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EDITORIAL: ENTERING THE FIELD OF CRIMINOLOGICAL RESEARCH

Dr Jaime Waters, Senior Lecturer in Criminology, Sheffield Hallam University

“We are - before we are academics, scholars or researchers - diverse human beings with a vast array of life experiences and complex histories. The emotive processes that stem from these and the theoretical insights they can provide should not be underestimated. My point here is that the ‘self’ is not just who we are, but a living embodiment of how we research, how we theorize and how we come [to] know and tell about our subjects. In this respect, no longer should it be relegated to footnotes or methodological appendices.” (Wakeman 2014: 719).

This special issue of the British Journal of Community Justice will focus upon the experiences of researchers making their entry into the field of criminological research. Its genesis can be traced to the annual meetings of the European Society of Criminology Postgraduate and Early Stage Researcher Working Group. During these meetings the group’s conversations repeatedly turned to the nagging feeling that the lived reality of criminological investigation did not always match up with what we had been taught in classes or read in textbooks prior to beginning our nascent research careers.

Professor Yvonne Jewkes’ keynote speech at the 2013 British Society of Criminology conference at the University of Wolverhampton, which highlighted some of the difficulties of engaging in criminological research, inspired us to bring together a special panel at the 2014 European Society of Criminology Conference in Prague on ‘entering the field of criminological research’. At this panel a group of emerging scholars picked up on the themes Professor Jewkes had discussed in Wolverhampton and sought to show the special difficulties faced by those more inexperienced researchers starting out on their journey into the discipline. At this panel Dr Jaime Waters discussed how personal biography had influenced her research, Dr Sabine Carl talked of the difficulties involved in interviewing professionals and experts, Bethany Schmidt spoke of values, allegiances, and politics in prisons research, and Filip Vojta presented on issues in qualitative research on international criminal justice, all from the perspective of those undergoing their initiation into the world of scholarly inquiry. The panel was very well received, and two of the papers presented on that day (Waters and Carl) have been developed for inclusion in this issue.
This special issue, building on the success of the Prague panel, seeks to document the experiences of early career researchers and to provide an account of the ‘messiness’ of the research process that, all too often, inexperienced researchers are ill prepared for. Across the four original contributions, a foreword by Yvonne Jewkes and an afterword by Stephen Wakeman, a number of key themes have emerged. For example, the papers raise the question of the place of emotion in research and the way in which emotions can potentially be useful in scholarly investigation. In her keynote speech in Wolverhampton, Jewkes’ specific focus was on the emotional investment often required in qualitative inquiry. She suggested that criminology as a discipline has remained largely silent on this matter. Indeed, with a few recent and commendable exceptions (for example, Wakeman, 2014; Lumsden & Winter, 2014), the discipline has tended to encourage the repression of the messy emotional reality of qualitative inquiry. Yet, particularly in the case of inexperienced researchers engaging in fieldwork for the first time, powerful emotions can often forcefully intrude on the research process. How they are accommodated thus becomes of real importance, and it is an issue not dealt with in conventional accounts of the research process. Here, Fleetwood takes a ‘confessional’ approach in her piece which allows for an exploration of her life experiences, how they informed her entry into the field and how they affected the actual process of data collection itself. Carl discusses the emotional aspect of her research, and considers how this shaped her analysis, while Waters’ piece features reflections on the emotional hardship of persisting with a ‘failing’ sampling strategy in the absence of better alternatives.

In a similar vein, the papers in the special issue interrogate the notion of reflexivity; what is its role and how can it be a worthwhile tool? There is a reflexive bent to all of the papers in the special issue as the relatively young authors found themselves constantly questioning their practice and technique as they attempted to collect data or bring their studies to fruition. Reflexivity is inherent in Fleetwood’s ‘confessional’ approach, for instance, whilst for Carl it emerges in the re-evaluation of her research and its findings. It is also clear in Preiser’s discussion of what gets included in one’s field notes and one’s findings, and, just as importantly, what does not. Meanwhile, the process of self-reflection engaged in by Waters as she struggled to bring her research to a satisfactory conclusion ultimately resulted in valuable insights into the nature of her participants.

Wakeman (2014: 708) has argued forcefully for the importance of autoethnography and the role it can play in the creation of meaningful criminological knowledge, and this is a repeated motif in our collection of papers. That our authors were not well known, experienced operators, yet to be fully assimilated into their role as researchers or academics, shaped their studies in profound ways. Both Carl and Waters use an autoethnographic lens to scrutinise their findings on politicians and prison ombudsmen and older illegal drug users respectively, while Fleetwood acknowledges the impact of her own biography on actually gaining access to her research subjects in the first place. Preiser’s understanding of how her personal biography impacted upon her ethnographic work clearly demonstrates the importance of situating oneself critically within the research endeavour.

