

For



# Ethnography

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## Accounts and narratives

A great deal of qualitative research is, of necessity, based on the collection and interpretation of language materials. That is because despite the apparent emphasis on participant observation, even research that calls itself ethnographic depends rather little on either participation or observation. Instead, what is attended to is conversational material collected in the field. Even more strikingly, qualitative research in general is widely equated with the collection and analysis of interview data. The fact that encounters are sometimes given the title of ‘the ethnographic interview’ (e.g. Spradley 1979) does not in itself make them any more authentically ethnographic than any other kinds of interview.

In other words, a great deal of information that is gathered in the course of field research consists of talk. Now it will be clear throughout this book that I disapprove most strongly of a complete reliance on interviews and the kinds of data that they yield. They provide little or no opportunity to investigate the multiple forms of social organisation and action that are the stuff of everyday life. They yield information (of sorts) in a vacuum, bereft of the sensory and material means of mundane reality. They furnish no opportunity to study the techniques and skills that social actors deploy in the course of their daily lives, or in accomplishing specialised tasks. Equally, however, I do not subscribe to the view that such spoken materials are entirely inappropriate forms of data. But they do not substitute for other sources of ethnographic analysis.

The crucial issue is the *analytic* use that is made of such spoken materials, and that in turn is predicated on what ethnographic attitude one adopts towards them. In part, the themes of this chapter re-visit the hoary sociological question concerning the appropriate relationship between participant observation and interviewing. As I shall go on to suggest, however, the real analytic issues go far beyond the treatment of interviews and conversations. It also involves the proper analysis of spoken actions *in situ*. Ethnographers really do need to pay sustained and systematic attention to the use that is made of language in naturally

occurring settings. This latter topic is not simply or primarily a matter of spoken interaction in the form of face-to-face speech – in the form of conversation – but of the use of a wide variety of speech acts and performances. These include accounts, myths, narratives, religious pronouncements, political speeches, lectures, scientific demonstrations. The list is indeed virtually endless – and therein lies the ethnographic richness. Some of the examples will be collected via interviews, others will be collected through participant observation and participant recording. It matters less how the data are derived; it matters a great deal how they are addressed and analysed.

It is the recurrent theme of this book that the methods of social life need to be analysed in terms of their intrinsic properties and organisation. We also need not shy away from the fact that social actions also have functions. In other words, and echoing classic formulations of philosophical speech-act theory, we *do* things with language, and with other means of representation. So when we think about language-use in everyday life, we need to think in terms of both the *forms and functions* of that language. Language-in-use is never a neutral medium of representation. Moreover, language-use always does far more than merely describing or reporting actions, events or feelings. We must always, therefore, have a due regard for the fact that language accomplishes social actions and realities, and that it has its own organising principles. Language-use is always conventional, and those conventions are themselves socially shared phenomena. Unfortunately, whatever their overt assumptions about language in action, far too many qualitative researchers seem oddly insouciant about the properties of language itself. Far too often there is a complete disjuncture between researchers' stated theoretical or epistemological standpoints and their use of language in their social analyses. In practice, we too often find informants' accounts of events, or memories, or descriptions of social action, reproduced as if they were transparent representations.

Yet virtually every theoretical perspective on which qualitative research rests recognises a central and constitutive role for language, and would stress that language-use is one of the most pervasive means of accomplishing social action, self-presentation, consciousness and the like. Symbolic interactionism, phenomenology, ethnomethodology, varieties of constructivism – these all accord language a special (though not exclusive) place in making social persons and social activities. This is something of a paradox, and it is a frustrating feature of contemporary research. Far too much of the work that is described as 'qualitative' relies on extracts taken from spoken accounts – derived from interviews, as I have said – reported as if they were unproblematic and transparent. They

are deployed to illustrate analyses of action, attitudes, values and so on. Little or no account is taken of their intrinsic rhetoric features. Of course, this is not a universal failing, but it is far too common. Ethnographers – alert to the properties of social conventions – should not have any excuse for treating language-use in such naïve ways. On the contrary, many of the best ethnographic opportunities are provided by a sustained, analytic attention to the forms and functions of practical language-use.

