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Paul Atkinson is Emeritus Professor of Sociology at Cardiff University, a Fellow of the Academy of Social Sciences, a Fellow of the Learned Society of Wales, and a Fellow of the Institute of Welsh Affairs. Recent books include *Creating Conditions* (with Katie Featherstone), *Everyday Arias: An Operatic Ethnography* and *Contours of Culture* (with Sara Delamont and William Housley). He is currently conducting ethnographic work in art-makers' studios. He and Sara Delamont were the founding editors of *Qualitative Research*, which they edited for fourteen years. Together with Sara Delamont, Amanda Coffey, John Lofland and Lyn Lofland, he edited *The SAGE Handbook of Ethnography*. He will be editor-in-chief, with Sara Delamont, Melissa Hardy and Malcolm Williams, of *The SAGE Encyclopaedia of Research Methods*.

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Introduction

Granular Ethnography

Ethnographic research is not just about the conduct of fieldwork. It also depends on appropriate frames of analysis, and of ideas. This book, therefore, outlines a number of key themes and ideas that ethnographic researchers in sociology, geography, health studies, educational research and other disciplines, might do well to think with. It derives from many years of teaching students, from a variety of disciplinary backgrounds, how to work with qualitative data of different sorts, how to relate their own data to ideas, and *vice versa*. Finding productive exchanges between data and ideas is one of the most difficult aspects of research and of teaching research methods. It is perfectly possible to find highly able researchers who have collected vast amounts of data and have little or no idea of what to do with them. Or who think that what to do with data consists of a set of mechanistic procedures based on *coding* their data thematically, with or without the use of qualitative analysis software. This is compounded when such students (or indeed postdoctoral researchers) have been inculcated with a vulgar version of analysis based on 'grounded theory', that is (wrongly) interpreted as a purely inductive approach to research, by which 'theory' emerges from repeated close readings of the data. That is, I repeat, a vulgar misrepresentation of the original inspiration of grounded theory, but it seems to be widespread. Students can then be disappointed. No matter how hard they stare at the texts of their data (fieldnotes, interview transcripts, narratives, documentary sources) nothing seems to happen. They find themselves just recapitulating some of the most obvious 'themes' in a way that approximates to a rather naïve form of content-analysis. As a consequence, we find doctoral theses and published works that just stitch together some more-or-less interesting snippets of interview transcripts or (more rarely) field observations and transcripts of naturally occurring data. And so

those data remain horribly under-analysed. The resulting papers or theses are jejune at best. Brilliant fieldwork can, in consequence, give rise to publications that are flat and lack analytic bite. Too often apparently random gobbets of data sprinkle the text, undigested. Sometimes we are told that such fragments can 'speak for themselves', when they do not. In the absence of clear and sustained analysis, informed by the disciplines of the social sciences, nothing can speak for itself. In the absence of ideas, research is pointless. Again, qualitative research is sometimes portrayed as if it were self-justifying: more humane, more liberal, more responsive than other kinds of social investigation (for which 'positivist' or 'scientistic' are used as derogatory terms). But there is nothing inherently superior about a particular research strategy unless it is accompanied by appropriate ways of thinking. Ethnographic fieldwork *can* be especially illuminating, and it *can* be especially ethical. It *can* be the research approach of choice (Atkinson 2014). But in and of itself it is no guarantee of anything. Being qualitative does not ensure quality of outcome. In the absence of theory it has no intrinsic value. Now 'theory' can seem a bit daunting, and is readily interpreted to mean some grandiose, impenetrable rhetoric that derives from elsewhere (often Paris). But 'theory' means 'ideas'. We need generic thinking that encapsulates ideas about how social life is ordered, structured and patterned; how social actors are formed and how they interact with one another.

