangements being studied may be. The question is, how is this study to be undertaken?

In asking this question we have throughout this book been raising problematic issues to do with 'new' approaches to ethnography imported from the social sciences in general and their problematic character for systems design. Thus we have questioned the very idea that studies of human action can proceed with any adequacy on the basis of sociological theorising and scenic description, as opposed to describing the organisation of action and interaction in terms of what is actually done and how that is actually achieved in an orderly way by the parties involved in doing it. To say that the kind of place in which an ethnography is done might make one type of ethnography more appropriate than another is, then, to say that the study of the workplace might be appropriately done ethnomethodologically, while the study of the home might be more appropriately understood in terms of an alternative approach to ethnography – e.g., by adopting a 'defamiliarisation' approach to understanding culture drawn from literary theory and appropriated by anthropology (Bell et al. 2005). Our point is that this strategy is essentially wanting because it fails to see that there are two very different understandings of what the social consists of and how in general social matters can be adequately described.

6.3 The Ongoing Relevance of the Missing What

If we examine some of the ethnographies of non-workplace settings and activities that have been done for the purposes of systems design in recent years then we discern some of the problems that we have raised with respect to our discussion of sociological ethnographies of work and occupations, which concerns their essentially scenic character. Such studies display a narrative character similar to the interactionist studies we examined earlier, which describe what is seen to be done in a setting; a description of human activity that has the character of "X did this, and Y did that", which proceeds to elaborate *what* is done without lifting the lid on *how* it is done as an organised interactional accomplishment. The 'interactional what' of the matter is set aside; missed. In the original CHI paper that gave rise to this book we referenced a paper that examined the use of large screen displays in some American 'mega-churches' to demonstrate the point (Wyche et al. 2007). We chose the study not to insult or slight the authors, as has been suggested (Grinter 2010), but simply because it provided a ready example of scenic description that anyone could take a look at and see what we meant by the term and the particular way in which it typically gets manifest in HCI – i.e., that the organised interactional accomplishment of human activity (prayer in this case) is missing, replaced instead by an analytically naïve narrative detailing *what anyone can see*.

Thus, on reading the paper we find that large screens are commonly placed "on both sides of a central stage", that the screens are used to display "words to hymns and bible verse", that people can be seen to be "looking at screens to know what verse to turn to" and to be engaged in "a flurry of paper turning", etc. However, while people may be seen to do these things, this type of description only touches what is going on in the lightest of ways. Certainly people may be seen to look at screens and to rapidly turn the pages of their hymn books but if asked upon leaving, "What have you been doing?", we think it highly unlikely that people would say, "Looking at screens" or "Turning the pages of a hymn book." They are more likely to say something along the lines of, "I've been to church", or "Worshiping God" or some other similar sort of description. The analytic question is not how members of the congregation might account for what they have been doing after the fact, however, but how is 'being at church' *done* as an interactionally organised matter? In attempting to address that question we might see one person announcing the hymn to be sung, we might see that displayed on a large screen, and we might see some people glancing at the display to find the hymn number and then turning the pages in their hymn books to find it, but in simply describing these *behaviours* are we describing how, in this case, a Christian act of worship is interactionally ordered?

If we describe what we hear in the first moments of a telephone call, for example – "Hello" "Hi, it's Ann" – are we describing how an exchange of greetings is ordered? No. Describing what we hear in terms of one person said hello, and then the other person said hi and recited their name is to '*merely observe*' – i.e., to render a scenic description of what anyone can see or hear. That anyone can see it and hear it is not in dispute. The issue is that to leave it at that is to just lightly touch and not even scratch at the *surface* of the socially organised character of human conduct. Instead of merely observing what anyone can see, we might instead treat the material at hand as something that might be interrogated to see *how* it is *ordered*.