I continue with a brief recapitulation of the recurrent debates concerning interview data. In essence, this depends on the competing claims of interviews and direct observation as sources of information about a given social world or social phenomenon. Silverman and I (Atkinson and Silverman 1997) have lamented the almost ubiquitous reliance on interviews among the various communities of qualitative research. Indeed, we noted that in some quarters the very notion of qualitative research (not a term we endorsed there or since, except for purely pragmatic purposes) seems to be equated with various styles of ‘open-ended’, or ‘in-depth’ interviewing. Moreover, there is a tendency even to describe as ethnographic studies that are almost or completely dependent on interviewing. This latter usage seems – for the sorts of reasons I have given already in this book – to be entirely unjustified. We suggested that this undue reliance on interviews as modes of data collection reflected the pervasive cultural phenomenon we called ‘the interview society’. We noted that contemporary society seems to place a particular cultural value on self-revelation, and the construction of accounts of personal feelings, experiences and preferences. We seem to be surrounded, in everyday life, through the mass media in particular, with broadcast and printed interviews through which private experience is translated into a public commodity. The interview-obsessed research community seem to be recapitulating those cultural patterns, often equating the ends of research with the investigation of social actors’ personal, private lives. The interview seems to be a means of penetrating personal experience, giving access to the individual actor’s ‘point of view’.

Now I think that to be a sound argument: there is certainly an over-reliance on interview data, and there are certainly too many publications – methodological and substantive – reflecting an obsession with the contents of experience. But the interview society in its various manifestations should also remind us of a slightly different thing. The interview *is* a cultural phenomenon. It is accomplished through the use of various conventions of language and repertoires of speech act. There are culturally shared kinds of stories and rhetorics of emotion that are expected of certain kinds of interviews. (I shall expand on this point later in this chapter.) So we might need to think about things from a

slightly different perspective. An ethnography of (say) sports performers, artistic performers, or celebrities ought to include an ethnography of the interview itself. It would be one of several, indeed many, kinds of performance and enactment that the relevant social actors would be party to. In its cultural embeddedness, the research interview is in itself an 'indigenous' method that is distinctive to contemporary Western culture. In other words, the interview is a distinctive kind of speech event. It calls on shared assumptions on the part of the interviewer and interviewee. In many contexts, there are preferred kinds of questions and preferred kinds of answers. They appeal to culturally shared frames of reference that hearers or readers will recognise. Interviewees can engage in various kinds of self-presentation and various kinds of rhetorical device in order to do so: they can elicit sympathy, seek self-justification, allocate responsibility and blame, and so on.

So, the interview in everyday life and the interview as part of the research process, are both particular kinds of spoken activity. They embody speech acts and performances. They reflect socially shared conventions of expression. They *construct* experiences, memories and the like. They contain accounts and narratives. They reflect conventional codes of comedy or tragedy. They elicit sympathy or repulsion. They draw on story-types: hard-luck stories, stories of success and failure, stories of troubles overcome, and so on. Now research interviews are deliberately designed occasions on which such performances are enacted, and to that extent there is no question: they do not constitute 'naturally occurring' sources of data. On the other hand, they provide occasions for the performance of events that rarely, if ever, occur under other (natural) circumstances. The life-history interview, for instance, rarely occurs in other social circumstances. Fragments of such biographical narration do occur of course: occupational interviews, medical histories and similar kinds of accounting can contain aspects of the life-history. But they are clearly nothing like the sort of extended encounter(s) through which hours and hours of talk are exchanged, and large tracts of a life are recounted.

So, if we cannot treat interview data as unproblematic forms of representation or reconstruction, then they become especially troublesome, especially if viewed as sources of information about informants' personal 'experience'. As Silverman (1993) points out, we cannot approach interview data simply from the point of view of 'truth' or 'distortion', and we cannot use such data with a view to remedying the incompleteness of observations. By the same token, we cannot rely on our observations in order to correct presumed inaccuracies in interview accounts. On the contrary, interviews generate data that have intrinsic properties of their

own. In essence, we need to treat interviews as generating accounts and performances that have their own properties, and ought to be analysed in accordance with such characteristics. We need, therefore to appreciate that interviews are occasions in which are enacted particular kinds of narratives, and in which 'informants' construct themselves and others as particular kinds of moral agents. Examples here include the analyses of parents' accounts of life with handicapped children as analysed by Margaret Voysey Paun (2006) and of natural scientists' accounts of scientific discoveries by Nigel Gilbert and Michael Mulkey (1984).

