Methodology: From Paradigms to Paradox

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In my native field, I have noticed an emerging trend for highly politicised analysis, particularly in what has become known as 'critical management studies'. It is a personal preference, but I have lost my appetite for discussions of power and politics. Critical management studies seems to have become a one-stop shop for all things leftist. It also appears to have created a straw man of mainstream management studies. This is not to say that I consider myself a right wing conservative. I don't. My reservation here is that leftist politics should not have a monopoly on all things critical.

An example may help. In the final year of my doctoral programme, my university won a research grant to explore the concept of ecological resilience from various disciplinary perspectives. I was recruited as part of the team. Unexpectedly, my data revealed that small-scale organic farming methods can be more destructive than large-scale non-organic methods. It seemed that economies of scale—in one sense at least—gave rise to ecologies of scale. My paper was rejected on the basis that it 'did not contribute to the message that we want to send'. I was flabbergasted. I knew this sort of thing happened in newsrooms, but at universities?

At the time, I found solace in writers such as Jeffrey Pfeffer, Gerald Salancik, and Karl Weick and, more generally, in what might be considered the proto-critical management discourses of the 1970s. However, unlike their contemporary counterparts (for whom power and

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*politics repeatedly trump other considerations), their intellectual
methods instead prioritised ontology, subtlety, and complexity. And
notably, though by no means explicit, I detected in their work an
analytical sensitivity to paradox. Paradox does not sit easily in
contemporary critical management discourses because it would, in
effect, undermine the ideological proclivities of the movement. And I
suspect an analytical focus on paradox would undermine ideological
convictions found elsewhere in the academy.*

INTRODUCTION

Since the publication of Burrell and Morgan's seminal text on sociological
paradigms in 1979, the framing of social science research methods has
remained largely unchanged. Though illuminating in so many ways, their
thesis has had the effect of entrenching ideological positions (see, e.g.,
Hammersley, 1992, p. 182). If we are to propel our understanding of
human behaviour to new pastures, we need to initiate an analytical shift
away from paradigms. This chapter argues that ethnography represents an
excellent vantage point for both experiencing and understanding paradox.
As part of this discussion I consider why it is that we find paradox so trou-
bling, before presenting a case for its alternative methodological and peda-
gogical potential in a world dominated—both on the left and right—by
linear cause-and-effect ontologies.

I begin by exploring the literature on paradox before conceptualising as
paradoxes several familiar challenges to the ethnographer. These include
the apparent impossibility of internalising an 'exotic' culture while simul-
taneously maintaining professional distance, and the expectation for eth-
nographers to concurrently convey to their subjects both empathy and
honesty. Although similar concerns have been extensively debated under
the rubric of ethics, this is not the intention for this discussion. Instead,
the emphasis here is on both justifying and bolstering the quality and reli-
ability of ethnographic data. To this end, it is argued that paradox must be
celebrated rather than concealed or maligned since it is, for the most part,
representative of social interaction itself.

In a rather curious twist, paradox is paradoxically indicative of method-
ological strength. To illustrate this another way, Alvesson and Deetz
(2000, p. 66, emphasis added) have suggested that 'interpretivists and
others often labelled as "subjective" often have the *better claim to objectiv-
ity* through the way they allow alternative language games and the

possibility of alternative constructions arising from existing communities denying both research community conceptions and preferred methods as privileged and universal.' Since interpretivists (of whom ethnographers are perhaps the most notable given their direct engagement with their subjects) are more sensitive to social constructions and research bias, they ultimately produce more 'objective' data than their positivist counterparts. And this, of course, becomes the definitive methodological paradox.

A Personal Interest in Paradox

As a master's student in the early 2000s, my research focussed on the conceptual parameters of utopia and dystopia. I soon noticed a peculiar quality to the concepts. Although habitually understood as polar opposites, a more nuanced interpretation revealed them not as opposites but as concepts with a tendency to morph into one another. On an academic level, at least, this was to be my first encounter with paradox and this is where my scholarly interest in the concept most likely stems from. From here, however, my growing intellectual curiosity for paradox is closely linked to ethnography; that I became an ethnographer meant that, sooner or later, I would be grappling with the concept. Nothing is quite as it seems when conducting an ethnography. As Holliday (1995, p. 17) reminds us: 'Textbooks on methodology can never quite prepare researchers for the actual experience of doing [ethnographic] fieldwork.' The ethnographer finds herself in this peculiar position of simultaneously belonging and not belonging. It is a sort of limbo or liminal state, as several of the other chapters in this book have illustrated. The point is, of course, paradox is much more noticeable when actively and consciously carrying out an ethnography because you are living, experiencing, participating, *and* observing all at once. The expectations of ethnography force us to pay attention to what is happening rather than simply accepting it without question. As we will see, paradox is endemic to everyday life and this—of course—is the reason it pervades ethnographic experience.

Beyond the boundaries of particular research projects, my reading with attention to paradox has taken me on a more extensive ethnographic journey; the more I read, the more it seems that paradox is unavoidable. It permeates experiences across academic disciplines. This is part of the reason why books such as the one you are reading are so revealing. By sharing knowledge between disciplines these experiences, frustrations, inconsistencies, and contradictions with which each of us is all too familiar on a

personal level are brought into the open where they can be formally acknowledged and—hopefully—better understood.

Some of the questions tackled in this chapter have been previously explored by philosophers. However, I cannot ignore the fact that it is my ethnographic experience that has driven my curiosity for paradox and shaped my conceptual enquiries. Indeed, such an approach affords a fresh vantage point. I wonder how many English language idioms and phrases are based on paradoxes: 'the grass is always greener on the other side,' 'you don't know what you have until it's gone,' 'try to please all and you end up pleasing none,' and so on. There is something alarming about the implication that unschooled wisdom appears to have a better handle on these ironies than does abstract philosophical thought.

Understanding Paradox

The *Oxford English Dictionary* defines paradox as 'a seemingly absurd or contradictory statement or proposition which when investigated may prove to be well founded or true'. The *Oxford Companion to Philosophy* delineates multiple uses of the word but asks ultimately: 'is there any common feature marked by this term?' In response, it suggests that 'part of any feature would be the idea of conflict.' By recourse to synonym, then, paradox refers to a manifestation of contradiction or conflict. For the purposes of this chapter, however, I would like to deemphasise these aspects, not to deny their relevance but to reconstruct paradox as something with unique pedagogical potential. Put simply, in assuming an ideological position (either consciously or implicitly), we automatically open ourselves up to unintentionally lending support to the opposite position. In this sense, we are better off taking steps to distance ourselves from ideology, and incorporating this as part of our methodological framing. There is, perhaps, a lesson here: where we seek to occupy a particular ontological and epistemological position, perhaps we ought to convey to the reader the preventative steps we are taking to ensure such a framing doesn't descend into ideological conviction?

