Chapter 5
Interpretation, Reflexivity and Objectivity

Abstract We have argued that 'new' approaches to ethnography in systems design return ethnography to its old remit of providing interpretations of and giving meaning to social and cultural matters. Something that features strongly within calls for 'new' approaches is the fundamental assertion that observations are always theory laden and that ethnography is, accordingly, inherently a matter of interpretation. In this chapter we examine this proposition and note that the descriptive practices for seeing and recognising human actions do not belong to or derive from professional sociological theorising, but reside in the doing of the actions themselves. Theory, in that case, turns out to be not so much a necessary way of observing the social world but of *re-describing* its observable characteristics for social science purposes. The job of re-description is also marked by an academic concern with reflexivity and the questions of objectivity and realism. Here there is a push to problematise the relationship between the ethnographer and the field of study with which they are engaged: to problematise how it is that ethnographers can be said to know and understand the world, and how they orient themselves to the factual and objective character of the social phenomena they are observing. For both of these matters the critical error lies in constituting these as *the fieldworker's problem*.

5.1 Observation and Interpretation

Seen and treated as an exemplar of traditional social science approaches to ethnography, Dourish and Bell's (2011) methodological orientation, and the problems we have seen that accompany it, turn upon the fundamental *assertion* that "observations are always theory laden" and that ethnography is, therefore, "inherently interpretive". As Lynch (1999) puts it,

> … 'theorising' in this context [means] the work of constructing intellectual genealogies that commemorate notable authors and foundational writings. It is part of a broader effort to index empirical investigations to bodies of literature. The work of indexing is facilitated by scholarly efforts to identify abstract themes and topics, formulate propositions and postulates, articulate common problems, and ascribe assumptions and presuppositions to authors and schools. This work is more than a matter of encoding and decoding a literature. It also has to do with methodology: the use of criteria, decision rules, and models which tie research designs to scholarly traditions. The point of such endeavours is to isolate funda-

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> mental precepts and to construct intellectual histories for one or another literary tradition of social thought.

It is plain to see in our elaboration of Dourish and Bell's work in the previous chapter that they are deeply engaged in theorising the social, and the suggestion that accompanies this effort is that it *cannot* be otherwise because all empirical investigations – all observations – are always theory laden, inevitably coloured by some literary tradition.

The idea that observation is theory laden is not a particularly new one; it was initially developed by philosopher of science Norman Hanson in 1958. Today it is deeply entrenched in the social sciences and underpins a broad range of otherwise diverse viewpoints. The idea suggests that it is impossible to see something – anything at all – without making use of a theory of some kind to recognise it. The suggestion derives from Hanson's reflections on arguments about the authority of scientific claims. Basically the argument goes that anchoring scientific claims in sensory experience or 'observation' provides an evidential justification that underwrites their authority. Hanson suggested that this was far too simplistic a view of science and the nature of observation; that there is in effect a 'praxiology' to perception:

> Pierre Duhem writes:
>
> Enter a laboratory; approach the table crowded with an assortment of apparatus, an electric cell, silk-covered copper wire, small cups of mercury, spools, a mirror mounted on an iron bar; the experimenter is inserting into small openings the metal ends of ebony-headed pins; the iron oscillates, and the mirror attached to it throws a luminous band upon a celluloid scale; the forward-backward motion of this spot enables the physicist to observe the minute oscillations of the iron bar. But ask him what he is doing. Will he answer 'I am studying the oscillations of an iron bar which carries a mirror?' No, he will say that he is measuring the electric resistance of the spools. If you are astonished, if you ask him what his words mean, what relation they have with the phenomena he has been observing and which you have noted at the same time as he, he will answer that your question requires a long explanation and that you should take a course in electricity.
>
> The visitor must learn some physics before he can see what the physicist sees. Only then will the context throw into relief those features of the objects before him which the physicist sees as indicating resistance. (Hanson 1958)

It might otherwise be said that 'seeing or 'observing' is entwined with some 'scheme of interpretation', some way of making sense of what is seen. Hence the suggestion that observation is always theory-laden.

Now the idea that people use schemes of interpretation is one that ethnomethodologists have themselves used to account for the ways in which they see and recognise the actions and interactions around them and detect the orderliness of their individual and collaborative endeavours (e.g., Sharrock and Button 1991). So how could we object to the suggestion that observation is always theory laden? Well, as Hutchinson et al. (2008) remind us, if observation is always theory laden then so too must be description (as a theory is a description), and this is where things get problematic. In short, the descriptive practices for seeing and recognising some action, for observing and accountably identifying it as the thing that it is – e.g., 'measuring the electrical resistance of spools' – do not belong to or derive from professional

sociological theorising, but from the social settings within which the action occurs. Thus, the practices for *correctly* observing and describing action – for seeing and accountably recognising any action for the action it is – are *built into* the action being observed. Yet professional sociology, anthropology and the social sciences more generally systematically ignore mundane descriptive practices for seeing and recognising or 'observing' action. As Hutchinson et al. put it,

> Professional sociology [etc.] does not provide an extensive re-classification of things that people are doing. That is, they have no substitutes for commonplace descriptions such as 'standing six places from the front of the bus queue' or 'scoring an equaliser in injury time' … Professional sociologists do not want to change or contest these descriptions, but want to argue, instead, about the understandings that attach to these actions when they are considered from the point of view of … some postulated social system …

Theory, then, is not so much a way of observing the world but of *re-describing* its observable characteristics; theory provides a scheme for doing the job of re-description and arriving at disciplinary interpretations of action and interaction. In doing so it leaves 'commonplace descriptions', which are part and parcel of the action and interaction observed, intact but untouched. Thus the mundane descriptive apparatus that people use to make action and interaction observable and reportable or accountable to one another in the very course of doing it is set aside, along with the orderliness that accompanies their accountable doings.

This begs an important question for systems design, and one that drives our objection to the uncritical acceptance of what Lynch (1999) calls, "much abused slogans from the philosophy of science". The question is this: whose theory – i.e., whose scheme of interpretation, whose way of accounting for the orderliness of everyday life – is to be used? The first order scheme that is part and parcel of the settings observed through fieldwork and the action that accountably takes place there, or the second order re-description that puts the fieldworker's in its place? The two forms of description are not at all the same: one is wrapped up in and elaborates the orderliness of action from the point of view of those who are party to it and within which the notion of 'theory' is rarely operative, the other elaborates the orderliness of action from an *essentially abstract* point of view that does possess the properties of a theory as outlined by Lynch above. The uncritical acceptance of the 'theory-ladeness of observation' masks the substitution of members' ways of seeing, recognising and thereby accounting for the orderliness of social life for the social theorists'. The substitution trades on the widespread abuse of the meaning of 'theory-laden' – it does not mean that theory as it is understood in the social sciences is a necessary part of observation. It means, as Hanson's use of Duhem's example makes perspicuous, that "there is more to seeing something than meets the eyeball" (Hanson 1958); that there is a 'praxiology' to perception, which locates seeing in the *recognisability* of the action being observed (Coulter and Parsons 1990).