When I was studying the Welsh National Opera company (Atkinson 2006) I did conduct interviews with a number of key informants, as well as a great many conversations that occurred in the course of field-work (between rehearsals, in WNO offices, at performances in the theatre), I also collected interviews from a number of individuals who were among the financial supporters of the opera company. I did not treat those interview materials as proxy sources of information about performances or rehearsals. I did, however, recognise that many people associated with the opera have stories and narratives to give, biographical work to undertake, and justifications to offer (for their aesthetic or other operatic commitments). Singers, for instance, draw on distinctive (but by no means unique) repertoires of narrative to account for their careers. Along with other performers, they do, for instance, have all sorts of stories about luck and success. Chance opportunities – lucky breaks – are presented as the origins of successful careers, for instance. These are among the well-rehearsed tropes of career-narratives. Similar accounts can be found in scientists' narratives concerning their scientific discoveries. In both contexts, these kinds of stories enshrine devices that allow the speaker to express her or his success, while allowing for a degree of personal modesty, rather than attributing success to brilliance. Culturally, such a degree of managed modesty is normally preferred; arrogance and self-praise are not. Obviously, this is not a question of accusing singers, scientists or anybody of misrepresentation. It most definitely *is* a matter of applying elementary analytic principles to such spoken materials. There is no merit in simply reporting what was said with no regard to how it was said, and what cultural conventions are deployed.

We should not, therefore, worry about whether 'the informant is telling the truth', if by that one understands the task of the analyst to distinguish factual accuracy from distortion, bias or deception. Similarly, ironic contrasts between what people do and what they say they do become less pressing. Rather, attention is paid to the coherence and plausibility of accounts, to their performative qualities, the repertoires of accounts and moral types that they contain and so on. It would be more fruitful, in

a way, to ask ‘How does the informant know s/he is telling the truth?’ or indeed, ‘How is truth enacted?’ and ‘How are plausible versions of persons and events conveyed?’ Thought of in this way, interviews are approached as a form of social action. This approach to interviewing as action can be illustrated with reference to the topic of memory. One way of thinking about interviews and the data they yield is to think about informants producing descriptions of past events. In part, therefore, the interview is aimed at the elicitation of memories. Viewed from a naïve perspective, it also follows that one of the main problems of this kind of data collection concerns the accuracy or reliability of such recollections. Such a perspective certainly presents pressing problems if one is using the interview to gather information about past events. The same is true of the elicitation of experiences. It is possible to view the interview as a means for the retrieval of informants’ personal experiences, if by that one means a biographically grounded and discursively constructed view of memories and past events. The analytic problems of memory and experience are equivalent from my point of view. It is possible to address memory and experience sociologically, and it is possible to address them through the interview (and through other documents of life). But it is appropriate to do so only if one accepts that memory and experience are social actions in themselves. They are both enacted. Seen from this perspective, memory is not (simply) a matter of individual psychology, and is certainly not only a function of internal mental states. Equally, it is not a private issue. (I am not denying the existence of psychological processes in general, nor the personal qualities and significance of our memories: mine is a methodological argument about the appropriate way of conducting and conceptualising social research.) Memory is a cultural phenomenon, and is to that extent a collective one. What is memorable is a function of the cultural categories that shape what is thinkable and what is not, what is counted as appropriate, what is valued, what is noteworthy and so on. Memory is far from uniquely (auto) biographical. It can reside in material culture, for instance. The deliberate collection or hoarding of memorabilia and souvenirs – photographs, tourist artefacts, family treasures or other bric-a-brac – is one enactment of memory, for instance. Equally, memory is grounded in what is tellable. In many ways the past is a narrative enactment.

Memory and personal experience are narrated. Narrative is a collective, shared cultural resource. As authors such as Plummer (1995) have reminded us, even the most intimate and personal of experiences are constructed through shared narrative formats. The ‘private’ does not escape the ‘public’ categories of narrativity. Just as C. Wright Mills (1940) demonstrated that ‘motive’ should be seen as cultural

and linguistic in character, and not a feature of internal mental states or predispositions, we must recognise that memories and experiences are constructed through the resources of narrative and discourse. A similar perspective has, much more recently, been articulated by Tilly, in suggesting that stories, big and small, can be understood in terms of the ‘Why?’ they implicitly address (Tilly 2006). Narratives and the resources of physical traces, places and things – these are the constituents of biography, memory and experience. When we conduct an interview, then, we are not simply collecting information about non-observable or unobserved actions, or past events, or private experiences. Interviews generate accounts and narratives that are forms of social action in their own right (Lyman and Scott 1968).