Two final points of caution. First, my intention here is not reductionist. I am not attempting to do for paradox what others have attempted to do, for example, for class (Marx), power (Nietzsche), or pleasure (Freud). Rather I see paradox as a concept with analytical potential across the full

range of scholarly pursuits, irrespective of whether our particular orientation is intellectual, emotional, or ethical. Second, we do well to ask ourselves the following question: is analysis focussed on paradox likely to yield anything different to analysis focussed on dialectics? Certainly, Hegel's notion of the dialectic might legitimately be considered a precursor to the idea that paradox is a central concept to all of life (see, e.g., Singer, 2009, p. 13). Indeed, operationalised under Marx, dialectics described the way in which contradiction elicits progress (see, e.g., Stent, 1978, p. 119). However, this should not suggest that my own observations are little more than a repackaging of Marx. For Marx, contradiction spurred evolution (and this argument has been lent new currency by Harari, 2011). My ethnographic experiences and readings of the experiences of others have demonstrated something else: that contradiction seems to be either ignored or attempts are made to either resolve or dissolve the contradiction. Both reactions, I argue, are problematic. Furthermore, for Stent, 'these conflicts and contradictions are unlikely to be resolved within the context of a western tradition' (ibid., p. 146). My position is notably different: paradox appears to be endemic to the human condition and hence, most likely, *ir*resolvable, irrespective of whether eastern or western traditions are authorised. Indeed, the desire to resolve contradictions reveals our difficulty in comprehending paradox; paradox transcends cause-and-effect ontologies and hence the suggestion it can be resolved loses traction.

Paradoxical Experiences in Ethnography

As Atkinson and Hammersley (1994, p. 256) have previously observed, 'paradox lies at the heart of the ethnographic endeavour and of the ethnography as a textual product.' Contemporary ethnography 'explores the discontinuities, paradoxes, and inconsistencies of culture and action [and does so] not in order to resolve or reconcile those differences' (ibid.). I here expand on this interpretation and in so doing identify ten paradoxes inherent to the ethnographic experience. These are the participant-observer paradox, the familiarisation paradox, the insider-outsider paradox, the honesty paradox, the consensus paradox, the all-too-human paradox, the certainty paradox, the plagiarism paradox, the linguistic construction paradox, and the autoethnographic paradox.

The Participant-Observer Paradox

There is a categorical paradox immanent in ethnography. The implication that the researcher is expected to, concurrently, participate *and* observe is problematic. As Boncori has observed (see Chap. 11), this very essence of ethnography is a paradox—or contradiction—*par excellence*. Barnes too (see Chap. 7) echoes Punch's (2005) concern as regards the ethnographer's capacity for observation when preoccupied with participation: 'One of the key objections to relying upon participant observational data is that it raises the question about how effectively a participant observer can observe the group if they are participating fully.' This dilemma is brought into relief if conceptualised slightly differently: the apparent impossibility of internalising an 'exotic' culture while simultaneously maintaining professional distance. Boggis (see Chap. 5), for example, reports that 'immersing myself within the culture of a community in order to study it, raised tensions in respect of distance and the maintenance of objectivity.' My own experience at Findhorn (see Chap. 2) is noteworthy too in this respect. Prior to my own ethnography at the community, sociologist Carol Riddell had visited Findhorn. However, it would appear that she rapidly 'went native'. In 1991, with the support of Findhorn's own press, she published a book entitled *The Findhorn Community: Creating a Human Identity for the 21st Century*. On the back cover, her biography reads as follows

> Carol Riddell lectured in sociology at Strathclyde and Lancaster Universities until 1978, after which she studied healing, clairvoyance and herbalism. She has lived in the Findhorn Community since 1983 and is a devotee of Sai Baba.

Riddell was, it seems, unable to transcend the paradox; she was unable to internalise an 'exotic' culture while simultaneously maintaining professional distance. Now, there may be many reasons for this. Unlike mine, for example, I am unsure whether or not her first visit to Findhorn was consciously intended as an ethnography. This aside, however, she was apparently unable to reconcile her credentials as a sociologist with her newfound New Age identity. But this begs the question: must we always choose? Intellectual curiosity is, by definition, roused by the unknown. Uncertainty, as Barnes ultimately acknowledges (see Chap. 7), is at its core. All too often, academic researchers *are* expected to choose and it is presumed that

they will select the rational at the expense of the emotional. For futurist, Alvin Toffler:

> Science first gave man a sense of mastery over his environment, and hence over the future. By making the future seem malleable, instead of immutable, it shattered the opiate religions that preached passivity and mysticism. Today, mounting evidence that society is out of control breeds disillusionment with science. In consequence, we witness a… revival of mysticism. Suddenly, astrology is the rage. Zen, yoga, séances, and witchcraft become popular pastimes. Cults form around the search for Dionysian experience, for non-verbal and supposedly non-linear communication. We are told it is more important to 'feel' than to 'think', as though there were a contradiction between the two. Existentialist oracles join Catholic mystics, Jungian psychologists, and Hindu gurus in exalting the mystical and emotional against the scientific and rational. (Toffler, 1970, p. 406)

Toffler describes the difficulty we have in reconciling the emotional and the rational and the extent to which each camp responds to this difficulty by entrenching themselves ideologically. History suggests this appears to be our default response. Of the participation paradox, Jackson (1989, p. 135, cited in Rose, 1990, p. 58) comments that:

> Many of my most valued insights into Kuranko social life have followed from comparable cultivation and imitation of practical skills: hoeing a farm, dancing (as one body), lighting a kerosene lamp properly, weaving a mat, consulting a diviner. To break the habit of using linear communication model for understanding bodily praxis, it is necessary to adopt a methodological strategy of joining in without ulterior motive and literally putting oneself in the place of other persons; inhabiting their world. Participation thus becomes an end in itself rather than a means of gathering closely observed data which will be subject to interpretation elsewhere after the event.

If you genuinely participate you will, in effect, *observe*. Equally, observation can readily be construed as participation, in the sense that the observer 'constructs' the observed. Here we might invoke myriad studies of surveillance or, indeed, the *observer effect* in physics. In sum, participation and observation and not mutually exclusive; for our purposes at least, participation (when conceptualised as an end in itself) is effective 'observation'. By concurrently participating *and* observing; by internalising 'exotic' cultures *while at the same time* maintaining professional distance,

the ethnographer has a unique opportunity and, I argue, a unique duty. Quite simply, it is a question of intuiting balance.