The 'praxiology of perception' is part and parcel of membership competence. It consists of the taken for granted knowledge of everyday practice (i.e., of what is done in a everyday life and how it is done) that Duhem's visitor, like the sociological theorist qua theorist, does not possess. Taken for granted knowledge furnishes a

setting's members with situationally-relevant schemes of interpretation. These schemes are rooted in and provided for through the mundane practices that members use to both conduct *and* recognise action. They are drawn upon to make correct observations of action and are situated, not in a literary tradition of social thought, but in the settings they elaborate, and in the doing of the actions themselves. Ethnomethodology's injunction is that the ethnographer take the schemes of interpretation that people employ to see and recognise a setting's features seriously. This seriousness is reflected in the requirement that the fieldworker develop 'vulgar competence', i.e., that he or she master just how it is that a setting's members see and recognise just what is going on around them (Garfinkel and Wieder 1992).

In saying this it might be argued that the ethnomethodologist must be making use of a theory to develop vulgar competence and their analyses of social order. As Hutchinson et al. (2008) tell us, those who are wedded to abusing philosophical slogans will insist that those of us who deny having and using a theory to observe and describe action must have one; that it *cannot* be helped no matter what we say, and that if we deny having a theory it can only mean that our actual theory is implicit or tacit: we obviously have a tacit theory about the praxiology of perception, for example. Nonetheless, the praxiology of perception does not provide *us* with a scheme of interpretation – let alone a scheme of interpretation rooted in a literary tradition of scholarly thought – but rather *orients us* to the practices that *members use* to see and recognise the orderliness of what is for them an obstinately familiar world. At best, the praxiology of perception is a presupposition, one of many that define ethnomethodology's program (Garfinkel 2001), but it and they do not constitute a theory of the social. As Lynch (1999) puts it,

> … it may seem reasonable to suppose that ethnomethodology *must* have some sort of coherent theory behind it … [However] Garfinkel and Sacks, in different ways and with differing success, undertook to initiate a practice that was fundamentally different from existing social science methods. They de-emphasized abstract theory and scholarship, and stressed the necessity to *do* studies. Their notions of practice differed from the currently fashionable interest in the social sciences with devising theories of practice, because *practice* was not just a topic of explanatory interest, it was the primary basis for attaining an ethnomethodological mastery.

Ethnomethodology's presuppositions, along with its policies and methods, do not constitute a theory in the sense that a literary tradition is drawn upon to interpret what is observed. Rather, they are "administered and used locally as an instruction" to uncover the orderliness of everyday life in the course of *doing* observational studies (Garfinkel and Wieder 1992).

This, then, throws into doubt the idea that ethnography is 'inherently interpretive'. When social scientists say this it is important to appreciate that they do not use the word 'interpretation' in its ordinary sense – i.e., in the sense that *anyone*'*s* description may be said to provide an account of what they see. Rather, the social scientist as scientist means that observation is always grounded in some theory of the social. This, however, is not *always* the case. Indeed most schemes of interpretation operative in the world are not organised in terms of *literary traditions in social science*, but in terms of the real world practices that provide for the situated

observability of a setting's work. This means, as Coulter and Parsons (1990) remind us, that "only some observations are theory laden" and that while an ethnographic account might be an interpretation in the ordinary sense of the word, where this effectively means that it provides 'just another point of view', it need not be so in the rarefied sense meant by social scientists: this rarefied kind of interpretation is not *necessarily* an inherent feature of ethnographic work.

But surely ethnomethodological accounts are second order accounts and thus offer essentially abstract interpretations just the same as mainstream social science accounts do? No, ethnomethodological accounts are of an entirely *different* order. They do not seek to re-describe a setting's features but to explicate through the production of 'corrigible sketch accounts', or detailed depictions of action and interaction being done, the lived work of a setting (Garfinkel and Wieder 1992). In ethnomethodological terminology, these sketches provide 'praxiological' accounts that make the activities done by members in a setting and (importantly) how they are done 'instructably observable' – i.e., see-able and recognisable in the same terms that they are for members. They are, therefore, corrigible – members can point out mistakes and identify corrections – and are as such open to revision. The same cannot be said for traditional ethnographic accounts as the work of a setting and the practices that members use to conduct, see and recognise it are supplanted by theoretical interpretations: the work through which action and interaction is achieved as that which it recognisably is for a setting's member has *not* been explicated by traditional ethnographic accounts. The orderly work of the streets, as it were, is surplus to the theoretical re-description of everyday life.

Now for all this talk of interpretation it is worth pointing out that people do not go around the real world with pre-existing interpretive schemas in their heads that they overlay onto words or bodily movements; rather the meaning of action is visible in its doing. Take an ordinary run of the mill workplace in the morning by way of example. Jim walks through the door of the kitchen to make a cup of coffee before starting work. He encounters John, who was there before him and who on seeing Jim says, "Morning Jim". Jim responds, saying "Morning John". In doing this Jim has *not* interpreted John as greeting him, he has not gone through some indefinable cognitive process of interpretation to make what John has said 'a greeting', as if he could come up with another interpretation such as John being humorous that would stand alongside the greeting. First, Jim recognises John's utterance as a greeting in it being done *as* a greeting – John uses a standard greeting term in his culture, situated in an 'initial turn position' in a sequence of interaction that provides for Jim to return a greeting (Schegloff 2007). Second, any sense that Jim has of John being humorous turns upon Jim *first* being able to find that John has greeted him. In any case, even here, this seems to stretch what we ordinarily mean by interpretation. We might rather say that Jim employs the "hearer's maxim" (Sacks 1992) and recognises the humour in John's greeting through the contextual character of their interaction – they go through this ritual every morning, John knows that Jim was out late last night, John has a twinkle in his eye and a knowing lopsided grin on his face, for example.

Simply put, actions are recognisably produced to be the things that they are. They are produced to be accountable – observably and reportably just 'this' (Garfinkel 1967). If they were not then how could the very phenomena of sociality be possible? How could Jim respond to John in a recognisably appropriate way? Jim and John are not interpreting their words and bodily movements through some theory that endows their actions with meaning. They are *doing accountable actions* and doing them through the practices whereby such things are recognisably done in their culture, which means that other members of the culture can recognise what is being done too. Jim and John's exchange of greetings is taken from a recording that an ethnographer (one of the present authors) made, and as a member of the same culture the ethnographer was just as able to recognise an exchange of greetings when it was done as the participants themselves. Like Jim and John the ethnographer did not require a body of literature, a theory of action, to see what was being done for what it was.