In the course of this book, therefore, I shall *not* be recapitulating methodological advice that is essentially about the conduct of ethnographic fieldwork *per se*. There are plenty of texts that do so. My emphasis will be on the quality and content of productive thinking that can and should inform ethnographic work. In the course of those discussions, I use the term *ethnography* to refer to a broad approach to exploration and analysis that is based on participant observation and the direct engagement of the researcher with the chosen research setting. It is not a single 'method', and as we shall see, it implies far more than just a strategy for data collection. I shall often (though perhaps inconsistently) use the less fashionable *fieldwork* to refer to the concrete activity of participation, conversation and observation that is the foundation of ethnographic research. Of course, these are arbitrary terms and distinctions. Some scholars now want to abandon the terminology of 'ethnography' (e.g. Ingold 2014), because it has become over-used. One understands the spirit of that complaint, not least because the word is often attached to research that is barely, if at all, ethnographic. It is sometimes used as if it covered virtually any research that is 'qualitative' in character. But it is too late for Ingold's querulous argument. The terminology is well embedded in our disciplinary subcultures, and it will have to do. It is

certainly not the preserve of anthropology, and it is not the prerogative of anthropologists to license ethnographic usage.

The logic of ideas

This book, therefore, is by no means a how-to-do-it book on the collection of qualitative data. There are many textbooks and handbooks that provide guidance on things like the conduct of ethnographic fieldwork, the processes involved in extended qualitative interviewing, or the management of focus groups. There are, moreover, many textbooks on the management of such data. There has, for instance, been something of a cottage industry in textbooks on the derivation of grounded theory in the tradition of Glaser and Strauss (1967). Authors like Charmaz (2014) have promoted grounded theory with great success. Now this is not the place to engage with a sustained debate on the value of grounded-theory procedures, and while Charmaz, and Clarke (2005), advocate sophisticated versions of the perspective, there are too many over-simplified versions in circulation (Atkinson 2014). The rationale for this book is that models of the analytic process – and they include grounded theory strategies – are idle in the absence of useful and productive *ideas*. The traditions that underpin all approaches to qualitative research are fundamentally concerned with the relationship between generic ideas and local manifestations. The basic logic – if one really has to have an epistemological justification – is what the pragmatists called *abductive* reasoning. And this is the fundamental idea underlying the original formulation of grounded theory in 1967. In essence, abductive perspectives mean that whenever we encounter an observation, an event, a record, we ask ourselves 'What might this be a case of?'. In other words, we search for some larger class of phenomena that it might represent or illuminate, some more generic process that might give rise to a particular instance, or some underlying pattern that might give rise to our observation. The generic idea has been outlined most recently by Tavory and Timmermans (2014). Now clearly there is a process of speculation in this approach, and ideas are constantly refined and developed in the course of such analysis. But it definitely means that one cannot proceed very far without a repertoire of ideas that can at least form a starting point for fruitful exploration. Ideas do not 'emerge' solely from close inspection of the data. One can read fieldnotes or transcripts until one is blue in the face, but with no ideas in one's head, the results will be nugatory.

In the real world of research – messy and inconclusive as it is – pure logic does not govern our activities. We make the most of what we have,

and make the most of what we can do. We use our native wit to work with whatever data are to hand. It seems to matter little whether the processes are described as grounded theory, or abduction, or extended case method, or analytic induction. In the real world, we strive to make sense of social worlds, social scenes, strips of action, personal accounts, documents of life and so on. We play ideas off against our data and our experience of a chosen research field. At the same time, however, we conduct such worldly research on the basis of disciplinary knowledge. We do not, after all, expect to become natural scientists simply by banging about at a laboratory bench in the hope that some valuable scientific knowledge will 'emerge' from more or less random explorations of materials and equipment. We do not expect to become cosmologists simply by staring vacantly at the sky, with or without a telescope. So when we embark on fieldwork of some sort, we ought to have some idea of what *sort* of social science we are embarking on, and what *sort* of ideas are likely to be productive. We bring ideas *to* the field as well as drawing them *from* our field data and our experiences. There is a constant, iterative process between data and ideas. Just doing fieldwork on the basis of 'exploratory' inquiry is not good enough. Exploring does not mean being directionless. To continue my previous analogy: the great explorers did not make their discoveries by wandering about aimlessly (even if they did get lost and discover territories by accident too).