Gimlin (2012) provides an instance of how narratives can be treated in a sociological fashion. The substance of these narratives is women’s accounts of cosmetic surgery. She identifies a number of narrative formats that are used to construct distinctive justifications and evaluations of cosmetic surgery. These are culturally shaped accounting mechanisms, not individual constructions alone. They include the trope identified in terms of ‘surgical; otherness’. This form of contrastive rhetoric justifies the speaker’s use of cosmetic surgery by juxtaposing it with extreme usages by others, such as women who are obsessive in their surgical transformations. Gimlin provides insufficient concrete data or detailed analysis to take this as far as she might, but it is a clear indication of how one might treat such accounts *as* accounts, and hence as cultural phenomena. In a similar vein, Taylor and Littleton (2012) treat interview data as accounts, focusing on the discursive resources through which their participants constructed autobiographical narratives. The materials are drawn from three complementary studies of creative workers at different stages in their career, from aspiring novices through to mature practitioners. They explore the discursive repertoires that are drawn on to construct career-narratives, including recurrent tropes that furnish biographical resources. Such career biographies include a repertoire of accounting devices that stress the early emergence of talent or a propensity towards creative interests. The biography is therefore, in part, constructed in terms of the revelation of creative talent – sometimes developed in the home, with supportive parents in childhood. This sense of continuity can be a significant aspect of identity-work, legitimating career-choice and aspirations. Accounts of biographical trajectories and career contingencies emphasise the actuality or possibility of good fortune and a ‘big break’, but also of the importance of continuing hard work. Participants also use a contrastive rhetoric that distinguishes their ‘real’ creative work from more mundane paid employment.



In these analyses of accounts, sociological and anthropological perspectives are informed by narrative analysis and discursive psychology. This is one among many cultural domains for which ethnographers need a sophisticated grasp of a range of analytic issues. At this point I return to the original formulation offered by Becker and Geer (1957). They refer to the study of 'events', arguing that observation provided access to events in a way that interviews cannot. In one sense, that is self-evidently true. We can observe, and we can make permanent recordings of events. On the other hand, we need to ask ourselves what constitutes an *event*. Clearly an event is not merely a string of unrelated moments of behaviour, nor is it devoid of significance. In order to be observable and reportable, events in themselves must have some degree of coherence and internal structure. An event in the social world is not something that happens: it is made to happen. It has a beginning, a middle and an end. It is differentiated from the surrounding stream of activity. Its structure and the observer's capacity to recognise it are essentially narrative in form. In that sense, therefore, a radical distinction between events that are observed and accounts that are narrated starts to become less stark, and the boundary maintenance more difficult to sustain.

Does this mean that we still acknowledge the primacy of particular kinds of social actions? Not necessarily. By acknowledging that accounts, recollections and experiences are enacted, we can start to avoid the strict dualism between 'what people do' and 'what people say'. This is a recurrent topic in the methodological discourse of social science. It rests on the commonplace observation that there may be differences or discrepancies between observed actions and accounts about action. They are different kinds of enactments, certainly, but I argue that the specific dualism that implicitly asserts an authenticity for what people (observably) do and the fallibility of accounts of action is both unhelpful and 'untrue'. By treating both the observed and the narrated as kinds of social action we move beyond such simple articulations, and instead reassert the methodological principle of symmetry. We should therefore bracket the assumption of authenticity, or the 'natural' character of 'naturally occurring' action, and the contrasts that are founded on that implicit dualism. If we recognise that memories, experiences, motives and so on are themselves forms of action, and equally recognise that they, like all mundane activities, are enacted, then we can indeed begin to deal with these issues in a symmetrical, but non-reductionist, way. In other words, it is not necessary to assert the primacy of one form of data over another, nor to assert the primacy of one form of action over another. Equally, a recognition of the performative action of interview talk removes the temptation to deal with such data as if they gave us access to personal or private

‘experiences’. We need, therefore, to divorce the use of the interview from the myth of interiority – the essentially Romantic view of the social actor as a repository of inner feelings and intensely personal recollections. Rather, interviews become equally valid ways of capturing shared cultural understandings and enactments of the social world. Now, in a way, this discussion of a classic dilemma is a diversion, because it leads us to a much more general point. I have now suggested that there is not necessarily any radical difference between an interview account and any other naturally occurring speech event in the field. Consequently, this means that ethnographers really ought to be paying serious attention to precisely those stories, narratives, accounts and spoken performances of all sorts. Moreover, they need to be observed and recorded in order to capture and analyse their formal properties.