This takes us back to Hanson's quotation of Duhem's physics experiment. The recognisability of what the physicist is doing – 'measuring the electric resistance of the spools' – does not turn upon the visitor's interpretation of his actions, but upon his *competence* (or lack of it) in the culture of experimental physics. As Duhem puts it, "the visitor must learn some physics before he can see what the physicist sees." We would argue that the same applies more generally. That the fieldworker needs to learn how the people he or she studies see and recognise action and interaction, and that vulgar competence is an essential condition of ethnography, *not* theoretical interpretation. One might wonder then, if theoretical interpretation is not a necessary, why it is that anthropologists, sociologists and social scientists more generally persist with what appears to be such a perverse practice? What is it about mainstream social science that compels social scientists to keep making the same mistake? Why would the ethnographer set the explication of practice aside and choose instead to re-describe the world through theoretical interpretation? It is towards understanding the predilection of some ethnographers to keep on ignoring the inherently accountable organisation of everyday life, in favour of reflecting upon their own descriptive practices, that we turn next.

5.2 Reflexivity in Ethnographic Observation

Reflexivity has become a key idea in many quarters of contemporary social science, it has certainly (re)defined it over the last generation. Dourish and Bell (2011) put it as follows,

> There is, of course, more to ethnography than its ability to ground conversations in daily lived reality. It also has attendant to it a set of theoretical practices that have to underpin critical self-reflection. By this we mean the ability to talk about one's biography, location, and subjectivity, and the ways in which they might shape the identification of research problems, projects, and participants. While we would hate to see ubicomp practitioners and the field more broadly suffer through the 'crisis of representation' that has beset

> anthropology, in particular, for the last twenty-plus years, there is much to be learned from that process, and a small dose of critical reflection about subjectivity, positionality, and voice would go a long way.

Dourish and Bell also point out that such critical self-reflection is largely absent in contemporary systems development, but that developing an appreciation of it is important to the interdisciplinary mix.

> To understand the ways in which ethnography figures within and without ubicomp is to understand not just its methods but also its methodologies and larger epistemological concerns with reflexivity … (Dourish and Bell 2011)

We agree, but not for the same reasons. What for Dourish and Bell is a source of illumination is, for us, a primary source of obfuscation. Reflexivity as it is largely understood in mainstream social science guarantees the systematic ignorance of the already accountable organisation of everyday life. Academic reflexivity is blind to the *incarnate reflexivity* that is 'built in' to everyday accounts as a methodological matter constitutive of recognisable social settings, scenes and events. It is towards understanding why and how academic reflexivity – the kind of reflexivity that Dourish and Bell champion – achieves and guarantees this ignorance that we turn our attention below.

The impetus towards critical self-reflection was occasioned, as Dourish and Bell point out, through what is described as the 'crisis of representation'. This afflicted not only anthropology but cut through the social and human sciences as well. It's origins and implications for ethnographic work are outlined by George Marcus and Michael Fischer in their critically acclaimed book *Anthropology as Cultural Critique*. Marcus and Fischer locate the crisis in the failure of the post-World War II 'positivist' paradigm that sought to develop an objective science of Man and was characterised by total or 'grand' theories of society. Academic dissatisfaction grew with these overarching theories during the 1960s and 1970s, in light of their failure to provide realistic and accurate representations of the conflict and social changes that were occurring (most notably in America) at the time. The dominant positivist paradigm could not handle what Marcus and Fischer (1986) call the 'messier' side of social action, and so confidence in it waned and the so-called crisis of representation emerged and took a widespread hold on the human sciences. At its heart lay uncertainty about Marcus and Fischer termed 'adequate means' of describing social reality. Our examination in Chap. 3 of the way in which two different grand theories of social life, a functionalist theory and a conflict theory, give rise to different descriptions of the cultural milieu in the same environment, is an example of the type of problem encapsulated for anthropology by the terms 'crisis of representation' and 'adequate means' of description.

Seen from the perspective of anthropology, uncertainty centred on the ethnographic account:

> Ethnography is a research process in which the anthropologist closely observes, records, and engages in the daily life of another culture – an experience labeled as the fieldwork method – and then writes an account of this culture … These accounts are the primary form in which fieldwork procedures, the other culture, and the ethnographer's personal and theoretical reflections are accessible to professionals and other readerships. (ibid.)

The cause of the uncertainty lay in the largely unexamined incorporation of the 'generalist orientation' that underpinned positivism in descriptive practice, which resulted, for example, in Malinowski's functionalist account of the Kula ring as we discussed in Chap. 3. In other words, and somewhat ironically as we shall see, the uncertainty revolved around a concern with bringing general theories of society into the description of particular social occasions and events. This occasioned a new kind of holism marked by a shift away from developing total theories of society to understanding 'mental culture' – i.e., what it means to be a member of a particular culture. The shift recast interpretive anthropology as a relativistic enterprise in which the ethnographer acts as a cultural interpreter of local systems of meaning to provide a "jeweller's eye view" on the world. It was accompanied by a pervasive and highly self-conscious interest in the *writing* of ethnographic accounts – the primary form of access that others have to those local systems and the ethnographer who studied them. This 'highly self-conscious interest' (or critical self-reflection) in the production of ethnographic texts is what constitutes academic 'reflexivity', and to our minds it creates a confusion with respect to what it is that is of interest in the ethnographic undertaking.

Academic reflexivity places *the fieldworker*, not the people he or she is studying and the naturally accountable organisation of their day-to-day activities, *at the centre* of the ethnographic enterprise. It is marked by methodological interest in the communicative processes by which the fieldworker gains knowledge of his or her subjects, and thus becomes the pivot-point around which issues of *validity* turn.

> The validity of ethnographic interpretation came to rest on fuller understandings and discussion of the research process itself … and the epistemological groundings of such accounts. (Marcus and Fischer 1986)

This, of course, may seem like an entirely reasonable matter. After all any science or rigorous means of inquiry must be able to account for how it knows the world – its methods as it were – but academic reflexivity reframes what is perceived as a positivistic expectation that an objective account of methods be provided by relocating epistemology in the fieldworker's accountable relationship with his or her subjects. In place of a conventional understanding of methods, the focus shifts to understanding how it is that interpretations are constructed by the fieldworker from the interpretations of his informants. This leads to the kind of claims made by Williams and Irani (2010), for example, that ethnography respecifies criteria of rigour by relocating it in "reflexive accounts generated by the body as an instrument of knowing" – the body in question being the ethnographer's, situated in the field (Conquergood 1991), hence our argument that academic reflexivity places the fieldworker and not the people he or she is studying at the centre of the ethnographic enterprise.

Academic reflexivity refocuses attention on the process of interpretation that *the ethnographer* engages in and develops to understand social life. This is seen as a challenging but essentially unproblematic move by Marcus and Fischer:

> [It] does not mean that the traditional rhetorics and task of anthropology to represent distinctive and systematic cultural forms of life have been fundamentally subverted … Rather,

> its traditional task is now much more complicated, requiring new sensibilities in undertaking fieldwork and different strategies for writing about it.

These 'new sensibilities' revolve around and focus attention on the ethnographic process of interpretation, and 'different strategies of writing' provide for the local elaboration of that process and the accountable relationship it enabled the ethnographer to foster with his or her subjects – in short, how it is that the ethnographer came to 'know' other cultural forms of life. By way of example, Marcus and Fischer elaborate 'defamiliarisation' strategies (see Bell et al. 2005) and Van Maanen (1988) elaborates 'confessional' and 'impressionistic' strategies (see Rode 2011), but reflexive strategies of writing are today diverse and ever developing, driven by broad theoretical interests in the social sciences. If they share anything in common apart from an analytic commitment to critical self-reflection then it is their avowed counter-position to what Van Maanen calls "realist tales".