Consequently, we all need ideas to get us going. Now in many contexts the ideas that I outline in this book will be commonplace and hackneyed, and I certainly enter no claim for originality here. The ideas derive from interactionist sociology, from social and cultural anthropology and from elsewhere. They are part of the stock-in-trade of key disciplines. Yet I make no apology for going over them here. There are now many researchers who are not rooted in the relevant disciplinary traditions. They have not been exposed to the range and depth of literature that will give them the analytic vocabulary I advocate. In the interactionist tradition alone, one cannot assume that they have read and assimilated the key works of Howard Becker, Everett Hughes, Anselm Strauss, Blanche Geer, Virginia Olesen, Gary Alan Fine, Joseph Kotarba, Ken Plummer, Erving Goffman, George Marcus, Martyn Hammersley, Phillip Strong, Sara Delamont, Susie Scott, or Sam Hilliard (to name only a few luminaries past and present). Equally, one cannot assume a broad familiarity with social or cultural anthropology and the work of Edmund Leach, Ruth Behar, Clifford Geertz, Mary Douglas, Marilyn Strathern, or Michael Herzfeld. And one certainly cannot assume that scholars will have read beyond their own disciplinary boundaries. As a consequence, too many people embark on 'qualitative' research with

great enthusiasm but little intellectual preparation. This is a particularly relevant consideration, given that qualitative inquiry is increasingly celebrated and conducted in research fields beyond what one might think of as the 'primary' disciplines. It is now significant in Health and Nursing Studies, in Educational Research, in Cultural and Media Studies, in Cultural Geography, in Gender Studies, in Criminology and Socio-Legal Studies, and a diverse array of applied research contexts such as Market Research. Indeed, qualitative research in general is – notwithstanding some difficulties and some assaults – flourishing on a global scale. The more people expound the need for quantitative training and research, the more graduate students and postdoctoral researchers opt for qualitative research projects for themselves. There seems to be an insatiable demand for books of advice on research methods. They are extraordinarily useful, not least as the sheer scale of qualitative research and of methodological training means that we cannot rely on one-to-one socialisation based on apprenticeship. But they do not in themselves help us to generate useful *ideas*. Too many inexperienced researchers report something like: 'I've collected all these data, and now I don't know what to do with them'. It is too easy just to say to them 'Well, in that case you shouldn't have collected all those data in the first place', because it is too late, and anyway it is a counsel of perfection to suggest that every potential qualitative fieldworker should have a complete grasp of methods and ideas before she or he embarks on practical fieldwork. Consequently, we all need some ideas to get us going and to keep us going, to provide ways into the data, and also ways into the great variety of ideas that might inform a qualitative analysis.

Thinking ethnographically

Now I do not mean to convey the impression that ideas and analysis only enter into the picture once our data have been collected. The logic of ethnographic inquiry means that we are, or should be, *thinking* analytically as we conduct our fieldwork. In the absence of guiding ideas – however embryonic and fluid – we hardly know what to look at and what to look for. Of course, we must not embark on fieldwork thinking we already know what we shall 'find', and merely illustrate ideas that are already fixed. Equally, however, we should not be blundering about trying to absorb and observe everything without any kind of guiding thoughts.

The main idea behind this text is to provide the student and the beginning researcher with analytic ideas through which ethnographic and

other qualitative data may be interrogated. They do *not* constitute a comprehensive catalogue of all possible concepts. Clearly that would be an impossible task anyway. They are also not a potted version of just one approach to sociological or anthropological analysis, although inevitably they do reflect a certain array of perspectives that reflect my own disciplinary and research interests. One of the key values of this sort of collection of key ideas is this: It is too easy for us, as practical researchers, to get caught up in the details of our own projects, and so to develop a somewhat blinkered approach to concepts. We work on, say, education and so find it hard to think outside the confines of educational research, or research on educational institutions. Likewise, research on health or nursing can get fixated on clinical settings to the exclusion of others. So often one can see just how much each could benefit from an acquaintance with the other. The same of course applies to any and all specialised fields of research. The true value of analytic concepts lies in the extent to which they allow us to go beyond those boundaries. Some of the most productive ideas are generic, in that they reflect a diversity of social settings. Put a bit more formally, we need to work with ideas that facilitate comparative perspectives. Indeed, the underlying logic of 'qualitative' field research is one of constant comparison. So when we ask ourselves 'What might this be a case of?' we ought also to be asking ourselves 'And where else might I find comparable cases?'. That does not mean that all of our field research must be comparative, in the sense of studying multiple sites (although it can imply that in some cases), but it does mean drawing on comparative literatures, and working with ideas that have the capacity to develop broader analytic perspectives. It is for this reason that I have, where possible, provided references to published work that has used a given concept, or something analogous to it, in different substantive fields. Again, this cannot be a comprehensive listing of all uses and citations, but it can, I hope, provide some guidance in the right direction. Productive use of this book will probably be developed in conjunction with some creative searching of published literature, tracing citations, and similar kinds of exploratory activity.