Let us continue by considering stories. The world is full of stories. They circulate endlessly, sometimes within small networks of friends and family, sometimes they travel over long distances and assume socially significant proportions. In recent years it has become commonplace to assert that the social world is quintessentially ‘storied’. Narrative is held to be an especially significant way for actors to organise their lives, their experiences, their feelings and so on. So it is. That gives us every reason to study stories, narratives, accounts with every seriousness. Too often, however, narratives and other kinds of spoken performance are celebrated but not analysed. Narratives have form and they have functions. They circulate. They both reflect and shape social relations and social networks. They convey and embody personal reputations. They enshrine morals and other cultural values. They are the vehicles for socialisation, and rumours or gossip are means of social sanction. In other words, stories and similar kinds of spoken activity are far too important to be treated merely as the undifferentiated repositories of actors’ personal experiences.

Most importantly, stories have structure. There is an inherent organisation to them, and through that organisation their social functions are accomplished. In other words, there are socially shared conventions that are used by social actors in order to accomplish a range of activities. Stories therefore create structures of plausibility, and they also enact persuasion, justification, legitimation and the like. Ethnographers, therefore, need to be especially alert to the stories that circulate within social worlds, and to analyse the kinds of work that they perform. The structural properties of personal narrative have been examined over a long period of time by William Labov, and his work is in many ways summarised in his monograph, based on stories of life-and-death experiences (Labov 2013). Linking his ideas explicitly to analyses of

oral epic, Labov points out how thoroughly personal and emotional accounts display recurrent structural properties. Narratives are not streams of consciousness. They are organised in accordance with culturally shared conventions. Labov also discusses the profoundly significant theme of *credibility*, although his treatment of it is limited. As I have already acknowledged, social scientists have long grappled with the issue of whether informants' accounts can be treated as reliable. But the point here is that credibility is a property of narratives themselves. Consequently, the ethnographically productive question is 'How does the informant construct a plausible account?'

Analytically speaking, it is not necessary to believe an account in order to appreciate its formal properties of plausibility. I have had occasion recently to examine some published stories from victims of alien abduction. There are many individuals who recount the experience of being extracted in some way from their everyday life, a period of lost time during which alien beings sequester them, and physical evidence of aliens' inspection or invasion of their body. Alien abductions provide rather useful exemplars for analytic purposes. They are recounted by people who clearly feel the reported experiences deeply, and those experiences are extremely personal. But that does not mean that they do not display characteristic, conventional features. It would be wrong to suggest that these accounts are simply incredible because they confound our everyday understandings of what is possible. They provide an excellent example of the truth-telling issue. As I have already suggested, the old chestnut of 'How do you know if your informant is telling the truth?' can more fruitfully be replaced with 'How does your informant know s/he is telling the truth?', or even 'How does your informant tell plausible stories?'. The latter depends on no adjudication of truth by the researcher, but they do open up more productive avenues of inquiry. This is not to imply, incidentally, that lying or self-delusion or boasting do not take place. Of course they do, and there is an anthropological literature on lying and deceit (Barnes 1994). There are many cultural settings where lying, in one form or another, is highly valued in itself. Equally, therefore, we can make such deceptive acts into topics of inquiry in their own right (e.g. Morrow 2013), but that does not absolve us from approaching such materials from an analytic perspective.

Typically, the abduction stories implicitly address their own credibility in that they begin with a narrative that establishes the normality of events prior to the abduction itself. Tellers are engaged in unremarkable, ordinary activity. The story is also framed, however, in accordance with narrative conventions concerning the foreshadowing of strange events – lonely roads, bad weather, darkness, waking up in the middle

of the night. These narrative forms represent what Labov would call the Orientation phases of the story: they establish the context as well as the *dramatis personae*, as well as the premonition that something newsworthy, remarkable is about to take place. The narrative thus posits a trajectory of normal time, which is then punctuated by a period of abnormal time. In narrative terms, therefore, the actual or presumed abduction stands in an equivalent position to religious conversions and similar extraordinary occurrences, in which the mundane is punctured by a sudden reversal and an intrusion of the supernatural. The central action – what Labov would call the Complication – is framed as inexplicable, defying the normality that surrounds it. Complication is compounded with yet further complications, when the abductee seeks for evidence and confirmation – often found in physical marks on the body, or reconstructions of the interrupted journey, and checks on the lost time that elapsed. The extraordinary is thus bolstered with further demonstrations of normality and evidential accounting. By no means all abduction stories are the same, but they often display common structural properties and narrative themes. Such stories illustrate one of Labov's observations: that what is reportable as newsworthy contrasts with what is so ordinary as not to be worthy of narration. In abduction stories, the contrast between the mundane and the extraordinary is central to the accounting process. The stories are structured around a series of implied contrasts: ordinary *versus* extraordinary; darkness *versus* light; ordinary time *versus* lost time; mundane explanation *versus* the inexplicable.