Realist tales are described as flat and dry in comparison to their reflexive counterparts, focusing on regular and often-observed activities in a setting and making use of quotations from the setting's members to convey to readers, as Van Maanen puts it,

> … that the views put forward are not those of the fieldworker but are rather authentic and representative remarks transcribed straight from the horse's mouth.

Ethnomethodology is cited as a "realist mode" of ethnography, which seeks to elaborate the perspective and practices of a setting's members and in whose accounts the author of the text (thus) disappears from view. Van Maanen argues that realist tales trade on the assumption that what the fieldworker saw and heard in the field is more or less what anyone else would see and hear. However, academic reflexivity challenges the assumption that there is in effect an objective reality that is knowable independent of the particular observer, and takes it instead that what is seen and heard and what therefore comes to be known is always dependent *on the observer*, on the ethnographic self towards which so much critical reflection is therefore directed.

In an attempt to educate designers as to how to read and interpret ethnography, Dourish (2014) puts it like this,

> What does it mean to suggest that the self is an instrument of knowing? It requires us to imagine that the process of ethnographic fieldwork – going places to see what happens – is not merely a question of traveling to the places where things happen in order to witness them but is more about the insertion of the ethnographer into the scene. That is, if we think about ethnography's primary method as participant-observation, then it directs our attention towards the importance of participation not just as a natural and unavoidable consequence of going somewhere, but as the fundamental point. This, in turn, suggests that question that often arises in interdisciplinary investigations – "doesn't the ethnographer alter things by being there?" – is ill-founded on the face of it. That is, the ethnographer absolutely alters things by being there, in exactly the same way as every other participant to the scene alters things by being there; indeed, there is "no there" without the participation of whatever motley band of people produce any particular occasion …

This reflexive view on the world suggests that knowing very much turns upon the ethnographer's interventions in the world and the relationships he or she develops

with 'whatever motley band' they are studying during the course of those interventions. Academic reflexivity is all about explaining the nature of that intervention – i.e., the process whereby an interpretation of the motley band's cultural forms of life was constructed in and through fieldwork – and in this respect reflexive writing strategies have two distinctive purposes.

In the first instance these strategies are intended to help the reader validate an ethnographic account. They provide, as Marcus and Fischer (1986) put it, "readers with ways of monitoring and evaluating the sources of information presented." It is notable that these strategies are not members' strategies but anthropological strategies extraneous to the actual situations they offer interpretations about. Nonetheless, an ethnographer who fails to demonstrate academic reflexivity in his or her writing is going to find their account invalidated by the professional anthropological community. In the second instance, and arguably in reaction to those of us who might argue against what is essentially an *idealist* position and suggest that there is for society's members a world out there that anyone can go and see and hear, and that what they see and hear does not necessarily depend on the theory-ladeness of observation, nor does it necessarily mean that they will alter what goes on in the course of seeing and hearing it, then reflexive writing strategies are purposed with the problem of understanding what has been seen and heard. Thus, even if the interpretive anthropologist were to concede that there is an objective reality out there that can be realistically described – which of course is never going to happen – there would still remain the problem of working out what it *means*. As Van Maanen (1988) puts it

> … it is no longer adequate for the fieldworker to tell us what the native does day in and day out. We must know what the native makes of all this as well.

Understanding the meaning of specific cultural forms of life is, for Marcus and Fischer, a challenging matter. The challenge consists in responding to critiques of relativism that sideline ethnographic studies for failing to connect local cultural forms of life to larger social organisational matters. The challenge for the interpretive ethnographer in working out the meaning of local cultural forms of life thus becomes one of working out, as Marcus and Fischer put it,

> … how to represent the embedding of richly described local cultural worlds in larger impersonal systems of political economy.

Understood as a representational problem this *problem of generalisation* is, again, seen as an issue of textual construction and Marcus and Fischer suggest that 'world-system theory' may be used as a means of building some vision of larger world-historical trends into ethnographic accounts (Dourish and Bell, as we have seen in chapter four, invoke the idea of *infrastructure* to do the same job). They also suggest that this will involve a radical reworking of the grounding assumptions by which anthropologists have conceptually constructed their subjects, one that recasts the subject of ethnographic inquiry from a setting inhabited by members to 'the system' (be it infrastructure or whatever other 'world-historical' motif is at hand at the time) and how it spans different cultural locales and even different continents.

> … the point of this kind of project would be to start with some prior view of a macro system or institution, and to provide an ethnographic account of it, by showing the forms of local life that the system encompasses, and then proposing novel or revised views of the nature of the system itself, translating its abstract qualities in more fully human terms.

What we end up with, again, is the idea of multi-locale or multi-sited ethnography and the invocation of socio-cultural theories to 'contextualise' fieldwork, as we examined in the previous chapter.

Interpretive anthropology has come full circle then, first eschewing general theories of the social and adopting a relativist stance and then adopting general theory in order to avoid its studies being marginalised. Marcus and Fischer along with other interpretive anthropologists and those who draw on their work in other contexts obviously do not recognise the irony, seeing the reflexive approach as one that provides 'bottom up' views of the social rooted in the so-called 'messiness' of social action, in contrast to the overarching top down theories that sparked the crisis of representation in the first place. Nonetheless, any and every attempt to locate local order in 'visions of larger world-historical trends' inevitably obliges the ethnographer to engage in generic practices of sociological theorising, no matter which way round the theorising is construed of. The result (as we have seen in our discussion of infrastructure in Chap. 4) is that ethnography is thus purposed to render theoretical constructs real-worldly, at the expense (as we have seen in Chap. 3) of the action and interaction that actually goes on, on the ground and understanding its naturally accountable organisation, not that interpretive ethnography can handle that either.

Critical self-reflection or academic 'reflexivity' inevitably reduces the study of social action to the study of how it is that ethnographers can be said to know and understand the world. With it, the problem of adequate description of the social becomes a problem of writing texts that 'embed' the ethnographer's interpretation of social action in 'larger impersonal systems'. Now, from the point of view of systems design we might think, though of course we might be entirely wrong, that designers might feel somewhat cheated if what they got when they hired an ethnographer to inform the development of a computing system in some way was not an understanding of the social action the system was being designed to support, but an intellectual account of how the ethnographer went about interpreting what he or she saw and heard in the field and what that means in terms of the broader world-system that the people studied live in. A designer might, by way of example, take a look at Paul Willis's *Learning to Labour* (1977), a study of the schooling of working class males in the UK that Marcus and Fischer are fond of citing as in many ways exemplary and a designer might ask, were they to be tasked with building a computing system to support teaching and learning in this context, whether or not an interpretation of the local culture and its relationship to capitalism would help them build a computing system? What would they be building a system to support? What teaching and learning activities would the system support? Try as they might when they read the text they would not find an answer to these kinds of questions. These sorts of questions reflect of course, as we have previously noted, the sort of empirical interest in ethnographies of work as a means of furnishing requirements for systems, which have been strongly criticised by proponents of 'new' approaches to

ethnography. Nevertheless they are the sorts of questions that actual systems designers have asked in the past and continue to ask as they seek not only to understand the contexts in which systems will be placed, but also to build systems that fit into them.