In developing this book, it has not been my intention to suggest that everyone has to work with this particular array of topics and ideas. Fieldwork should not be done simply in order to illustrate pre-existing notions, and this is certainly not intended to be the equivalent of flat-packed qualitative data analysis. We should always use ideas to be the equivalent of what Herbert Blumer called 'sensitizing concepts'. That is, ideas that help us to think, that give us perspectives on our research field, that may prove fruitful. In other words, they do not have to be fully formed and tightly, definitively defined. They should give us directions

along which to look, and along which to develop our thinking. Ultimately, of course, we should be using such ideas as preliminary indicators in the course of extending or modifying them. And in an ideal world, we should be developing our own ideas. I do not want researchers to use this book as a collection of recipes that are to be taken off the shelf and followed slavishly. A doctoral thesis or a postdoctoral research project should be conceptually innovative, generating new ideas and not just recycling old ones. But fruitful ideas do not spring from data fully formed. They have to be worked at, and worked with. And they do not arise in a conceptual vacuum: in the absence of any guiding ideas, the average researcher may come up with nothing at all.

Rescuing qualitative research

I want to stress the significance of analytic ideas or concepts because there is a great deal of qualitative research that seems conceptually sterile: for a development of this view, see Atkinson (2014). Again, this partly reflects the sheer popularity of qualitative work across a broad range of fields. A great deal of contemporary work is based on extended, qualitative interviews. Now much valuable data may be gleaned from interviews, and there are many topics that can only be studied through such methods. But then too many 'analyses' just provide us with extracts from those interviews, arranged in terms of 'themes', so that the result is a glorified content analysis of what informants said, interpreted to reflect what they 'felt' or had 'experienced'. The regurgitation of gobbets of data does not constitute thorough analysis. But this shortcoming often reflects an absence of disciplinary and conceptual background, as well as a failure to grasp the full potential of ethnographic or other qualitative work. There is also another widespread phenomenon: many doctoral and other researchers are daunted by the demands of 'theory'. Too often, social theory presents itself as a barrier to productive research and to detailed analysis. In the social sciences – and it is more pronounced in some than in others – there is a requirement to 'use' theory. That is often taken to mean that the student or postdoc ought to align herself or himself with some 'grand' theory or some famous (often European) theorist. Or it can mean basing one's work on one overarching 'theory' (globalisation, fluid modernity, risk society, postmodernism). Students are often asked to provide their 'theoretical framework', as if it were a prosthetic aid – a kind of epistemological Zimmer-frame. Now experience suggests that many people find such requirements unhelpful at best and paralysing at worst. I know I would. It has the sort of stultifying

effect that Glaser and Strauss (1967) were trying to counteract when they proposed their heuristic vocabulary of grounded theory. This is, moreover, the kind of approach that is favoured by teachers of social science, who trade in 'isms' and 'ologies', rather than real researchers, who trade in data and ideas. Academics who specialise in teaching 'theory', which usually means other people's theories, can have a stultifying effect on real social science. The teacher strives for internal consistency, while the researcher strives for productive concepts. The teacher renders all research philosophically impossible, while the researcher tries to make it doable. Methodologists can render research so complicated that students do not know which way to turn. This book is for the researchers. The teachers will have to look after themselves.