The Familiarisation Paradox

Expertise is typically understood by virtue of familiarity with a subject area. As scholars, perhaps above all else, we are expected to be *familiar*. In ethnography, it is rarely this straightforward. Silverman (2007) points out that ethnography actively seeks out both the mundane in the remarkable and the remarkable in the mundane. Another way of looking at this is to either render the 'exotic' familiar (i.e., to familiarise ourselves with a new culture to understand it from that perspective) or to make the familiar 'exotic' (i.e., to 'defamiliarise' our existing culture to gain a fresh perspective). Bell (1999, p. 21) comments of this process in my native field: '[Some] organizational ethnography involves a process of defamiliarization, through which concepts like "strategy" and "human resource management" are made strange.' The notion of deliberately defamiliarising oneself is, of course, paradoxical, but Hammersley and Atkinson (2007, p. 9) argue that it is necessary 'in an effort to make explicit the presuppositions he or she takes for granted as a culture member'. This paradox of familiarisation is likely part of the broader concern academic ethnographers experience in terms of expertise. As academic ethnographers we are simultaneously expected to be an expert (as befits the expectations of our students or subjects) while at the same time each of us is, at times, doubtful of our own abilities, not least in terms of *in*experience. The notion of *imposter syndrome* therefore takes on an interesting guise under the vicissitudes of ethnography. Do all ethnographers suffer perpetually from imposter syndrome? To complicate matters further, Hammersley and Atkinson (1983, pp. 84–85, as cited in Holliday, 1995, p. 28) have suggested that in many ways the most favourable role for a participant observer to adopt in the early stages of fieldwork is as a 'socially acceptable incompetent'. Rather than present oneself as an expert, which may have the corollary effect of condescension, intentionally presenting oneself as foolish may well be more appropriate. It is probably part of the reason that ethnographers can't help but lie (Fine & Shulman, 2009, p. 193). However, as Vine (2010, p. 646) has commented of the same text, 'this thoroughly disheartening thought is alleviated, at least in part, with the hope that fibs too can be creative': the ethnographer's falsehoods create ethnographic realities. But is this any different outside the experiential

flow of ethnography? No. As Sharon notes in her vignette (see Chap. 14), reflecting on her professional experience as a career coach, 'I am caught up in the notion that I need to be seen as "the expert" rather than a facilitator of meaning, but I can't seem to get past this.'

The Insider-Outsider Paradox

For Rose (1990, p. 10), ethnography represents a 'democratic epistemology' implying that 'the thinking of the ethnographer and those studied inhabit the same historical moment.' Atkinson and Hammersley (1994, p. 256) explain that

> prolonged immersion in 'the field' and the emphasis on participant observation commit the ethnographer to a shared social world. He or she has become a 'stranger' or 'marginal native' in order to embark upon a process of cultural learning that is predicated on a degree of 'surrender' to 'the Other'. The epistemology of participant observation rests on the principle of interaction and the 'reciprocity of perspectives' between social actors. The rhetoric is thus egalitarian: observer and observed as inhabitants of a shared social and cultural field, their respective cultures different but equal, and capable of mutual recognition by virtue of a shared humanity.

Most students of ethnography will be familiar with this 'egalitarian' approach. However, we have a problem. In approaching ethnography in this way, do we prevent ourselves from obtaining an external perspective? Atkinson and Hammersley go on to acknowledge that the classic texts of ethnography often inscribed a distinction between the Author and the Other as a means of securing this external perspective. So which approach is better? To 'talk the talk' of egalitarian rhetoric (in the interests of securing insider status), or to preserve outsider status with the perspective advantages that may bring but risk accusations of superiority? You're damned if you do and you're damned if you don't. Furthermore, for those already considered insiders in one sense or another (by the virtue of skin colour, perhaps, or some other shared demographic) Ganga and Scott (2006, p. 1) identify another complication:

> [T]o a large extent, interviewing within one's own 'cultural' community—as an insider—affords the researcher a degree of social proximity that, paradoxically, increases awareness amongst both researcher and participant of the social divisions that exist between them.

In the sphere of organisational ethnography, Holliday (1995, p. 26) suggests that 'The process of managing one's identity as a researcher—and the more complex schizophrenic identity of researcher-cum-employee—is itself very stressful, involving continual renegotiation.' This is relevant as it demonstrates how the researcher is both insider and outsider simultaneously, and echoes the 'professionally-induced schizophrenia' described by Mascarenhas-Keyes (1987, p. 180). And how uncanny a resemblance does this have to life more generally! Most of us will be accustomed to the experience of the first few months in a new job with a new employer. This schizophrenic positionality is thoroughly familiar. But, even beyond that immersion period, though not necessarily by name, many of us will be aware of the 'pronoun test'. For Rousseau (1998) the pronoun test is acutely relevant to conceptualisations of identity: do employees refer to the organisation for which they work (or are a member) as 'we' or 'us', or as 'they' or 'them'? Or to what degree do participants use both, at different times, depending on how they might feel about the organisation? Certainly, my own experience of working for the University of Suffolk alternates between a desire to belong to it and a desire to distance myself from it. Holliday (ibid.) continues:

> Initial entry to the field can involve 'learning on the job' to be done during the period of fieldwork. Thus, it is possible to be both insider and outsider as a not yet fully fledged member of the organisation. The initial focus of fieldwork is concentrated around learning how to do the task, leaving little room for reflection. Later, when the job is learnt and a position within the firm consolidated, it is possible to take a more detached view of the study setting.

What could be more effective, then? Without even trying, an ethnographer is getting multiple perspectives of her setting simply by virtue of the learning process. Indeed, this interpretation need not be restricted to the context of work. We could easily substitute the business for wider family, community, school, social club, gang, and so on.

I return, once again, to my own experience at Findhorn. I believed that was perceived by my subjects as a 'mainstreamer' in their 'alternative' community. To some extent, this was probably self-consciousness. But what was I to make of the situation? I had read extensively on ethnography and although aware of the diverse approaches within the method, I certainly knew one thing: I didn't want to emulate the colonial tradition of cultural superiority. But I faced a problem. So conscious was I to secure

insider status that I began to denigrate mainstream culture and I did so
with 'born again' vigour. I engaged in what might be described as *ethno-
masochism*. Worse still, I didn't really believe what I was saying, at least not
without qualification (which I withheld). I was, in effect, engaging in the
egalitarian rhetoric Hammersley and Atkinson describe. At the time I felt
dreadful. But in the years that have passed since, I have accepted this. I see
it less as deceptive and more as representative of real life. When introduced
to new people in *any* situation, we rarely take issue with their beliefs. We
search instead for common ground and, in so doing, inevitably compro-
mise—and subconsciously re-evaluate—our own beliefs. My conduct at
Findhorn was no different. In order to secure insider status, I had no
choice but to *Other* the outsider. This felt like a natural response. The
outsider (and her ritual denigration) was essential to securing insider sta-
tus. The two were intertwined. Notably, Cooper and Law (1995, p. 244)
draw on the work of Starobinski to argue that there is a false distinction
between inside and outside:

> inside and outside are not separate places; they refer to a correlative struc-
> ture in which "complicity is mixed with antagonism… No outside would be
> conceivable without an inside fending it off, resisting it, 'reacting' to it."
> (Starobinski, 1975, p. 342)

Later in the same text, they draw on the words of Latour and in so doing
explain that 'the inside and the outside world can reverse into one another
very easily' (Latour, 1985, p. 154, as cited in Cooper & Law, 1995, p. 244).
The field of psychology is especially revealing in this sense. Jackson and
Carter (1985, p. 22) remind us that Lacan rejects the idea of an autono-
mous unitary Self, in favour of a subject mediated by the preexisting world
of the Other. Or as Bowie (1979, p. 135) puts it: 'The subject is made and
re-made in his encounter with the Other.' There is something decidedly
paradoxical about the relationship between the individual and the collec-
tive. The absence of autonomy is posited as an unfillable *lack* at the centre
of our being. Furthermore, in *The Abilene Paradox*, Harvey (1988, p. 96)
reflects on the fact that for Jung *any* dimension of human behaviour can
also be expressed in its opposite form. It is also worth noting that it is within
the field of psychology that we can readily observe the paradox between
social identity and cognitive dissonance. On the one hand, social identity
theorists suggest it is usual to possess conflicting views about something. In
their study of women construction students, Powell et al. (2010, p. 573),

for example, conclude that 'identity is often contested ground for women construction students who, while subscribing to an ideal that the sector is accessible to all those who want to work in it, uphold gendered stereotypes about women's suitability for so-called masculine work such as construction.' And yet on the other hand, theories such as cognitive dissonance suggest to possess conflicting views is deeply unsettling.