Despite the pervasive nature of academic reflexivity as a fundamental mode of inquiry and representation in the social and human sciences, its ability to handle practical matters – both of the accountable organisation of everyday life and the building of systems rooted in the social – begs questions as to the appropriateness of such an approach *in* and *for* systems design. Interpretive anthropology, as any other form of reflexive inquiry, is built on an inherent cognitivism that places the ethnographer at the centre: it's all about how 'I' conducted 'my' study and connect 'this' local situation to 'these' world-systems. It should be no surprise then that academic reflexivity *makes* ethnography into an inherently interpretive enterprise that can do nothing else but ignore the accountable organisation of everyday life, as there is little room in the interpretive ethnographer's *egocentric* world for anything but his or her own methodological ruminations. Even if everyday life were to smack the interpretive anthropologist in the face, the resulting account would still be about the ethnographer and how he or she interprets the meaning of any such action: as Garfinkel and Wieder (1992) put it, "eyeless in Gaza."

Nevertheless, it seems plain to mainstream ethnographers that ethnographic knowledge turns upon 'the body as an instrument of knowing', or the fieldworker 'being there' in more prosaic terms, and that academic reflexivity is, as Marcus and Fischer put it,

> … a means of attacking the naïveté of those who think cultural transmission can occur without mediation or interpretation, that ethnographers can merely be scribes …

There are, however, a number of confusions built into the idea of the body as an instrument of knowing, the role and effect of the ethnographer's participation, and the idea that understanding how participation gets done elaborates that which is participated in. Really all of these emanate from the way that the social sciences have *since their inception* understood their task to be one of *re*-interpreting what everyone knows about social life in terms of theoretical schemas, including the schemas of self-reflection. The first confusion here concerns the importance of 'particular cohort production' – i.e., that what is going on in the social world turns in some way on who is party to its production, and how they are party to it. In a sense it is taking the idea of addressing the actor's point of view to absurd lengths, and illustrates the problem of rendering the actor's point of view in terms of the actors themselves (Davies 1999). Ethnomethodology has respecified the actor not in terms of *who* they are but in terms of *what* they are accountably doing, not in the sense of what it is that they can be said to be doing, how their action can be interpreted if you like, but in terms of *how* they are doing that which they are recognisably doing (Czyzewski 1994). This is a radical re-orientation of the social science enterprise, an enterprise that in the hands of interpretive anthropology arrives at the absurd position that the interest is in the ethnographer and their study practices above the social world to which those practices are applied. Ethnomethodology's respecification

of social science radicalises the very idea of the actor's point of view (Sharrock and Button 1991). An example might help, and we turn to a branch of ethnomethodology, conversation analysis, to make it.

It might be supposed that given the emphasis on situated action in ethnomethodological studies which have been done for design purposes that ethnomethodology places importance on the 'here and now', the context, the setting in which actions and interactions take place. This would be correct, but only in as much as it emphasises how social matters are ordered and organised as *local* achievements. This is not to say that the practices or methods involved in the local achievement of order are dependent upon who is involved. In the social sciences who people are typically matters for its descriptions, where the 'who' can be provided for by a particular social theory or methodology. For example, in conflict theory people are identified by their relationship to power; theories of patriarchy stress the gender identity of the person; or in methodological terms the 'who' is prescribed by the method, as with academic reflexivity where the identity of the author as, for example, an ethnographer, assumes importance. However, in a seminal paper on the 'systematics' of turn-taking in conversation Sacks et al. (1974) provide a powerful example of the ways in which the methods people use to order and organise their actions and interactions can cut across the identity of persons, that they can be *cohort independent*.

Thus Sacks et al. demonstrated that the orderliness of an exchange of turns in conversation is not dependent on *who* is involved. For instance, an exchange of greetings is in part organised in terms of 'adjacency pairs' – e.g. "Good morning." "Good Morning." – as a recognisable feature of turn-taking (Schegloff 2007) *irrespective* of the fact that it involves, for example, the headmaster of a school and a pupil or even a random selection of pupils, though it can accommodate just who is involved (e.g., "Good morning Jones.", "Good morning Headmaster."). Specifying just who is involved is not a requirement of conversational exchange, however, any more that specifying just where it takes place is. Thus, at the same time as turn-taking in conversation was shown to be cohort independent, it was also shown to be *setting independent*. The methodical ways in which people conduct talk cuts across different social environments then, such as conversation being carried out on an airplane, or at work, or in a restaurant, etc.[1]

None of this is to say that personal attributes and settings are not important for the organisation of human action and interaction. It is to say that their relevance is an occasioned matter, that they are made visibly relevant in peoples' actions and interactions. In other words, the relevance to a description of interaction in terms of its occurrence in a particular setting, or its being done by a person to whom a personal attribute may be assigned, resides in the action or interaction itself, not in some sociological characterisation underpinned and motivated by a particular social theory or methodology. Thus, for example, the relevance of the fact that talk is taking place in a court of law resides in the way in which people organise taking turns

[1] It is telling in this respect that some of the early material used by Sacks was gathered from therapy sessions and was used to describe how, for example, stories are constructed, not how 'therapy talk' is done (see Sacks' *Lectures on Conversation*).

at talk in courts of law, which often displays different characteristics to the organisation of turns at talk in casual conversation, and in the selection of the terms used to refer to persons (e.g., the witness, defense counsel, your honour, etc.), not in a sociological theory of power and the judiciary.

The point is that it is not so much the persons involved, the actors, that are of concern in conversation analysis but rather the methodical practices employed by people to order action and interaction as conversation; it is not 'the actors' that conversation analysis strives to address. Schegloff takes this point up in a response to Stivers and Rossano's (2010) description of initial turn-taking sequences, which they account for in terms of the way in which actors impose normative obligations on one another. However, conversation analysis, as Schegloff (2010) describes it is not concerned with what people do as actors but *how* they do what they do and how it is possible to gain an understanding of *how* they do what they do through an examination of their actions and interactions. It might appear that the difference is slight, and Stivers and Rossano might wonder at the force of Schegloff's problem. However, much turns on the difference between describing things in terms of *the doer* or *the doing*.