Thus to describe the orderliness of greetings, for example, we might on examining the phenomena notice, as we described in Chap. 5, that they are situated in a rather obvious place in interaction: in an 'initial turn position' at the *beginning* of a conversation. Rather less obviously, it is available to observation that speakers clearly attach *priority* to greetings; it is not down to happenstance that greetings occur at the beginning of conversations then. It is apparent too, at least when we look at greetings being done, that speakers order greeting utterances *relative to one another*: with regards to doing greetings over the phone the relative order consists of the person who answers it offering the first part of the greeting, and the person who calls offering the second part of the greeting. In this respect it is visible that greetings are done in parts, and indeed in parts that are *adjacently paired* and which therefore involve *speaker change*. The change of speakers turns upon parties to the conversation being able to recognise *completion-transition points* – i.e., on recognising that the first greeting utterance has been completed and that it is appropriate for speaker transition to *now* take place; that it is now the caller's turn to speak and complete the second part of the greeting. Thus greetings can be seen to be ordered through the use of a *sequencing rule*: on the recognisable completion of the first part of greeting, the selected speaker does the return. The sequencing rule governing

speaker change can also be seen to be *conditionally relevant*, which is to say that despite the priority attached to greetings in beginning conversations there is nothing in the world that says a greeting *must* be paired with a greeting. Rather, a greeting has to be *warranted*, which is why the caller not only says "hi" in return to the call answerer's greeting but also identifies herself as someone who has the *right* to call.

Greetings are much more complicated organisational things than we can do justice to here (see Sacks 1992; Schegloff 2007). However, the point of the example is not to elaborate greetings per se but, just as it was in our original CHI paper, to beg the question as to the *adequacy* of scenic descriptions that merely recount observed behaviour: to beg the question as to whether or not we can understand *how* people organise, in and over course of their interactions, doing 'greetings' or doing 'being at church' (etc.) from descriptions of surface behaviours?

Now designers interested in the use of large screen displays in American megachurches and other settings may well say that they find such behavioural descriptions useful, but in making this point we are not, to emphasise again, trying to govern what designers may or may not pick up from fieldwork. We can quite well imagine how the behavioural description of hymn book thumbing might stimulate the design imagination: a design where the display of the hymn number on a large screen enables some in the congregation to turn to the correct page, while for others it automatically brings up the correct hymn on their portable device; or we can imagine the order of service being displayed on screen and highlighted as it proceeds; or upon reaching the collection point in the service a customised recommended donation figure is sent to each individual's portable device; etc. In critiquing scenic descriptions of surface behaviour we are not trying to legislate what designers may find useful, but are instead making the point that you *do not need ethnography or ethnographers* to describe what anyone can see. What, after all, is different here to a designer spending a day at church and seeing the behaviours for him or her self? Or watching a video someone has made of the congregation at the church? Or indeed sitting at their desk and imaginatively ruminating upon their own behaviours at church? Maybe such approaches are good enough for some design projects, maybe hanging around for a day would work for some design undertakings, but scenic description of surface behaviours will not furnish a social methodology for systems design. While such behavioural descriptions capture something of interaction they nevertheless fail to grasp how participants are involved in the *orderly production* of interaction and the distinctive *social occasions* it elaborates.

Scenic descriptions of the surface behaviours involved in 'being at church' miss how it is that just these people assembled here and now pull off just this assembly of people *as a congregation at worship together*. It is not just that they are meeting in a church – other kinds of assembly can and do occur in churches – and collective worship can occur in other places to churches; and it is not that it merely consists of such behaviours as looking at screens and turning pages, though doing that may be part of the orderly accomplishment of this particular social occasion, but (and this is the problem) how such behaviours are understood to be part and parcel of the orderly production *of the social occasion* is not provided for in the mere description of behaviour, for the behaviour in question (looking at screens and turning pages) may be part of other *orchestrated* interactions. In noticing this – i.e., that what is

being observed is an orchestrated social occasion, consisting of a group of people being led through a series of pre-formulated or 'scripted' episodes, and in which one person performs particular actions which then provide for the rest of the assembly to perform en masse a series of actions made relevant by 'the conductor' – it may be possible to find that the activities observed in collective worship are ordered through *generally* operative social practices for producing orchestrated social events.