The Honesty Paradox

The term is not used, but Gans (1962) in Bryman and Bell (2011, p. 124) reveals a paradox when exploring the ethics of ethnography: 'the researcher must be dishonest to get honest data.' Indeed, Denzin (1968), cited in the same volume, argues for an 'anything goes' stance as long as it does not harm participants or damage the discipline. More recently, during the ethnography stream at the European Group for Organizational Studies (EGOS) (2014) conference I made a note of the words presenters used to describe their experiences of conducting ethnography. In addition to those which we are by now quite accustomed, these included *aggressive*, *betrayal*, and *deceptive*. This seems to be a world away from the descriptions brokered in the often brief sections on ethnography in research methods textbooks. Related to this is the question as to whether ethnography ought to be covert or overt. While the 'observer effect' implies that overt ethnography will most likely modify subjects' behaviour (notably, Barnes experiences this for himself; see Chap. 7), covert ethnography presents ethical problems. Inevitably, since the ethnographer is all too human (see The All-Too-Human Paradox, below), she will most likely do a bit of both. However, crucially, this in no way represents a departure from real life since we present ourselves differently in accordance with circumstances; our behaviour is contingent on our environs. I cite, once again, my own experience at Findhorn. Given the highly emotive and contingent experience in a New Age community, the solicitation of permission to use a voice recorder was not only impractical but—notably—would have been extremely insensitive. I therefore did use a voice recorder, but kept it concealed in a pocket. When you are immersed in the field for weeks on end, there are times when the researcher's capacity for recall is bound to be compromised. I was, at various times, tired, frustrated, or confused. The voice recorder was essential to assist in the collection of relevant data. I acted dishonestly to acquire honest data. Of photographic documentation, too, how often does an ethnographer go through the process of securing formal permission to photograph her sub-

jects? To do so would render the process ungainly, bureaucratic and—by implication—create 'dishonest' representations of those photographed. A dishonest strategy is essential if we are to generate truer photographic data. To this end, Prince and Riches (2000: xi) suggest that their camera was used principally in situations whereby its use 'could pass for tourist snaps'.

The Consensus Paradox

As researchers new to ethnographic field work, we are schooled in sensitivity. We are schooled, in effect, to be sensitive and empathetic to our subjects as a means of avoiding conflict. The wording on your university's ethics approval process will most likely prime you to orient your research in this way. By implication, consensus between the researcher and her subjects reigns. But Janis's (1972) teachings in respect of *groupthink* (in which a prevailing desire for harmony results in dysfunctional decision-making) or the story Harvey (1988) recounts in *The Abilene Paradox* (in which a group of people collectively decide on a course of action that is counter to each of their preferences) caution against unbridled empathy. Consensus has an unmistakable allure, but it is through conflict that progress typically unfolds. We learn through our mistakes. Even catastrophe can be considered paradoxical since without it we become complacent. And complacency leads to further—and perhaps more damaging—mistakes. The concept of apocalypse is especially pertinent. Translated from the Ancient Greek for 'an uncovering', apocalypse describes a disclosure of knowledge; a lifting of the veil; a revelation. On the one hand, we are enlightened; on the other catastrophe unfolds. When conceptualised through the lens of apocalypse, then, knowledge or enlightenment elicits a deep-seated tension. We may therefore ask ourselves: will ethnography *without* conflict and *without* mistakes achieve anything truly insightful?

The All-Too-Human Paradox

As part of the review process, my Findhorn chapter was read by various people. Without exception, each of these reviewers (both formal and informal) has passed comment in respect of the hot tub scene. The circumstances of the environs were not especially relevant to the point I was trying to make at the time (in respect of Sofie's work life), but I decided to leave in the detail, conscious that I would reflect upon it in this chapter. Sofie was an attractive woman and similar in age to myself. In spite of the

professional expectations of academic research, I will not overlook the fact that I was physically attracted to her. We were alone in the hot tub and were both naked, as was conventional at Findhorn. The simple fact of the matter is that bathing nude in a hot tub was an erotic experience. However, at the time, I did not report this in my field notes. Why not? Perhaps it wasn't strictly relevant to my research endeavours. Perhaps, as a student, I felt compelled to maintain some sort of unspoken scholarly respectability. But in respect of intellectual insight, what a wasted opportunity! How many of us can claim to have gone through our many years of education, for example, without ever having an all-consuming crush on a teacher or classmate? How many of us have not felt considerable discomfort in respect of medical procedures which in some way invade our sense of the erotic? How many of us can say that our attraction to a colleague at work has not affected (for better or for worse) our ability to do our job? Such experience is intrinsic to the very fabric of our social lives and so as ethnographers to ignore it, or—worse still—repress it, is only going to compromise that insight.

During that same visit to Findhorn, I overslept one morning. I wanted to reflect on this as part of my research (notably that I was for the first time completely relaxed), but my supervisor commented to me back on campus that such a 'confession' was tantamount to sloppy ethnography and would imply to the reader a 'disinterested researcher'. It would paint me as 'lazy', he said, and that would not do. I yielded to his authority. In some respects I regret this because on a personal level it demonstrated that I felt at ease with life in the community. Surely, as ethnographers we have a responsibility to convey experiences beyond the parameters of what they might imply on a surface or 'respectability' level?

The Certainty Paradox

One of the recurring themes across the contributions in the book is that of existential uncertainty. Indeed, for several of our authors this concept of uncertainty has constituted a preoccupation. In an early draft of his chapter, by way of a preface to his own experiences transitioning from a positivist researcher to an ethnographer and the sense of existential doubt this elicits, Barnes (Chap. 7), for example, opened with a quote from Rilke's *Letters to a Young Poet*: 'Have patience with everything that remains unsolved in your heart… live in the question.' This is pertinent. Historically, our approach to paradox has been to view it as an inconvenience; we have

preoccupied ourselves with how best to resolve or dissolve the paradox. But is this necessary or indeed desirable? Most of us will be familiar with the philosophical truisms that underpin these experiences: 'the only thing we can be certain of is uncertainty'; 'the only constant is change', and so on. In turn, these find an analytical lineage dating back to Heraclitus of Ephesus's observation that 'you cannot step into the same river twice.'

But what, if anything, is the ethnographer to make of this? The certainty (or lack thereof) reported in this book is more practical than existential. Strudwick (Chap. 6), for example, airs concern that in her native discipline of radiography there was a danger that ethnographic research may be seen as un-scientific, lacking rigour and therefore easily dismissed. She utters the following questions: How much should I ask? How much should I participate? Should I simply observe? There is, of course, no straight answer.