Starting off analysis from the actor's point of view means that issues such as who the actor or actors are might be relevant and what 'they' in this instance may require from particular 'others'. However, as conversation analysis has made perspicuous, the organisation of conversation cuts across issues of who – the organisation of talk in action and interaction is *independent of particular actors*. Thus it is not an actor that exerts a normative obligation on an interactant, it is the organisation of talk (just try breaching the situation next time someone you know greets you, try ignoring them and see what happens). Garfinkel, as we mentioned above, described how actions are done so as to be accountable, that is they are done so as to be recognisable as what they are, and this recognisability resides in the organisation of the action *not* in the person performing that action. It might otherwise be said that the orderliness of action is not a personal property. While individuals use the organisation of action to get the activities they are involved in done, the organisation is not *reducible* to individuals: you do greetings as they are recognisably done in your culture just like the next man or woman, you queue in the way in which queuing is recognisably done by the others around you, you drive in much the same way as well, and so on. You do things in the way in which everyone else does them because that is what your culture provides for and equips you with: methodical practices for producing and recognising action and interaction (Sacks 1984). Schegloff (2010) sums up what is of interest then in quoting Goffman,

> … not persons and their moments, *but the organisation of those moments*. (our emphasis)

The second confusion raised by academic reflexivity concerns the very idea of reflexivity itself. It is a confusion introduced by the social sciences in the appropriation of the notion of reflexivity *from the everyday world*, where it is an indispensable part of the way in which members order action and interaction. In that appropriation the way in which reflexivity works and how its workings are used in the everyday world is *lost*. Garfinkel (1967) described reflexivity not as reflective state, self-critical

or otherwise, but as an incarnate feature and inherent property of the practical organisation of everyday life; a constitutive feature of account-able action. The hyphen here is not accidental, it emphasises that action is done in such a way that members can recognise *and* describe it as the particular thing that it is. A simple example will hopefully clarify the point. Andy is walking out of the office; Graham and Peter are walking in. Graham knows Andy and says, "Hi Andy." Andy looks at Graham, makes no response and walks on by. Graham turns to Peter and says, "He snubbed me. He didn't even acknowledge I was there, let alone say hello!" Now any wide-awake member of society will see the snub in this. It is an ordinary if uncomfortable occurrence and while the whys and wherefores of it may not be apparent, that Graham has been snubbed by Andy is plain to see. What the ethnomethodological preoccupation with reflexivity would also have us see is *that* the snub can be and is an account-able matter for Graham and anyone else witnessing it *is* provided for by the methods implicated in its production: there is a reflexive relationship between recognising *what* was done and *how* it was done.

To unpack this we can see that one way in which a snub can be done is through *not doing something*, and quite visibly not doing it. Thus, the snub in this case has been done by Andy not doing what was provided for in Graham's initial greeting, which is to return it. As Schegloff (2010) would put it, the snub is done by a return greeting being 'noticeably absent'. The account "He snubbed me. He didn't even acknowledge I was there, let alone say hello!" brings to notice the absence of a return greeting and is, as such, tied methodically to the snub's production, to the not doing of that return greeting. The account is a constitutive part of the act then, not in the sense that it interprets what is being done, but in the sense that there is a *methodical relationship* between the *accounts* that members provide in the course of action and interaction and the *production* of recognisable social scenes and events (e.g., the witnessable doing of a snub). Thus, reflexivity in everyday life speaks to the entwined or interdependent relationship between action and its account-ability. This relationship is given and used by members in methodical ways, as Andy's snub was given to Graham in the above example in the noticeable absence of a return greeting. Reflexivity as a members' matter stands in stark contrast to reflexivity as used by the social sciences then. In everyday life it speaks to the methodical relationship between members' accounts and the settings, scenes and events they make observable (Garfinkel 1967), whereas in the social sciences the idea of reflexivity is used by to license reflection on one's investigative practices and theoretical interpretations of action.

The reflexivity of accounts in everyday life also makes it plain to see that members' are skilled *analysts* of the social order. They not only know how to put their actions together in methodical ways so as to provide for the recognition of their actions, they also know how to 'see' and 'read' those methodically assembled actions; hence Graham being able to account for what Andy did in not returning his greeting. This brings us full circle back to the matter of praxiology. The reflexivity of accounts invites the ethnographer to develop mastery in the methodical practices that members use to see, recognise and understand the social world. The reflexivity of accounts puts in place of the self-conscious ethnographer and the ethnographer

as scribe an *apprentice* model of fieldwork (Lynch 1999); a model that puts emphasis on the ethnographer developing vulgar competence in the activities being investigated and the sketching out of praxiological accounts. These accounts provide both the ethnographer and the reader with 'tutorials' elaborating a setting's distinctive features – its local cultural forms of life if you will – and how they are made accountable in the methodical achievement of the situated interactional work that provides for their observability. This, in turn, elaborates the practices that reflexively provide for the recognisable orderliness of a setting's work.

While the tutorials may be viewed as interpretations in the ordinary sense of the word, that is, as 'just another' perspective on the social, they do not need to be 'contextualised' or embedded in larger impersonal systems in order to convey their meaning and generalise the results. On this view, the meaning of action is embedded in its accountability – in the observable and reportable sense it has for members as they go about doing, seeing, recognising and reasoning about action. Meaning is, then, *built in* to action and the methodical ways in which it is built in provide for the generalisation of ethnographic results, not as reflexive interpretations embedded in larger impersonal schemes but as praxiological accounts that *display* the practices that members use to recognisably assemble their activities as accountable affairs in society (Sharrock and Randall 2004). The generalisations provided by praxiological accounts are not of the same order as those provided by generic practices of sociological theorising then. Generic practices of sociological theorising do generalisation by extrapolating, through theoretical means of interpretation as elaborated in Chap. 4, from some specific activity or cultural form of life to broader world systems. Whereas praxiological generalisations operate by describing the practices that members use to see and recognise what Sharrock and Randall call 'the regularities' of everyday life. Practices, in other words, that enable *members* to detect and analyse the orderliness of action (e.g., to see and recognise what Duhem's experimenter is doing) and which elaborate that order in being reflexively implicated in its naturally accountable production (Crabtree et al. 2012).

The rub then with respect to reflexivity is that the social sciences have appropriated an ordinary feature of social life to do ethnography, but in that appropriation they have made reflexivity into a disciplinary matter as opposed to a members' matter; a matter for intellectual consideration, rather than a practical matter bound up with the organised conduct of social life. The base assumption that witnessing something involves mediating it by being there and interpreting it through a theory leads to a dilemma of not being able to touch the thing mediated because it only exists in its interpretation. In these terms there is, as Dourish puts it, "no there" there, nothing that exists independently of the ethnographer. There is only that which is constituted through participation. Hence the necessity for reflexive examination of the ethnographer's mediating and interpretative practices, for that is all there is. Thus all ethnography can be is a reflection on how *it* engages with the world and what *it* finds in that engagement.

Nevertheless, and putting the absurdities of idealism aside, just by being in the world it is obvious to any wide-awake member of society that the world is full of 'heres' and 'theres' and full of things taking place within them. The world is like

that for the interpretive ethnographer when he or she takes his or her academic hat off, just as it is for anyone else. They are snubbed; they snub. They misunderstand; they are misunderstood. They do *things* to other people and they have things done to them. And they do the things that they do in recognisable places, 'here' and 'there'. It does not take an act of interpretation based on some social theory to recognise action in the world. A spouse packing their bags, banging doors and remaining steadfastly mute as they walk out does not require some theory to be used to interpret that they are leaving home; it is plain to see that they are leaving in the doing of these things. Is the interpretive ethnographer going to stand and reflect on how their theory enables them to interpret this, or are they going to breakdown in tears or shout out "come back" or "good riddance"?