The interactions of people at worship, the interactions of people engaged in a musical recital, the interactions of people at a swearing-in ceremony, all share the characteristic of them being orchestrations. The problem, however, is how do we distinguish between them? How can we discern the orchestrated interactions that provide society's members with the mundane means of recognising that an assembly of people are situationally and collaboratively engaged in doing 'being at church', or doing 'being at a musical recital', or doing 'being at a swearing-in ceremony'? The answer, clearly, is not to be found in scenic descriptions of surface behaviours, but in the *specific* (and missing) 'interactional whats' that constitute each particular characterisable social occasion *as* the occasion that it accountably *is* for those involved in its orchestrated production. If design is turning to the social, and seeking to build the social into the design mix, then these situationally specific 'interactional whats' could be decisive for it, simply because they are decisive for those who are involved in the orderly production of the distinctive social occasions they are engaged in the first place.

However, if ethnography resides at the scenic level of description, detailing merely observed behaviour that anyone can see, as opposed to attempting to analyse the orderly production of the 'occasioned' character of social interaction (Zimmerman and Pollner 1970), it can and will *misdirect* designers' understanding of the foundational relationship between ethnography and systems design and what designers can hope to take away from ethnographic studies. A study by Blythe et al. (2010) illustrates this point and the ongoing relevance of the issue of the 'missing interactional what' to ethnographic studies done for the purposes of systems development. We pick up on this study for two reasons. First it positions itself with respect to studies of work designers have been used to (studies influenced by ethnomethodology) and argues that attending to the setting as a workplace fails to allow design to understand other possibilities for technology. Second, even though we have more space in a book than in the original paper, paying respect to the matters we criticise means touching them in some depth and this is not possible for each study we might find problematic. Thus we select this paper to illustrate our concerns because it is authored by a number of those who would argue that new settings for design require new ways of apprehending them.[2]

[2] One of the 'off-line' criticisms that we received of our original paper was that we were punching below the belt because we used examples that were written by 'junior' researchers. The paper by Blythe et al., however, is authored by robust, long in the tooth, senior researchers of professorial standing. Thus we hope that attention can be focused on the ideas, not the people offering them, which is what we were actually doing in the original paper.

Blythe et al. are interested in design possibilities with respect to a distinctive cohort they call 'the older old' – i.e., (on their definition) people over 80. They explored the possibilities of designing for this cohort in a residential care setting, suggesting that to treat this social setting in a 'conventional' way – as a workplace in which to gather requirements for the development of systems – would result in systems to support carers in their work of caring and managing those they care for, rather than for the residents. Developing an alternative cultural understanding of the setting and the cohort purportedly avoids this situation and subverts the conventional technological approach, which rests upon a 'techno-utopian vision' that seeks to delay and even avoid the placing of the 'older old' in residential homes through the development of 'smart' technology in single occupancy dwellings.

In an attempt to make living in residential care 'better' than living at home ethnographers worked alongside the residents, designers, artists, and school children to develop a range of 'ludic' technologies to enhance the 'lived experience' of being in a care home. The lesson for ethnography and systems design is humbling:

> … most … people … will not have experienced what is like to live in a residential home. There are large distances to be negotiated in order to try and understand the lived experience of our older old participants and to respond to that understanding through design … If we have succeeded at all in enhancing our participants' experience of aging through technological interventions, it is not by observing users, identifying needs, goals and activities, then specifying the requirements of design solutions. It is by spending time, living with them a little, and by letting our relationship grow to a point where we could respond empathically with something. (ibid.)

It is easy to be swayed by an appeal to our own lack of experience and the implicit emotional plea to respond empathically to the situation of others but this conclusion is, nonetheless, one that stands upon scenic descriptions that *mask* consequential aspects of the socially organised nature of life in a residential care home. Furthermore, in masking the orderliness of residential life, the alternative cultural account also masks opportunities that systems design may well have been able to capitalise upon to improve the lot of those who find themselves in such places; opportunities which could certainly involve understanding requirements for technologies but that might also involve just thinking about the *type* of technology and order of technological support, and even whether or not it might be better to try and improve the quality of life through non-technological means.