As part of this exploration, several of the authors in this volume have tackled the concept of liminality. These ethnographic experiences at the liminal state seem to imply on the part of most, if not all, a sense of both fear and fascination as two sides of the same coin. For Dale and Burrell (2011, p. 113), architectural ruins are emblematic of this peculiar coupling: 'Fear comes from the significance that ruins hold for the integrity of our own world whilst the fascination with ruins lies in their liminal status between organisation and disorganisation, architecture and dust, order and chaos, humanity and nature. They materialise tensions in temporality and spatiality, survival and decay.' Fear and fascination inevitably disorientate. Drawing on the research of both Rosen (1991) and Foster (1990), Holliday (1995, p. 21) comments thus:

> ethnography allows the researcher to drift and formulate ideas in the research setting, and to explore uncharted ground. While at times this may feel like losing one's way, it in fact produces a far more dynamic and processual view of the research setting. Further it shows clearly how research itself is processual, and that in this way issues which may not have been thought of at the outset emerge through the fieldwork, and can rise to prominence.

It is a common concern among early career ethnographers that they feel as though they are losing their way. But this, once again, is what life is like: ethnographic methods mirror verisimilitude. A little further on, Holliday (1995, p. 30) refers to the 'chaotic nature of my experiences', and further normalises this experience. Indeed, it reflects in its entirety the picture of organisational life famously painted in *The Nature of Managerial Work*, by Henry Mintzberg in 1973. Management is not about command,

control, and coordination, as convention would have it. On the contrary, management is about muddling through, getting interrupted, and keeping your head above water. Uncertainty propels inquiry. It is the backbone of intellectual endeavour. But just try declaring that on your next ethics application form!

The Plagiarism Paradox

We live in a world where plagiarism is scorned and yet, in research—particularly ethnography—it is the dangers of *inverse* plagiarism that are the more arresting. For Fine and Shulman (2009, p. 185):

> [Ethnographers] engage in the inverse of plagiarism, giving credit to those undeserving, at least not for those precise words. To recall the exact words of a conversation, especially if one is not trained in shorthand is impossible [or indeed if you are not using a voice recorder; see *The honesty paradox*]. This is particularly applicable with those who maintain the illusion of 'active' or 'complete membership' by not taking notes within the limits of the public situation.

In this sense, paradoxically, the more 'genuine' your ethnography, the less likely you are to accurately represent your subjects since your note-recording capacity is inhibited by immersion. Perhaps, therefore, and given the scholarly tradition of 'accuracy' in respect of sources, inverse plagiarism is inevitable. However, and once again, it need not detract from the strength of the ethnography. Inverse plagiarism is another inevitability of everyday lives (e.g., when embellishing stories in the interests of effect). An inspiring book, a provocative film, an engaging lecture, each will likely involve inverse plagiarism, hyperbole, and embellishment. A dull one most likely will not.

The Linguistic Construction Paradox

For Humphries and Watson (2009, p. 40), 'ethnography is writing'. More specifically we might argue that ethnographic writing is reportage. As Liamputtong (2009, p. 42) reminds us, 'Through conversation… individuals have an opportunity to know others, learn about their feelings, experiences and the world in which they live. So if we wish to learn how people see their world, we need to talk with people.' However, given the

centrality of writing to ethnography, the biases associated with linguistic construction affect ethnography more, perhaps, than any other research method. In this sense, then, every word the ethnographer transcribes and every word she uses as part of her interpretation, both enhances our understanding of a phenomenon *and* creates further bias. As Best (see Chap. 9) writes, 'I've shaped you. I'm shaping you now.'

Vocabulary, too, is relevant. My own experience at Findhorn revealed a divisive vocabulary. To outsiders, Findhorn was most definitely a 'commune'. To insiders, the word commune was never used; 'community' was preferred. How was I to describe Findhorn? Which term would I use, or would I use a different term altogether? The academic literature had long abandoned commune in favour of intentional community, but this is in no way neutral. In abandoning the term 'commune' the discourse says, quite firmly, that it wishes to dissociate itself from those who regard such collectives derogatorily. This is clearly about identity. I felt that the use of 'intentional community' would prove rather ungainly throughout the entire narrative and so, ultimately, settled on 'community'. However, intentionally or not, this set out an allegiance. It carved out an identity, a political position, and I wasn't entirely comfortable about this. It is much the same in respect of the relatively recent move by the academy to distance itself from the terms 'prostitute' (in favour of sex worker) or 'gypsy' (in favour of traveller). The terms 'sex worker' and 'traveller' are no less biased than their counterparts (prostitute and gypsy); they merely represent a shift in political position (or, more accurately, a shift in the labelling of such positions). Boggis's research in this volume reveals something interesting in respect of disability, too. Boggis (see Chap. 5) explores Oliver's (1983, p. 261) observation that for some 'the term "people with disabilities" should be used in preference to "disabled people" because this prioritises people rather than disability'. However, for others, it seems, 'disabled people' is the preferred terminology of those within the disabled movement because it makes a statement: they are not 'people with disabilities', but people who are disabled or disadvantaged by society's responses to their differences.

The Autoethnographic Paradox

As Weir and Clarke have argued in Chap. 8, there is unquestionably an authenticity of knowing oneself. To this end, they defend autoethnography in light of Delamont's (2007) critique. However, one may choose to

point out that common sense suggests that the worst person to ask about me is me. This is, of course, part of the reason dating websites such as *mysinglefriend.com* have been so successful. Rather than engage in the uncomfortably narcissistic exercise of marketing yourself to potential partners, the task is delegated to a friend.

Notably, this is—I think—slightly different to the argument regarding the purported inability to 'fight familiarity' proffered by Delamont (2007). It is about perspective, yes, but it's not that the autoethnographic perspective is *wrong*; it's just different. It's no less valid. The point I'm trying to make is that there is a wonderful tension here. It's foolish to denigrate the tradition on the basis of an inability to fight subjectivity since it is that same subjectivity that enables the different perspective. Notably, for Jeffcut (1991, p. 13, cited in Holliday, 1995, p. 22) 'the objective of [ethnographic] interpretation is to bring us into touch with the lives of strangers, [and] one of those strangers is inevitably oneself.'

The experience of autoethnography will likely be unsettling for generations of researchers to come. But this doesn't invalidate it; on the contrary, it underscores its vitality. The autoethnographer is not an objective scribe. Rather, what's revealing about autoethnography is the sense of change and transformation; tension and contradiction. For Learmonth and Humphries (2012), for example, 'Throughout our adult lives we have both been haunted by a sense of doubleness—a feeling of dislocation, of being in the wrong place, of playing a role... Presenting ourselves as objects of research, we show how, for us, contemporary academic identity is problematic in that it necessarily involves being (at least) 'both' Jekyll and Hyde.' Finally, there's the perennial accusations of narcissism. Narcissism was explored in autoethnography as early as William Whyte's *Street Corner Society.* And, yes, writing about oneself *is* narcissistic. That is inescapable. But, once again, therein lies its significance.