That is why I have tried to avoid couching this work in terms of 'theories'. I greatly prefer to write and to think in terms of 'ideas' and 'concepts'. We may not all be – or want to be – theorists in the grand manner. But we can all surely have ideas and work with ideas. We can have ideas about the social phenomena we study, and we can all work with ideas. We can pick up ideas from the literature we read, and we can assimilate ideas to our own research interests. So, data are stuff to think with, and ideas are the tools we use. A volume such as this does not substitute for advice about the proper conduct of social research, and there are many texts that provide such advice. Equally, it should not substitute for proper, in-depth reading of the relevant literatures. We cannot become expert practitioners on the basis of short introductory works like this, any more than we can become confident speakers of a new language on the basis of a phrasebook. But it is my intention that it should be a *vademecum*, an introductory work of reference that can help to point the reader in productive directions. It may be a first point of reference, but never the final word.

The ideas that I outline occupy, for the most part, what has been called the 'middle range'. They are not intended to capture the grand narratives of sociological or anthropological theory. They do not seek to encompass the great sweep of modernity and postmodernity, or of neoliberalism and globalisation, or the totality of social structures and historical processes. Rather, they are the sorts of ideas that help us to make sense of work, institutions, encounters and interactions, of biographies and identities, indeed the entire complexity of everyday life and its practical accomplishment. They are the stuff of field research in hospitals and clinics, schools and colleges, prisons, or workplaces; in streets or paddyfields, theme parks or red-light districts. They help us to document the cultures and interactions of formal and informal groupings, networks, associations and subcultures. These are the kinds

of topics that are and should be the subject matter of contemporary qualitative research.

The various chapters here in no sense describe discrete topics. They are not a toolbox from which one can assemble a comprehensive portrayal of any given social setting. The ideas need to be deployed carefully, in combination with other ideas, some of which may be in this book, others inevitably not. Throughout I have tried to caution against careless use of such concepts. It is far too easy to bandy such ideas about, creating the semblance of sociological or anthropological analysis, without really using or developing them accurately. So the reader will find that from time to time I am to be found cautioning against sloppy or vacuous usage. So I do not think it counts as very penetrating analysis to argue, say, that a given phenomenon is 'socially constructed' and to leave it at that. For that is just the start. What is needed – and what is more difficult – is the clear specification of *how* that is accomplished, by whom, using what resources, with what consequences, and so on. Likewise, simply stating that a certain category of social actors have a 'moral career' is not very exciting in and of itself. It is a truism. What is significant is an analysis of how such careers unfold, how they are shaped and managed by others, what stages and benchmarks are observable, how transformations in selfhood are understood, what the consequences are, whether there can be 'failed' careers, and so on. Such ideas can help us to comprehend the complexities of personhood, and so generate adequately complex descriptions of them.

Some of the key concepts I outline might seem less than fashionable. You may object that 'labelling', to take one example, is rather played out as a concept. Certainly 'labelling theory' has been marginalised in studies of deviance. But I shall try to indicate where such an idea still has the potential to illuminate. In this case I shall want to suggest that studies of labelling and its consequences can certainly illuminate the crystallisation of identities, the amplification of deviance, the construction of moral careers, and other thoroughly collective processes that surround the attribution of deviance, the diagnosis of ill health, the identification of personal problems, and the like. Conversely, there are some ideas that seem, if anything, *too* current. The social sciences are always prone to enthusiasms. Some thinkers get taken up and their ideas are flogged to death. So they should be used sparingly and with precision. I mention here the idea of 'habitus' that seems pervasive in any sociological and anthropological analysis concerned with bodily practices, with socialised competence, with dispositions based on any given social position. As I suggest, it is in danger of becoming one of those catch-all ideas (a bit like 'culture') that explains everything and nothing. There is clearly

nothing wrong in using and developing such an idea, but again, it should be used with precision. It is no good using it as an off-the-peg notion that can be applied mindlessly to all and every social field, without thought as to how it is manifested, how it is acquired, or how it is recognised.