We can begin to appreciate these issues by considering the principle *scenic* observation: that the cohort, the participants or the people involved here are the 'older old'. This description of care home residents is cast in terms developed by ethnographers, it is a social identity constructed by them not by the residents, and one through which they can then impute certain attributes to those they study. However, the ethnographers are not really studying the 'older old' at all, as if this was an homogenous group of people who possess and display the social characteristics attributed to them by the ethnographers involved in the study. Merely reflecting on our own experiences substantiates the point. One of the authors of this book has an 89 year old mother who lives in her own flat. She does her own shopping; belongs to clubs; drives her car to visit friends and relatives, and to do her shopping;

she flies to foreign destinations; keeps in touch with her son using e-mail and video conferencing apps; she also uses her computer to create greetings cards, store and share photographs she has downloaded from her camera, and to shop online. The same author has also made friends with an 87 year old man whom he regularly meets for a 5 km walk through the French countryside, that is when the 87 year old is not driving to England to visit his relatives, or commuting between his flat in Marseilles and his house in the hills of the Var hinterland where he entertains his 60 year old girlfriend.[3]

The object of the study is not the 'older old' and what they do. They may be describable as very old, but to describe what they do *as* the actions of old people would require showing that, in how they did what they did, they oriented to what they were doing as very old people. The mother and friend cited above are doing things that people who are not 'older old' do. Older people may do just the sorts of things and in just the sorts of ways that people who are not old might do them, and they may also do things in ways that orient to their age; it depends on their personal situation. However, it seems to us that in the examples we are given of the actions of those studied by the ethnographers in Blythe et al.'s study that age and ability is not the primary point. That what really matters here is not that people might be described as 'the older old' but that they are 'residents', and importantly *residents in a care home* for the elderly, that matters. How is this fact consequential with respect to the question of what it is that members of the cohort do? Well, we can readily imagine that some of the things that they do are a result of being elderly and the afflictions that beset older people, whether they are in care homes or not. However, and importantly, as Blythe et al.'s study makes visible, many of the things that the residents do involves building into the design of their actions the fact that they are *residents in a care home*. Take the following observation, for example,

> Staff regularly coordinate group activities such as karaoke sing-a-longs, games of catch and quizzes … Quizzes often featured a flip chart to record answers to memory games. Occasionally these activities would be met with some resistance. A staff member would suggest, for example, a quiz to think of as many boys names beginning with 'B' as possible. Some residents would suggest names but others would mutter '*Bugger off*'.

This and other scenic observations offered in Blythe et al.'s study make it visible, but leave unexplicated, that many of the actions done in the care home are done in such a way that they display that they are being done by residents in *an institution*. As the 'bugger off' example illustrates, an oriented to feature of interaction in the care home is that it is partially regulated by 'staff'. It can also be seen, for example, that residents' actions display that they orient themselves to living in a care home in terms of turning up at meal times. That is they make visible that they reside in an environment that regulates their activities according to set routines and procedures,

[3] As we were writing this a very apposite news item appeared on one of the UK's television channels about a couple who were celebrating 80 years of marriage; she was 101 and he was 105 years old. They were interviewed in their home sitting on the settee, smartly dressed, quipping, holding hands, lucidly reminiscing about aspects of their life together and as 'on the ball' as the 30 odd year old interviewer.

at least by turning up or allowing themselves to be guided to known places at known times.

So, one way in which 'the older old' order their actions and interactions, at least on the basis of the scant data the paper presents, is as residents in an institution. In this respect residents' actions may display characteristics not of the 'older old' (whatever they might be), but of people living in institutions and the topic of the paper may be more about *institutional living* than being about what might otherwise be considered as 'being very old', for not everyone who is very old lives in an institution. In this respect, we might ask questions about institutional living, and institutional actions and interactions, rather than just questions about being old and in this regard a study by the interactionist Ervin Goffman has some distinct relevance (Goffman 1961).