The Definitive Methodological Paradox

For Denzin and Lincoln (1994, p. 15), research endeavours are 'defined by a series of tensions, contradictions and hesitations'. Ethnography is no different. Indeed, in ethnographic research, these tensions hint a much deeper basis: a paradox which lies at the very heart of the objective-subjective binary. Addressing the related discourses of truth, objectivity, and cause-and-effect in turn, I here conceptualise the definitive methodological paradox.

Truth

Truth and methodology have an awkward relationship. I have lost count of the number of undergraduate dissertations I have read in which in their methodology section reads something like this: *I have chosen a positivist approach because I am interested in the truth*. Although most likely a result of misunderstanding the purpose of methodological framing, that students fall into this trap is hardly surprising. We are primed to think of objectivity as 'good' and subjectivity as 'bad'. Objectivity, we are told, means truth. But even the hardest of hard sciences has no legitimate claim to the truth. We continue to teach Newtonian physics in our schools even though—by the perspectives of Einstein or quantum theory—Newtonian physics is wrong. But does this mean that Einstein or quantum theoretical approaches are correct. No. Semiotician Umberto Eco hints at as much in his novel *The Name of the Rose:*

> Perhaps the mission of those who love mankind is to make people laugh at the truth, *to make truth laugh*, because the only truth lies in learning to free ourselves from insane passion for the truth. (Eco, 1984, p. 491, original emphasis)

Nietzsche [1887] (1989, p. 151) has said, 'Strictly speaking, there is no such thing as science without any presuppositions.' Rather, (ibid., p. 119) 'there is *only* a perspective seeing; *only* a perspective knowing; and the *more* affects we allow to speak about one thing, the *more* eyes, different eyes, we can use to observe one thing, the more complete will our "concept" of this thing, our "objectivity", be.' Ethnographers are best placed to be the myriad eyes Nietzsche describes, each—in turn—contributing by way of a unique perspective to the collective ethnographic record. In this way, truth is more legitimately described as something subjective; something *emotional* (Bochner & Ellis, 2016, p. 85), and as something we *feel* rather than acknowledge (ibid., p. 218).

Objectivity

Henry Mintzberg (1979, p. 583) asks us some pertinent questions:

> What is wrong with small samples? Why should researchers have to apologize for them? Should a physicist apologize for splitting only one atom? A doctoral student I know was not allowed to observe managers because of

the 'problem' of sample size. He was required to measure what managers did through questionnaires, despite ample evidence that managers are poor estimators of their own time allocation. Was it better to have less valid data that were statistically significant?

Twenty-seven years on we are *still* forced to apologise for the same. In spite of the ideographic orientation of their research, Thoma*s* and Southwell (see Chap. 13) were forced to apologise for their 'small sample' of twenty. And what of the circumstances when we have a sample size of one; a single datum? In qualitative research methods classes, I am asked this question perhaps more than any other: *How many interviews do I need to do?* Inevitably, I respond with three pieces of advice: (1) I ask the student 'How long is a piece of string?' (2) I suggest they revisit the concepts of ontology and epistemology. (3) I point them to this brief passage in Holliday's (1995, p. 17) ethnography of a small business:

> At the very outset [of my research] I began to worry that I had not really seen the inside of a small manufacturing firm and so had no idea what kind of questions I would need to ask when I began my fieldwork. If I had been researching by questionnaire, of course, I might *never* have seen the inside of a small business.

These three pieces of advice are normally enough for the student to figure out that a small sample size is frequently advantageous. For Gelsthorpe (1992, p. 214) 'a rejection of the notion of "objectivity" and a focus on experience in method does not mean a rejection of the need to be critical, rigorous and accurate; rather, it can mean making interpretive schemes explicit in the concern to produce good knowledge.' The point here I think is that it is better to caveat (and say 'this is my story') than to control for variables (and so deny the existence of a story). As Becker (1967, p. 239) explains, it is impossible 'to do research that is uncontaminated by personal and political sympathies'. And to quote Alvesson and Deetz (2000, p. 66, emphasis added), once again: 'interpretivists and others often labelled as "subjective" often have the *better claim to objectivity* through the way they allow alternative language games and the possibility of alternative constructions arising from existing communities denying both research community conceptions and preferred methods as privileged and universal.'

Linear Cause-and-Effect

We are schooled from an early age to think in terms of 'cause-and-effect' or—in the humanities—'beginning-middle-end'. Such instruction is, of course, a gross over-simplification. For Marsden (1993, p. 115), for example, 'There can be no power without resistance because it is the relationship between A and B that causes the behaviour of both.' In reflecting on her ethnographic story, Best (see Chap. 9) says: 'Everything I've presented is in a linear fashion—when no story is really linear—it's chaos.' Only the very simplest of story would adhere to the expectations of linearity. Are 'stories' in the natural sciences any different? No. In *Paradoxes of Progress*, Stent (1978, p. 148) writes: 'Provided that the questions one asks of Nature are not too deep, satisfactory answers can usually be found. Difficulties arise only when… the questions become too deep and the answers that must be given to these questions are no longer fully consonant with rational thought.' Where analysis remains shallow, cause-and-effect ontologies (or 'stories') tend to operate effectively; it is where we dig a little deeper that paradox emerges. As a result, we become fearful of deeper analysis.

However, in spite of what we've said about truth, objectivity, and cause-and-effect, we live in a world where there is a bias towards analytical simplicity, or 'elegance' (as has become the popular term). We are told frequently that 'Simplistic explanations are the most effective' (BBC, 2016). Certainly, most positivist/quantitative research strives for simplicity. But this is a fundamental problem because our world is far from simplistic. 'Successful'—by which we really mean 'popular'—explanations are rarely accurate. Turning once again to my native discipline, theoretical models tend to come in the form of 2×2 typologies. Examples include Porter's diamond, the Boston matrix, and even Burrell and Morgan's sociological paradigms. Why is this? Is there some underlying elegance to the universe that favours such a configuration? It seems unlikely. A more likely explanation for the prevalence of 2×2 typologies is that they are simple. Furthermore, although typologies may purport to reflect, in practice they tend to *reinforce*; typologies are a way of organising. They are inevitably associated foremost with positivist/quantitative methodologies. By actively resisting a temptation to 'typologise', and instead pursuing research sensitive to a grounded theoretical approach, effective ethnography can rise above these concerns.

Let's look at this another way. Ethnography might be described as the *method acting* of academic research. Method acting traces its origins to Stanislavski's philosophy, a philosophy which was part of the theatrical realist movement based on the idea that good acting is a reflection of truth, mediated through the actor. For Shakespeare, of course, all the world's a stage and we are 'merely' actors. For Gephart (1978, p. 556), the methods by which social actors construct everyday life are important:

> Such actors are viewed as engaged in constructing and reconstructing social realities through generating and using meanings to make events sensible. A dramaturgical metaphor is often employed; actors must manage appearances and constantly ad lib essentially vague social roles in an emergent stream of existential being and awareness. A basic assumption is that social reality is not merely a stable entity but passively entered and apprehended, but one which requires actors (members) to work at accomplishing this 'reality for all practical purposes'.