I need to add one major rationale for this book. It is a theme that runs throughout several of my other contributions to methodological literature. I resist the implication that so-called qualitative research is primarily an exercise in the empathetic representation of social actors' and researchers' *personal* feelings, experiences and biographies. It will be apparent that most of the ideas I outline are relevant to the ethnographic exploration of social worlds. That is, analysis based on *fieldwork* of some sort. Fieldwork, in that sense, implies some degree of participation in and observation of everyday life in naturally occurring social settings. It is research about social encounters, occasions, situations, organisations, ceremonials and rituals. It is about collective social processes, of negotiations, performances, secrets and deceptions. Yes, biographies and careers are relevant too, but they must be understood as embedded within such social and cultural frames. They are just as much constructs as anything else that is subject to sociological or anthropological analysis. In other words, my aim is to inform a thoroughly *social* reading of social life, and not a purely *personal* one. Also, I am not among those who see the primary purpose of ethnographic research as the representation of a social world primarily or solely from the actors' points of view. Social actors do, indubitably, have their own perspectives on their own and others' activities, and their everyday actions are based on such interpretative competence. But simply reporting empathetically does *not* exhaust the needs of sociological or anthropological analysis. In addition to and beyond the first-order understandings of social actors themselves, we also need the second-order constructs of social scientists, so that we can construct understandings, based on comparative perspectives, of how everyday life is accomplished, what patterns of social order are possible, how social identities are made and transformed. The everyday actor – or the social actor in her or his everyday frame of mind – has a practical interest in making these things happen, but not the social scientist's analytic impulse.

Another rationale for this book stems from the following, rather jaundiced, observation: there is too much 'thin' qualitative research, including research that flies the flag of ethnography. One reads far too many research papers and monographs that have remarkably little to say about the wonderfully complex ways in which social life is ordered and the extraordinarily diverse arrangements of specific cultures. In fact we often get studies that are barely sociological or anthropological

at all, in that they report the words (rarely the actions) of individuals who – according to the ethnographer's published version – have virtually no social life. That is, they display no particular cultural specificity, and no discernible social forms or principles of organisation. Now adequately thick description of any given social world does not always have to take account of everything described in this book. Clearly we do not need collectively to revert to a sort of standardised cultural inventory as the outcome of our fieldwork. But an adequately 'thick' account of social life really does need to take account of its many layers and codes. Equally, therefore, we need conceptual apparatus that will enable us to make proper sense of such forms and processes. We need to be able to capture the social processes of interaction, the temporal and spatial arrangements, the patterns of embodied action and communication. And consequently, we need to deploy a wide variety of analytic ideas that can at least help us to gain a purchase on a social world (Atkinson, Delamont and Housley 2008).

Granular ethnography

I am, therefore, advocating ethnographic work that is adequately 'thick' in a specific sense. Rather than stretching the idea of thick description too far, I shall use my own label. It is less succinct, perhaps, but it resists glib simplifications. I want to urge the conduct of ethnographic research that is based on *granular analysis*. That is, it traces the grain of everyday life. The grain is given by the naturally occurring forms of social order and cultural forms. The ethnography is, therefore, faithful to the multiple ways in which everyday life is ordered and enacted. It reflects the conventions and codes of culture. It documents just how social actors achieve and perform what they do. Too often, ethnographic fieldwork seems to result in accounts through which the author tells a story. It is often a compelling story. It may be a story of honour among thieves, of young people's resistance to oppressive state apparatuses, of the pride and dignity of old men, of workers' coping mechanisms. But too often we learn precious little about how those and similar activities are actually sustained and accomplished. We 'hear' people's stories, but they are too rarely analysed as narratives and accounts. We learn of gender but too little of how masculinity and femininity are performed. We learn about work, but little of how workers actually perform their tasks, and how they interact with clients or machines. We read about 'art worlds' and cultural activities, but too little of how culture is enacted or how art is actually produced. So we need detailed and sustained *analyses* of how