It will be apparent that abduction stories are not unique in their components. In some ways they closely mirror that widespread genre of stories collectively referred to as *urban legends*. The latter differ in terms of their narrative framings, however. Abductions are narrated by people who experience them, while urban legends are recounted as second- or third-hand accounts (the events having happened to a friend-of-a-friend). As I have already hinted, they also have common properties with stories of religious conversion and miraculous intervention. All of these accounts *can* be deeply felt and regarded as entirely factual by their tellers. Equally, they can be recounted with a degree of caution or even scepticism. Their narrative structure does not depend on the degree of credence vested in them. Narratives are not the means for researchers to gain access to informants' personal experience. The task is rather to understand how experience is framed, constructed, shared and transmitted.

Mythic structures are pervasive in narratives and stories that reconstruct the past. While it is not in itself an example of ethnographic fieldwork, a recent monograph on the legends surrounding John

Coltrane's recording of 'A Love Supreme' provides an especially telling example. That recording is, in several senses, 'legendary' in jazz circles. And as Whyton (2013) demonstrates, those legends capture a series of oppositions in myth-making: the music was fully devised, the music was improvised; the recorded version was direct and spontaneous, there was over-dubbing of Coltrane's own voice; it was a unique performance, it was recorded in a different version again the following day. The contrasts that are embedded in the legends of the recorded music, Whyton argues, allow its hearers to frame an appreciation of the work and of jazz more generally through a mythic resolution of paradoxes and dualisms.

Equally, we can recognise that there are distinctive types or *genres* of narrative. Abductions and religious experiences can clearly be grouped in terms of revelation or conversion stories. In many settings we can identify a variety of stories about professional or personal reputations. For instance, in the course of my own ethnography with the Welsh National Opera Company I spent time with singers – guest artists and members of the company's chorus – before, during and after rehearsal periods. Inevitably, indeed in common with all such work settings, talk would include stories in the form of personal reminiscence or reported events concerning performers, directors, conductors and others in the opera world. These are often moral tales that are used to establish and to share the character of key individuals. For instance, such stories can construct individuals as 'difficult' or 'impossible' people (a category not at all rare in opera and other performing arts, but equally known in many other occupational settings). The story was told of a well-known American soprano who was known to be so 'difficult' that companies were unwilling to engage her. It told of the singer complaining that the limo sent to pick her up from her hotel and to take her to the theatre was late. She was sitting in the back of the car, and phoning her agent, ordering the latter to phone the limo company to instruct the driver to drive faster (rather than deigning to speak directly to the driver herself), only to be reminded that the only reason she was running late was because she had sent away the first limo that had arrived – on time – because it was too small, although she was travelling alone. This was just one anecdote told about the pathologically picky behaviour of this particular artist, told so as to construct her behaviour as manifestly unreasonable through implied contrasts with what was expectably normal behaviours. These kinds of stories have distinctive functions in the working context of the opera company: they portray the stereotypical *diva* as an alien and monstrous character, implicitly compared to ordinary, hard-working singers and musicians.

Some individuals can be assembled through series of anecdotes that mingle respect, admiration and a hint of terror. The late Sir Charles Mackerras was just such a character among members of the opera company. He was always regarded with great respect and even affection. In the course of my fieldwork I was struck on more than one occasion that when Sir Charles arrived in the rehearsal studio or in the orchestra pit in the theatre, in the course of rehearsals, he could – apparently by just his charismatic presence – inject a sense of urgency and a lifting of the performers' collective spirits. He also enjoyed a distinctive reputation as a conductor who mingled the authority of his musicological knowledge with an intensely practical approach to rehearsal and performance. He was also a demanding music director. Consequently there were repeated stories of Sir Charles's stern criticisms of singers. Characteristic comments were reported: 'Yes, this *could* be very good. It isn't any good at the moment. But it could be good.' One singer reported Sir Charles whispering detailed criticisms to singers even while they were hand-in-hand bowing and taking their curtain-calls in the theatre after a performance: 'Would you take a closer look at bar one hundred and thirty-two, please?'