Goffman's work on institutions attracted some notoriety by drawing together institutions that were traditionally considered to be very different from one another: the concentration camp, the prison, the hospital, and the monastery, for example. He noted that many institutions, and we can here include residential care homes for the elderly, have an organisational feature in common with one another, which is that they *encompass the entire daily round* of their 'residents' lives. He referred to these institutions as 'total institutions'. The rhythm of the daily life of those living within in them is provided for by the routines of the institution – through roll calls and meal times, for example. Thus the ways in which people within institutions act or interact with one another can build in and display an orientation to features of the institution itself. Now obviously there are differences between institutions within the category 'total institution'. Relatives do not visit inmates of concentration camps while they do visit inmates of prisons, for example, but under different conditions to those visiting residents of care homes for the elderly. Within these differences it is possible to find how the features of living within a total, all encompassing institution, are contextualised, and how it is possible to characterise one from another by attending to and explicating the 'missing interactional what' to make visible how they are differently and specifically 'occasioned' in action and interaction.

Blythe et al. set that very challenge aside, focusing instead upon a particular scenic feature – here we have some very old people – that sets the stage for how we might address their condition. Yet the very ways in which their condition is addressed – the use of simple ludic technologies alongside the interventions of artists and children – is less to do with old people per se and more to do with living in an institution, which regulates and organises their lives according to set rhythms and routines with little to punctuate or elevate the tedium and boredom of the intervals between them. Is it surprising then that in such an environment the residents responded well to novel technological interventions or the appearance of artists and children in an otherwise *predictable* life? No more so than one would be surprised by the rapturous response that the inmates of Folsom prison gave to Johnny Cash, whose performance punctuated their repetitive, monotonous, tedious and boring institutional lives as inmates in a secure facility.

By focusing on a particular scenic feature – the 'older old' – systems design is being encouraged to move away from the development of smart technologies to support the elderly in their own homes and from the development of better workplace systems to support the delivery of care, to instead focus upon making the lives of some old people 'better' by realising that they can have fun. We do not dispute the fact that new technology can be introduced that makes life in the care home more fun; but then so could more interesting board games or the introduction of imaginative entertainment programmes, some might even enjoy Johnny Cash if he was resurrected. But there are a number of questions here. Again, do we need ethnography to make this point, and an ethnography that seemed to take some time so that the ethnographers could build up empathy with the 'older old'? Do we even need ethnography to help us in the design of technologies to be used for fun? A scenic description may spark the imagination for a fun technology, but it may equally as well come about through a designer visiting an elderly relative in a care home.

However, if we move even a little from the scenic description – that here we have a group of people we will call the 'older old' – and take instruction from participants in the setting in how to *see the phenomena at hand*, the social 'thing' that *they* are actually engaged in, then we might find that there are issues that the design of fun technologies need to contend with. We might find, for example, that the parties to the setting's daily round are more than just the 'older old'. After all the setting of a residential care home for the elderly is a complex one. For some people it is their workplace, whereas for others it is their home, for others a place in which they are nursed, for others possibly the place in which they die, and for some it is a place they visit. How people build this complexity into the institutional order may be consequential for the design of technology to support life in the residential care home. The institutional rhythms and routines of the residential care home drive significant aspects of resident's lives. It may well be the case then that any attempt to alleviate the effects of institutional living would benefit from taking its rhythms and routines into account (Chevherst et al. 2003). It is not just having fun that is the issue, but having fun *in* a residential care home, and how this might impact upon the design of technologies when considered in the round. No matter how much fun a technology might be, if it clashes with the institutional order it might well turn out to be problematic. Understanding, then, how different members of the setting's cohort – residents, the healthy, the ill, the dying, staff, visitors, relatives, etc. – 'work' together to produce the daily round of life in the care home may be consequential to any technology's *actual embedding* within an institutionalised context. Simply taking a scenic feature of a setting – some of the people here are very old – and substituting that for another scenic feature – some of the people here are working or visiting – is really beside the point. The point is that saying fun can make the life of older people 'better' is just to touch the surface. The issue is how having fun can be done within an institutional context such that it is built in to the institutional circumstances of its conduct. Such issues cannot be answered through scenic characterisations, but are demonstrably *missed* by them.

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