social life is actually enacted. So the kinds of issues outlined in the rest of this book are intended to provide guidance to the would-be ethnographer, to remind her or him that fieldwork is not an exercise in empathetic commentary on a given social field, nor is just about exposing social injustice, or celebrating a distinctive way of life (important though they may also be). Social worlds have complex layers of order. They have texture. They have their timetables and spatial arrangements. They have their rituals and ceremonials. They are brought into being by actors' use of accounts, narratives and gossip. They have their canons of taste and aesthetics. They have material and sensory contours. Consequently ethnographers need to work with the grain of social worlds. That is, we need to be faithful to these multiple social forms, conventions and modalities. It does not mean, of course, that we can or should analyse everything all the time, or that we should try to incorporate everything into our published reconstructions of social life. It does mean, however, that we should not devote to them little or no attention.

I am not a believer in celebrating competing 'paradigms' in this context. Ideas can and should come from a variety of sources. It really is just too limiting to think that one has to throw in one's lot entirely with, say, phenomenology, or symbolic interactionism, or ethnomethodology, in order to make adequate sense of the social worlds around us. Rejecting everything else in favour of poststructuralism or postmodernism seems equally futile. The world is full of illuminating and productive ideas, and we should learn what we can from wherever we can find inspiration. Admittedly, many of the ideas outlined in the remainder of this book are derived from interactionist sociology, much of it emanating from the United States. Here I use 'interactionism' very broadly, not restricting it to symbolic interactionism. This reflects my own research experience and interests. But it is also testimony to the fecundity of that intellectual tradition, in which data and ideas are constantly intertwined, and where 'theory' is rarely divorced from empirical field research (Atkinson and Housley 2003). That kind of sociology and the discipline of anthropology share one very important feature. (Actually they share more than one.) Both are thoroughly grounded in the personal and intellectual discipline of fieldwork. Interactionists and anthropologists must commit themselves to the demanding task of reconstructing a given life-world, of retrieving the remarkably detailed and extensive cultural resources that social actors use, and documenting the methods they employ in producing orderly social conduct.

Sometimes ignorant critics – people who have not themselves tried to do any sustained empirical research – complain that field research, or ethnography, produces 'mere' descriptions of chosen social worlds.

My response is always much the same: 'What do you mean "mere"?'. Anybody who has actually undertaken such a task will know that it is hard work, and far from easy, to produce an adequate 'description' of a given social process, institution, cultural setting, or whatever. Far from being 'mere' descriptions, our ethnographies reconstruct a given social world, in conceptually rich ways, that uncover the orderliness of everyday life, the skilful work of social actors, the sources of social identity and its threats, the modes of social control, and the varieties of creative innovation that are possible. Or at least, they should do so. They are complex accounts that reveal something of the complexity of the social world. They also simplify that variety, by working with generic concepts that capture and summarise it. They explicitly relate the chosen social setting to broader, context-free ideas. In the last analysis, of course, such descriptive work is the stuff of science. When Crick and Watson published their Nobel-winning paper on DNA, they were offering a 'mere' description of the structure of DNA. The description was a good one, in that it was simple, elegant and accounted for the evidence (based on the work of Rosalind Franklin and Maurice Wilkins). It was hardly trivial, however. Our ethnographic 'descriptions' should display similar qualities: based firmly on the evidence, our accounts should be elegantly simplified, should accommodate the complexity of the phenomena, and be conceptually rich.

So in using the contents of this book, please do not adopt the ideas slavishly. It should, of course, be most productive to ask ourselves 'What might this be a case of?' and then develop novel and productive ideas for ourselves. It is certainly not my intention to encourage the stale recycling of existing ideas. But in the absence of starting points, we are none of us going to derive nourishing and exciting ideas out of thin air. Equally, of course, these ideas do not float in a conceptual vacuum. Not only are they densely related to one another, they also relate to a number of traditions and schools of thought in the social sciences that fundamentally share some common core characteristics. Some of these are alluded to in the contents of this book, and this is not the place to rehearse them in great detail. I have outlined what I take to be the common ground of thought elsewhere (Atkinson 2014). But a brief reminder here is in order in framing the book's contents.

Some key commitments

We need to remind ourselves, and we need to base our fieldwork on, the inherent orderliness of social phenomena. Even though its surface