None of this is naïve realism; it is understanding that society's members live in a world of 'heres' and 'theres' animated by the things they do *independently* of the interpretive ethnographer's imagination. Ethnomethodology respecifies understanding of the world as residing in members' analyses of it, analyses provided for through *their* reflexive practices, which are constitutive of the world.[2] The point of note here is that, understood in these terms, the ethnographer is not merely a scribe noting down what they see, or an interpreter constituting 'heres' and 'theres' in the world through a mediating social theory. Rather, the ethnographer is being instructed in the 'heres' and 'theres' of the world by those doing the things that accountably animate the specificities of social life and make them demonstrably real and concrete. Such things, such specificities, such settings, scenes and events that actually make up the social world we live in, may and indeed will be done by some 'motley gang'. However, what is done *does not* turn on the gang's personnel, but on the organising methods of action and interaction the gang observably and reportably employ to get their business 'here' and 'there' done.

A last point here with regard to the significance of the reflexivity of action. Whenever we give an example we turn to simple, what some might call trivial, examples, such as greetings'. We can understand how it might appear that questions regarding the larger social system in which local actions and interactions could be seen to take place in, or questions as to the meaning that technology has in a culture, may seem to the designer, as indeed they seem to the mainstream social scientist, to be ones that are more important than people saying hello to one another, or not saying hello as the case may be. However, we need to bear in mind what is at stake here. In describing the snub, for example, we are not merely describing what happened between Andy and Graham, but are using what happened between them to make visible that the real world intelligibility of members' accounts in general is provided for through particular methodical practices which organise the social world. Thus, in the snub we view a particular *ordering mechanism* at work: not doing something that has been provided for. It would be short-sighted to see any such a mechanism as trivial, for indeed wars have been accounted for in these terms.

[2] This, of course, is a strong claim to make, especially with regards to the world of 'natural facts'. See Garfinkel et al. (1981) for a detailed explication of the point.

At 11.15 on 3rd September 1939, Neville Chamberlain, the then Prime Minister of Britain, declared war on Germany in the following statement:

> This morning the British Ambassador in Berlin handed the German Government a final Note stating that, unless we heard from them by 11 o'clock that they were prepared at once to withdraw their troops from Poland, a state of war would exist between us. I have to tell you now that no such undertaking has been received, and that consequently this country is at war with Germany.

Here, Chamberlain is making it visible that something that was provided for – word that the German Government was prepared to withdraw their troops from Poland – was noticeably not done. Social mechanisms of action and interaction may be used to do things we might characterise as trivial, but they might also be used to do things considered momentous. Schegloff's quotation from Goffman is again apposite:

> … not persons and their moments, but the organisation of those moments.

5.3 Objectivity and Realism

The interpretive anthropologist, like most mainstream social scientists, is likely to insist that praxiological accounts are nothing more than realist tales and that we are, despite our objections, naïve realists who would perpetuate an outmoded 'objective' program of research. Merritt (2011), by way of example, insists that,

> Crabtree, et al., believe that ethnography in HCI research should only be used according to the former status quo … [they] argue for objective, empirical observations for use in HCI design …

The issue of objectivity and the idea that social reality is independent of the descriptions that can be given of it have troubled social science from its very beginnings. However, those troubles are ones that arise for the social sciences in the ways in which they have developed an understanding of what it is to do social science, and they become ones that are omni-relevant. This contrasts with the ways in which objectivity and reality are spoken about in the everyday world, where neither are omni-relevant matters, but occasioned ones.

We can and do perfectly well use the words 'objective' and 'reality' as everyday matters without a problem. Someone's wife really did leave him, for example, and while he is a friend we are trying to be objective about it because it has to be said, he really did give her a hard time. Problems in the use of these words arise when the social sciences, as we have seen that they do with other words, appropriate them for the purposes of doing social science. Since social science descriptions of what is really going on in the social world are often at odds with descriptions that people might give of their own doings, and at odds with each other, some way of *legitimising* those descriptions has been sought. Initially this was done through the use of positivist methodologies, which were deemed to be 'objective' in mimicking the natural sciences, but as these began to

be questioned ideas associated with the view that reality is socially constructed, and thus available to reflexive inquiry, came to the forefront. Although they can be juxtaposed against one another, neither positivistic objectivity nor constructionist reflexivity guarantees insight into social arrangements. As Lynch writes,

> … attempting to *be* reflexive takes one no closer to a central source of illumination than attempting to *be* objective … *Ordinary* and *occasional* virtues and difficulties can be ascribed to thinking about what one is doing … but reflexivity *in general* offers no guarantee of insight or revelation. (Lynch 2000)

Egon Bittner, writing some time ago in 1973 elaborates difficulties in both the positivist position and reactions to it, providing substance for Lynch's later remarks.

> For many years … strict compliance with certain canons of objectivity alone guaranteed the attainment of all objectives of rational inquiry. Clearly this is no longer the prevailing view … Quite the contrary, in some quarters objectivity has fallen into ill repute and is explicitly denounced … even where the criteria of objectivity are adhered to … much less is made of it than used to be the case. But neither contempt nor neglect will make the problem of objectivity disappear and sociologists cannot – must not – divest themselves of the responsibility for rendering an accounting of the way in which they try to do justice to the realities they study. (Bittner 1973)

Now Bittner's insistence could be taken as an invitation not only to sociologists but to other social scientists as well to engage in academic reflexivity: to render an account of the ways in which *we* try to do justice to the social realities that *we* study. The reader might take the invitation to be underscored given Bittner's critique of the "naïve realism" built into the positivist paradigm that was dominant in the 1960s, which essentially sought to impose a model of natural scientific inquiry on the study of the social and which lives on today in quantitative modes of inquiry.

However, on closer inspection Bittner is not simply arguing that what makes positivism naively realistic is a misplaced analytic commitment to the rationality of natural science, but the unexplicated predication of the objective study of society on what *anyone knows about it*.

> … naïve realism … entails the belief that the knowledge normally competent, wide awake adults have of the world around them, about the society in the midst of which they live, and concerning human affairs *is*, despite its ambiguity, uncertainty, and incompleteness, an adequate beginning point for more systematic study aimed at the removal of these inadequacies … … … Although … the proverbial man on the street has motives in seeking information that differ substantially from the motives that move scientific curiosity … naïve realism … meant the unexamined acceptance of the reality of the world of everyday experience as a heuristic fact …

Positivism is naively realistic then not so much because it adopts the model of natural science but because it is based in unexplicated ways on common-sense reasoning (something we have encountered in previous chapters and which we will elaborate on in more depth in Chap. 7). This means that dispensing with the objectivity of natural science does not of itself *dispense with naïve realism* because what anyone knows about society remains the unacknowledged bedrock of interpretive approaches to social science as well as positivistic ones.