'Real' life is, paradoxically, an act. For, Deloria (1969, p. 146) 'irony and satire provide much keener insights into a group's collective psyche and values than do years of [conventional] research.' Ultimately, of course, 'human behaviour is based upon meanings that attribute people to and bring to situations, and that behaviour is not "caused" in any mechanical way, but is continually constructed and reconstructed on the basis of people's interpretations of the situations they are in' (Punch, 2014, p. 126).

Scientists simply cannot be external to their experiments. A biologist himself, Stent (1978, pp. 212–213) reminds us that

> the kind of impersonal and objective science on behalf of which authority is claimed is only a myth and does not, in fact, exist. Since scientists are human beings rather than disembodied spirits, since they necessarily interact with the phenomena they observe, and since they use ordinary language to communicate their results, they are really part of the problem rather than part of the solution. That is to say, scientists lack the status of observers external to the world of phenomena, a status they would have to have if scientific propositions were to be truly objective.

Further on, Stent shifts attention away from the objective and to the intersubjective:

an individual's moral judgements arise by a transformational process operating on an innate ethical deep structure. But despite their subjective source, his moral judgements are not seen as arbitrary or completely idiosyncratic by others, because the innate ethical deep structure is a universal which all humans share. (ibid., p. 226)

Notably, intersubjective insight is the bread and butter of ethnography. Crang and Cook (2007, p. 37), for example, argue that 'to talk about participant observation should not be to separate its "subjective" and "objective" components, but to talk about it as a means of developing intersubjective understandings between the researcher and the researched.' 'Ethnography is neither subjective nor objective. It is *interpretive*, mediating two worlds through a third' (Agar, 1986, p. 19, emphasis added). One way in which I encourage my students to recognise this is by positing the concept of the collective unconscious. The collective unconscious is determined by recurring primordial behaviour throughout history. In this sense Jung was fascinated with, for example, the occult, religion, and parapsychology not because of their particular ontologies, but what their very existence as cultural artefacts tells us about humankind and its predispositions. In this sense, any attempt to educate ourselves out of these artefacts is likely to be existentially troubling. An empirical focus on intersubjectivity also enables ethnography to generate understanding in respect of *process* rather than *result* (see Cooper & Law, 1995, p. 238). And this is why ethnography has a unique responsibility. Ethnography is non-finite; it is live; it is dynamic; it unfolds; it is 'flying by the seat of your pants' (Van Maanen, 1988, p. 120). It is forever 'in process'.

Earlier in this book, Boggis (Chap. 5) drew on the pertinent words of Stanley and Wise. I restate them below:

Whether we like it or not, researchers remain human beings complete with the usual assembly of feelings, failings and moods. All of these things influence how we feel and what is going on. Our consciousness is always the medium through which research occurs; there is no method or technique of doing research other than through the medium of the researcher. (Stanley & Wise, 1993, p. 157)

Although supposedly objective research seeks to distance the researcher from her experiment or study, the 'reality' is that this mediation is likely the only thing 'true' about the research. Herein lies the paradox.

PARADOX AS PEDAGOGICAL DEVICE

For, Rappaport (1981, p. 121) 'the most important and interesting aspects of community life are by their very nature paradoxical; and [so] our task as researchers, scholars, and professionals should be to "unpack" and influence contemporary resolutions of paradox.' It is my belief, however, that attempts to resolve (or dissolve) paradoxes are misplaced. That is not to say that I believe instead we should work carefully to avoid paradox. No, paradox is an important part of life. But academics are reluctant to engage with paradox because to do so would undermine our role as 'experts', since 'expertise' invariably assumes logic. However, we have—I hope—debunked the concept of the 'expert' (in terms of familiarity), earlier in this chapter. So how might we use paradox by way of pedagogical device? Take the paradox associated with identity. Liberal-minded academics (and ethnographers are perhaps a case in point) are fond of lending voice to marginalised groups. But how desirable is this? The pressures of identity politics, for example, seek overt recognition of minority groups such as, for example, LGBT. But to what extent does this further marginalise minorities from forming part of an integrated community? Drawing on Oliver's work, Boggis (see Chap. 5) recognises something similar in respect of disabled groups and how they are labelled. The point, of course, is *not* for the pedagogue to suggest that LGBT designations are destructive, or that a particular nomenclature in respect of the disabled is warranted; rather the point is to suggest that any research that smacks of ideological closure should be viewed with suspicion, irrespective of how noble its ambitions appear to be.

WHAT NOW?

Paradox is pervasive: from the theory of relativity (Einstein, 1916) to the pursuit for world peace (Mosley, 2009). Paradox exists *between* disciplines too. Although usually considered in binary opposition, science and religion rest upon comparable causal ontologies. Indeed, they are frequently invoked to justify one another. Isaac Newton, for example, held that absolute space and absolute time are constituted by the omniscience and omnipotence of God, as his 'Sensorium' (Powers, 1982, p. 31).

That paradox is pervasive means ethnographers must proceed with extreme caution. Although—ironically—we have demonstrated that eth-

nography often has the better claim to objectivity, there is no room for complacency or self-righteousness. For Yanow (2010, p. 1400):

> ethnography entails a complex interchange between the researcher's prior conceptual boxes and the field data generated—and one can only hope, from an interpretative methodological perspective, that the data are not being force fitted into those conceptual boxes but rather that the shape and content of the boxes are being allowed to develop into a bottom-up fashion in light of those generated, non 'given' data.

And this is crucial. The sensitivity built into the ethnographic enterprise does not guarantee it will be deployed. One concern is that while positivists may be blissfully ignorant of the biases underpinning their frameworks, interpretivists—who are not—may be using these to their advantage. After all, paradox manifests itself in both directions. As Atkinson and Hammersley (1994, p. 253) imply, positivism may actually be more sensitive to participant well-being than interpretivism: 'It is suggested that by its very nature anthropology (and the point can be extended without distortion to ethnographic work in general) involves "representation" of others even when it does not explicitly claim to speak for or on behalf of them.'

Ours is a brave new post-paradigms ontology and it is one in which ethnographers have a formidable responsibility.

References

Agar, M. (1986). *Speaking of Ethnographies*. London: Sage.

Alvesson, M., & Deetz, S. (2000). *Doing Critical Management Research*. London: Sage.

Atkinson, P., & Hammersley, M. (1994). Ethnography and Participant Observation. In N. Denzin & Y. Lincoln (Eds.), *Handbook of Qualitative Methods*. London: Sage.

BBC. (2016). Retrieved January 27, 2017, from http://www.bbc.co.uk/news/magazine-35311422

Becker, H. (1967). Whose Side Are We On? *Social Problems, 14*(3), 239–247.

Bell, E. (1999). The Negotiation of a Working Role in Organizational Ethnography. *International Journal of Social Research Methodology, 2*(1), 17–37.

Bochner, A., & Ellis, C. (2016). *Evocative Autoethnography: Writing Lives and Telling Stories*. London: Routledge.