Likewise, there were multiple stories about opera directors. I was told that one internationally famous director stormed out of rehearsals when he discovered that he could not have a performer throw himself off a high place on the set without a safety harness and health-and-safety approval. Or another director who discovered that a spiral staircase that had been specified in the design of the opera had been constructed spiralling in the wrong direction, and as a consequence he had locked himself in his hotel room, refusing for some time to come out. Yet another reportedly shouted at the leading lady 'Why don't you just go shopping, and get yourself a brain?' (These stories did not relate to any of the directors I actually observed.) These and similar stories were part of the collective folk-memory of the opera company. They were exemplars of a genre that is found in many organisational settings: they are the sort of *cautionary tale* that enshrines key values and breaches of occupational morality. They are akin to the occupational *atrocities stories* that Dingwall (1977) documented. They are also reputational stories, used to key newcomers (like myself) into a distinctive aspect of occupational culture, and to reinforce shared understandings of significant figures in the professional field. Stories like this circulate within occupational networks and organisations.

Here, of course, we have moved from narratives collected in the course of interviews to those collected as they arise more spontaneously in the course of ethnographic fieldwork. We need to note that this is one of the key modalities of everyday cultural performance, and these narratives

are themselves a significant type of ethnographic data. Ethnographers therefore need to pay analytic attention to several things simultaneously. They need to be alert to the circulation of stories: who tells them to what audiences, their paths of transmission. They need to examine the social functions that stories are used to perform: enculturation, blame, sanction and so on. They also need to examine them for their formal properties: how they are constructed, how they represent events, how they construct actors and how they embody tellers' evaluations of the reported activity. These call for sustained analysis. It is, in other words, of little value merely to collect and to reproduce narratives, and to offer them as exemplars of 'experience'. We need, as researchers, to demonstrate *how* they are enacted, *how* they are shared, and *what* they accomplish in context. Moreover, experience is something that is itself constructed through acts of memory and telling. By recounting narratives of one's own past, or by passing on stories from elsewhere, the teller establishes her or his own stock of occupational and personal experiences.

Analytic attention therefore calls for an understanding of how narratives and accounts perform distinctive kinds of *speech acts*. We have already seen that such accounts – whether they are derived from interviews or from naturally occurring events in the field – accomplish things. If people 'do things with words', then they also 'do things with narratives'. People construct complaints, confessions, boasts – indeed the entire gamut of possible activities. These are all accomplishments, performed through the use of social and discursive conventions. Amongst other things, therefore, it is as important to know *how* an event is transformed into an experience, or *how* it is memorialised in personal narratives and biographical accounts, as it is to know *that* somebody reports an experience or a memory. Indeed, it is more important to do so: the conventions and techniques of everyday rhetoric are socially shared. They are the cultural resources that make possible a shared social world of motives, biographies and justifications that are the stuff of a *sociological* analysis. Equally, of course, forgetting or the suppression of memory can be an important function in the construction and circulation of stories (cf. Blenkinsop 2013).

From a complementary perspective we can see how biographies, lives or identities can be fashioned through narrative means, and through more specific speech acts. Life-history interviewing, and the collection of life-histories within a broader ethnographic context do not merely document a life that pre-exists independently of its telling. Recent intellectual interest in biography and autobiography has contributed to an undue emphasis on issues of personal experience, and of the individual social actors. What can get lost in such a

perspective is the existence of narrative conventions through which the (auto)biography is assembled, narrative coherence is accomplished, turning-points are portrayed and events are assembled. We need to analyse how narrators justify themselves and their actions, how they formulate their evaluations of others, and thus how lives are matters of celebration or regret, as tragedies or comedies of manners, as stories of victimhood or triumph.

In other words, the very notion of a life or a biography implies a narrative form. Lives, temporal markers of change and development, and the unfolding of narratives is, therefore, a fundamentally important set of ideas for the contemporary ethnographer. What it does *not* mean is that we should simply take such things as given, as unmediated phenomena. Too often, ethnographers and other qualitative researchers assume that they are dealing with social actors whose lives and identities are relatively stable. Now this is, of course, something of a paradox, since the underlying assumptions of interpretative or interactionist social research is that lives and identities are fluid and processual. Hence, if we take seriously that latter tenet, then we need to ask ourselves – ethnographically speaking – just *how* such fluid identities get accomplished. There is no single answer to that deceptively simple question. Clearly, they are emergent properties of interactions and chains of interactions. They are also accomplished through performances whereby they are collaboratively worked on by actors and their immediate collaborators. So, if we routinely collect and co-construct with our research participants documents of life then we certainly cannot think of those materials as speaking for themselves. Indeed, thinking that any data speak for themselves has several consequences. First, it means that we absolve ourselves of the need actually to do anything with those data, encouraging under-analysed studies. Secondly, it robs us of the intellectual work – the fun even – of finding challenging and innovative ways of undertaking a thorough analysis.