Interpretive programs and the idealist critiques that underpin reflexive arguments against objectivity are consequently as problematic as the programs of positivism, as they offer no guarantee that naïve realism will be dispensed with either.

> … the fieldworker … forever confronts 'someone's social reality' … when he dwells on the fact that this reality is to 'them' incontrovertibly real in just the way 'they' perceive it, he knows that to some 'others' it may seem altogether different, and that, in fact, the most impressive feature of the social world is its colorful plurality. Indeed, the more seriously he takes this observation, the more he relies on his sensitivity as an observer who has seen first hand how variously things can be perceived, the less likely he is to perceive those traits of depth, stability, and necessity that people recognise as actually inherent in the circumstances of their existence. Moreover … he renders them in ways that far from being realistic are actually heavily intellectualised constructions that partake more of the character of theoretical formulation than of realistic description.

Bittner's commentary is anything but an invitation to academic reflexivity as a fundamental mode of inquiry and representation. Rather, he problematises positivistic and interpretive approaches in equal measure. You can no more understand someone else's social reality through the imposition of a natural scientific model than you can by rendering theoretical 'formulations' or interpretations of it.

Bittner's is not a critique of objectivity per se then, only of the positivist version of it and interpretive reactions to it. The problem is not one of getting rid of objectivity in social science research, but of figuring out what it might actually amount to.

> … it still remains to be made clear what objectivity in sociology might consist of, if it were to take full account of the objects of social science inquiry in their actually given nature … What then is left for a new start?

Bittner's comments stand the charge of naïve realism levelled at ethnomethodological studies on its head. It is not ethnomethodology that is naïvely realistic, the positivistic and reflexive social sciences are because they build in common-sense knowledge of society as a 'heuristic fact', assuming and using its commonly known features without understanding how those features are themselves brought about as organised features of everyday life. This is a familiar early charge of ethnomethodology against social science. The 'new start' that Bittner refers to involves suspending the common-sense perspective, or 'bracketing' it off in ethnomethodological terminology, to investigate its orderly properties and how members display and use them in their actions and interactions.

Ethnomethodology's intent in making this new start was and is to investigate the knowledge that normally competent people have of the world around them and the society in the midst of which they live. Not abstractly, but concretely in particular settings and in the conduct of particular actions and interactions that make up their affairs. Rather than trade in generic definitions of objectivity and reality, ethnomethodology sought, and continues in this vein, to understand what constitutes objective reality from the perspective of society's members *as they go about their daily business*. The production of praxiological accounts was and is a means of doing this, of describing what the members of the settings we investigate take to be objective features of their lives: the activities that take place in their world; the ways

in which they are done and reflexively organised; the taken for granted knowledge the doing and organising relies upon; the schemes of interpretation this knowledge provides for; and the meaning those schemes of interpretation enable members to ascribe to society as an objectively accountable feature of *their social reality*. Objectivity and reality are then matters for members, just as reflexivity is a members' matter.

There are, of course, ethnographers working in an interpretive tradition that object strongly to the suggestion that interpretive forms of ethnography are no better equipped to provide empirical insight into social reality and the organisation of everyday life than positivistic approaches. Blomberg and Karasti (2013), for example, insist that interpretive approaches are just as capable as an ethnomethodological approach at understanding social reality as it is practically encountered and practically organised by the parties to it:

> … we do contest Crabtree et al.'s implication that interpretive ethnography is not focused on 'detailed empirical studies of what people do and how they organise action and interaction in particular settings'. To the contrary, as Geertz argues, ethnography is always tied to the details of the lived experiences of the people studied … … … we do not concur [then] with those who suggest 'new' … ethnographic approaches do not provide a valuable contribution to CSCW … On the contrary, we believe as the saying goes 'the proof is in the eating' …

Blomberg and Karasti's comments are not confined to Computer Supported Cooperative Work but apply to ethnography and systems design more generally. Their response to the kinds of arguments we have made about interpretive approaches is, however, itself problematic in two key respects.

First, and to be clear, Geertz does indeed argue that ethnography is always tied to the details of the lived experiences of the people studied:

> If anthropological interpretation is constructing a reading of what happens, then to divorce it from what happens – from what, in this time or that place, specific people say, what they do, what is done to them, from the whole vast business of the world – is to divorce it from its applications and render it vacant. A good interpretation of anything – a poem, a person, a history, a ritual, an institution, a society – takes us into the heart of that of which it is the interpretation. (Geertz 1973)

However, and as noted in Chaps. 2 and 3, Geertz *also* points out that the locus of an interpretive ethnographic study "is not the object of study". The object of study for Geertz is the imputed structural forces at play on people in the situations they find themselves. What people do in their situated actions is, then, a platform from which to view structural forces at work. Conflating Geertz with ethnomethodology's interests in empirical studies of situated action not only misunderstands ethnomethodology, it also misunderstands Geertz. Ethnomethodological studies treat situated action as a *topic* of investigation and focus on the explication of the orderliness of action and interaction as it is achieved by a setting's members. Interpretive ethnography on the other hand focuses on the interpretation of situated action and thus on how it is perceived as a *resource* for thinking "creatively and imaginatively" about the "mega-concepts" in social science (ibid.) – i.e., as a resource for sociological theorising. As Garfinkel and Wieder (1992) make clear the two are incommensurate

and irreconcilable, which makes Blomberg's and Karasti's comments difficult to understand (unless of course they have not understood Garfinkel in the first place).

It is, perhaps, to counter our objections to interpretive ethnography that Blomberg and Karasti hold out what looks like an olive branch:

> As Randall et al. (2001) argue, '… what justification we have for arguing that any particular thing is 'going on' should be evident in the data and open for inspection.' Perhaps on this point ethnomethodological and interpretive ethnography can agree.

The evidential nature of social studies of course is something we agree with, just as we agreed with Dourish and Bell's (2011) emphasis on analysis. However, these particular calls place analysis in the hands of the social scientist, and (as discussed in detail in Chap. 4) the documentary methods of interpretation they use to evidence their claims. The social world that the social scientist seeks to analyse is itself reflexively produced in the analyses that members are doing of it as an ongoing and account-able feature of their actions and interactions. The reflexivity built into action and interaction contrasts with the reflexive way of seeing the world and accounting for its organisation that is manifest in interpretive anthropology. Academic reflexivity is not an analytic approach that elaborates how someone else's social reality is seen and recognised as an objective order of affairs constituted *in* everyday action and interaction. The alternative is to understand objectivity and reality as these matters are turned in member's accounts and the reflexive constitution of the social occurrences, scenes and events that make up and shape *their* world. Objectivity and reality in the social sciences has little to do with this, however, but rather with how these matters can be turned for the doing of social science. The distinction her is not theoretical but an easily – indeed an absurdly easily – visible matter: the objective world and social reality are manifest in member's (not social analyst's) actions and interactions. In the next chapter we examine how people interactionally constitute as an objective matter just *what* is going on in their actions and interactions, and how the 'interactional what' of social action is *missing* from mainstream social science accounts.

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