Bowie, M. (1979). Jacques Lacan. In J. Sturrock (Ed.), *Structuralism and Since: From Levi-Strauss to Derrida*. Oxford: Oxford University Press.

Bryman, A., & Bell, E. (2011). *Business Research Methods*. Oxford: Oxford University Press.

Burrell, G., & Morgan, G. (1979). *Sociological Paradigms and Organisational Analysis: Elements of the Sociology of Corporate Life*. London: Heinemann.

Cooper, R., & Law, J. (1995). Organization: Distal and Proximal Views. *Research in the Sociology of Organizations: A Research Annual, 13*, 237–274.

Crang, M., & Cook, I. (2007). *Doing Ethnographies*. London: Sage.

Dale, K., & Burrell, G. (2011). Disturbing Structure: Reading the Ruins. *Culture and Organization, 17*(2), 107–121.

Delamont, S. (2007). *Arguments Against Auto-Ethnography*. Paper presented at the British Educational Research Association Annual Conference, Institute of Education, University of London, September 5–8.

Deloria, V. (1969). *Custer Died for Your Sins: An Indian Manifesto*. Norman, OK: University of Oklahoma Press.

Denzin, N., & Lincoln, Y. (1994). Introduction: Entering the Field of Qualitative Research. In N. Denzin & Y. Lincoln (Eds.), *Handbook of Qualitative Research*. London: Sage.

Eco U. (1984). *The Name of the Rose*. New York: Harcourt Inc.

Einstein A. (1916). *Relativity: The Special and General Theory*, (Translation 1920). New York: H. Holt and Company.

European Group of Organizational Studies (EGOS). (2014). Subtheme 15: (SWG) Organizational Ethnography: The Theoretical Challenge, Rotterdam.

Fine, G., & Shulman, D. (2009). Lies From the Field: Ethical Issues in Organizational Ethnography. In S. Ybema, D. Yanow, H. Wels, & F. Kamsteeg (Eds.), *Organizational Ethnography: Studying the Complexities of Everyday Life*. London: Sage.

Ganga, D., & Scott, S. (2006). Cultural "Insiders" and the Issue of Positionality in Qualitative Migration Research: Moving "Across" and Moving "Along" Researcher-Participant Divides. *Forum: Qualitative Research, 7*(3), Article 7.

Gelsthorpe, L. (1992). Response to Martyn Hammersley's Paper "On Feminist Methodology". *Sociology, 26*(2), 213–221.

Gephart, R. (1978). Status Degradation and Organizational Succession: An Ethnomethodological Approach. *Administrative Science Quarterly, 4*(23), 553–581.

Hammersley, M. (1992). *What's Wrong with Ethnography? Methodological Explorations*. London: Routledge.

Hammersley, M., & Atkinson, P. (2007). *Ethnography: Principles in Practice* (3rd ed.). London: Routledge.

Harari, Y. (2011). *Sapiens: A Brief History of Humankind*. London: Vintage.

Harvey, J. (1988). *The Abilene Paradox and Other Meditations on Management*. Oxford: Maxwell Macmillan.

Holliday, R. (1995). *Investigating Small Firms: Nice Work?* London: Routledge.

Humphries, M., & Watson, T. (2009). Ethnographic Practices: From 'Writing-up Ethnographic Research' to 'Writing Ethnography'. In S. Ybema, D. Yanow, H. Wels, & F. Kamsteeg (Eds.), *Organizational Ethnography: Studying the Complexities of Everyday Life*. London: Sage.

Jackson, N., & Carter, P. (1985). The Ergonomics of Desire. *Personnel Review, 14*(3), 20–28.

Janis, I. L. (1972). *Victims of Groupthink*. New York: Houghton Mifflin.

Learmonth, M., & Humphries, M. (2012). Autoethnography and Academic Identity: Glimpsing Business School Doppelgängers. *Organization, 19*(1), 99–117.

Liamputtong, P. (2009). *Qualitative Research Methods* (3rd ed.). Oxford: Oxford University Press.

Marsden, R. (1993). The Politics of Organizational Analysis. *Organization Studies, 14*(1), 93–124.

Mascarenhas-Keyes, S. (1987). The Native Anthropologist: Constraints and Strategies in Research. In A. Jackson (Ed.), *Anthropology at Home*. London: Tavistock.

Mintzberg, H. (1973). *The Nature of Managerial Work*. Upper Saddle River, NJ: Prentice Hall.

Mintzberg, H. (1979). An Emerging Strategy of "Direct" Research. *Administrative Science Quarterly, 24*, 582–589.

Mosley, N. (2009). *Paradoxes of Peace*. London: Dalkey Archive Press.

Nietzsche, F. (1989[1887]). *On the Genealogy of Morals*. New York: Vintage.

Oliver, M. (1983). *Social Work and Disabled People*. Basingstoke: Macmillan.

Powell, A., Dainty, A., & Bagilhole, B. (2010). *Achieving Gender Equality in the Construction Professions: Lessons from the Career Decisions of Women Construction Students in the UK*. Retrieved May 17, 2013, from http://www.arcom.ac.uk/-docs/proceedings/ar2010-0573-0582_Powell_Dainty_and_Bagilhole.pdf

Powers, J. (1982). *Philosophy and the New Physics*. London: Methuen.

Prince, R., & Riches, D. (2000). *The New Age in Glastonbury: The Construction of Religious Movements*. Oxford: Berghahn.

Punch, K. (2005). *Introduction to Social Research*. London: Sage.

Punch, K. (2014). *Introduction to Social Research: Quantitative and Qualitative Approaches* (3rd ed.). London: Sage.

Rose, D. (1990). *Living the Ethnographic Life*. London: Sage.

Rousseau, D. (1998). Why Workers Still Identity with Organizations. *Journal of Organizational Behavior, 19*(3), 217–233.

Silverman, D. (2007). *A Very Short, Fairly Interesting and Reasonably Cheap Book About Qualitative Research*. London: Sage.

Singer, I. (2009). *Philosophy of Love: A Partial Summing-up*. Cambridge, MA: MIT Press.

Stanley, L., & Wise, S. (1993). *Breaking Out: Feminist Consciousness and Feminist Research*. London: Routledge and Kegan Paul.

Starobinski, J. (1975). The Inside and the Outside. *The Hudson Review, 28*, 333–351.

Stent, G. (1978). *Paradoxes of Progress*. San Francisco: W. H. Freeman & Co.

Rappaport, J. (1981). In Praise of Paradox: A Social Policy of Empowerment Over Prevention. *American Journal of Community Psychology, 9*, 1–25.

Toffler, A. (1970). *Future Shock*. London: Pan Books.

Van Maanen, J. (1988). *Tales of the Field*. Chicago: Chicago University Press.

Vine, T. (2010). *Book Review: Organizational Ethnography – Studying the Complexities of Everyday Life*. In S. Ybema, D. Yanow, H. Wels, & F. Kamsteeg (Eds.) London: Sage (2009). *Organization 17*(5), 645–649.

Yanow, D. (2010). Review Essay. *Organization Studies, 31*(9 & 10), 1397–1410.