Here I have been alluding to the long-standing sociological interest in the nature of accounts and social actors' accounting methods. In itself, this is not especially startling. In essence, this standpoint implies that accounts are always constructed from a particular point of view. Moreover, they are always constructed from culturally available resources. Consequently, they are simultaneously social constructions in their own right and they are the means whereby social construction gets done. Accounts, therefore, do not merely report events. They shape them, they *make them into* events. They construct – for the teller and for the hearer (or reader) – what is tellable and what may be newsworthy. They provide evaluative and moral frames within which to place the account.



They position the teller of the account as a particular kind of person, and give them a standpoint or a distinctive perspective. Moreover, accounts are constructed in accordance with narrative conventions and from culturally shared resources.

Accounting in action can be witnessed vividly in the professional work of *making a case*. We know that it is the task of the ethnographer constantly to ask herself 'What is this a case of?'. This can equally be a practical matter for the social actors themselves. The construction of accounts and similar kinds of speech events is a professional matter, for instance, for social workers, the police, lawyers and medical practitioners. They all need to take whatever 'evidence' is to hand, and to transform it into the sort of account that constitutes a 'case'. Hence, professional workers are often engaged in what is essentially *narrative* work. A pertinent case is furnished by Gathings and Parrotta (2013), who document lawyers' constructions of courtroom narratives about their clients, in order to secure leniency in sentencing. These narratives are constructed in accordance with normal expectations concerning social worth, and consequently they reproduce normative assumptions concerning gendered social performance. So, it is argued, men are constructed as good workers and good providers, while women are narrated as good mothers and carers. Men are victims, women are dependent. Such professional narratives are part of the lawyers' stock-in-trade, and they help to create institutional realities of identity-work, labelling and sentencing in the criminal justice system.

I and others have frequently commented on such narrative case-construction on the part of medical practitioners. Hunter (1991) provides a valuable point of reference. She documents how profoundly medicine is predicated on narrative competence. Especially in settings such as major hospitals, there are multiple occasions on which medical practitioners and students are called upon to summarise and 'present' patients as cases. There are shared formats for such cases, and there are many occasions, of varying degrees of formality, at which they are presented to colleagues and superiors. Moreover, the construction and delivery of such a case calls for rhetorical skills, and individuals are evaluated on the basis of their performances and their narrative competence. These skills are acquired, and they are differentially distributed. Equally importantly, they are among the means whereby medical knowledge is constructed and shared (Atkinson 1995).

Consequently, we as ethnographers need to pay sustained, analytic attention to the nature of accounts and narratives, whether they derive from interviews or are recorded in more 'natural' circumstances. We need to address *inter alia*:

- How are accounts constructed: what are their formal properties?
- What accounting devices are deployed?
- What functions do they perform?
- How do they construct or portray their teller(s)?
- How are they constructed from cultural conventions?
- When and how are they recounted?
- How do they circulate?

If we take such issues seriously, then we do not need to base our ethnographic understanding on crude distinctions between speech and reported events. We can treat narratives and accounts as events, or as speech-acts, in their own right. This does mean that we should not merely reproduce extracts of speech as if they were straightforward representations devoid of social convention or function.

Forms of talk are, therefore, embedded in the social worlds in which we undertake our fieldwork. They are not sources of unmediated access to individual persons' interior lives and experiences. They are as much cultural artefacts as a pot or a carpet. We need to pay analytic attention to their construction, reception, and their circulation. They have form and function that are thoroughly dependent on shared cultural conventions. It may, perhaps, not be necessary for all would-be ethnographers to become fully expert in the analysis of documents of life and personal narratives. Equally, however, it is simply not good enough to treat such materials unproblematically. There is currently a vast amount of published research that is, by any criteria, scientifically unsound. By neglecting the internal organisation of spoken activities, and by taking the content of narratives or accounts at face value, researchers manifestly fail to commit themselves to an adequate level of analysis.