Finally, the papers provide a sense of how the institutional and political environment that research takes place in can often serve to obscure the difficulties that budding researchers
face. As Lumsden and Winter (2014) have argued, and as any student of Foucault would concur, the creation of knowledge itself is bound up with issues of funding, politics, and governance. The current criminological milieu can often seem to consist of “a renewed and growing dominance of and push for positivist and normative criminology and crime science and the push for applied evidence based research, which further includes increased professionalisation, use of metrics and the impact agenda in the United Kingdom, the pursuit of knowledge transfer opportunities, enterprise activities and funding” (Lumsden & Winter, 2014: 2). For the more inexperienced investigator setting out on their journey into the field, this environment can be especially harsh as it can render trivial difficulties such as the ones detailed in the contributions here. Furthermore, external constraints can be particularly ominous to junior researchers as the risk to one’s nascent career should a study not deliver on its objectives can be significant.

In sum, the contributions presented here offer reflections on a very particular time in their authors’ research careers. It is clear that there are some specific challenges that inexperienced researchers face on their entry into the research field. It is also clear that those who are new to the praxis of research must come to terms with the messy and often emotional reality of the environment they find themselves in very quickly if they are to be successful. Perhaps inadvertently, in making these accommodations, our contributors managed to uncover certain truths about the research process that are of universal importance to the discipline. The issues they raise here, though they might well resonate especially keenly for newcomers to the research process, are surely applicable to all researchers, whatever their level of experience or esteem. Acknowledgement and understanding of such issues can only lead to better research practice. As Wakeman puts it, “an increased focus upon the self in criminological research can produce significant advantages in three interlinked fields: the ways in which research is done, the theory that stems from it and then the ways in which it is presented” (2014: 706). Thus this is not an exercise in “navel gazing, merely placing the researcher at the centre of the work” (Lumsden & Winter, 2014: 10), but a staging post towards better research across the board.

Contributions to the Special Issue
Professor Yvonne Jewkes, whose keynote speech at the 2013 British Society of Criminology conference inspired this special issue, provides the foreword which sets the stage for the contributors and offers a number of insights that are taken up in the papers that follow. For instance, Jewkes highlights how much research, inadvertently or otherwise, conceals the messiness involved in ‘getting in, getting on and getting out’ and as a result does a disservice to more junior researchers. She also discusses the lack of emotional recognition and discussion in research publications, the ‘bureaucratization’ and ‘commodification’ of research and results, the difficulties involved in ‘reconciling the personal contradictions inherent’ in the research process, and, more broadly, the notion of reflexivity. Jewkes reminds us that, in the end, research is always an ‘inherently personal, political and partial endeavour’ and that ‘objectivity and balance may not only be impossible and impractical goals, but may also be undesirable if these qualities neutralise important issues’.
Fleetwood focuses on her experiences of undertaking ethnographic fieldwork with imprisoned drug traffickers in Ecuador. She takes a ‘confessional’ approach, examining globalised hierarchies alongside her own biography (‘middle-class, white British’ and ‘western, Anglophone foreigner’). The role that storytelling played in her fieldwork is also discussed. Fleetwood explores, among other things, how the interplay between her biography and her adoption of narrative practice served to facilitate both physical access to the prison as well as the developing relationships with her subjects. Indeed the data collected in the field and the analysis that followed was decisively shaped by the narrative storytelling that all the participants (including Fleetwood herself) engaged in.

Carl explores some of the difficulties involved in interviewing professionals and experts, reflecting on her emotions in conducting two qualitative interviews for her PhD. The differing relationships that developed between the researcher and the researched led to interviews that varied greatly in tone and tenor. Carl reflects on how the interaction between herself and her interviewees and the emotions that bubbled up on the days in question affected the subsequent analysis and presentation of the findings. She argues for the value of a second or even third analysis of the data, which allows for the passage of time and the subsiding of emotions and can assist the researcher in gaining new insights. She ends with a ‘helpful guideline for (self-) reflection’ and invites future researchers to learn from her experiences.

Waters, using the lens of autoethnography, examines how her own personal biography affected the research process in a study of ‘hidden’ older illegal drug users. The lengthy and difficult gestation of her research was caused in part by her age, nationality, professional standing and differing cultural outlook to her participants. Yet, on reflection, these difficulties helped to reveal something of the nature of the participants themselves. In the end it was possible to ‘know’ the participants a little better precisely because of the problems involved in bringing the project to a successful conclusion and the sometimes strained relationship that developed between researcher and researched.

Preiser writes on the tensions and the risks involved in an overt participation observation of nightclub bouncers in Germany. Her wide ranging discussion focuses on three main aspects of this type of ethnographic work: Firstly, the risks involved for the participants and the researcher; Secondly, how openness, discretion and active but restrained participation can contribute to the success of the endeavour; and finally, how intentional blanks in fieldnotes - partly inadvertent, partly intentional - can help to protect the participants. In sum, Preiser argues that this type of work involves a constant balancing act between competing logics. On the one hand is the logic of ethnography to reveal that which is ordinarily hidden, and on the other is the logic of research ethics to prevent harm to participants as far as possible. She suggests that ethnographic data needs to remain incomplete, because it is through silences and absences that we can guarantee the privacy and anonymity of our participants and thus encourage them to allow us into their worlds.

Stephen Wakeman, whose article in the British Journal of Criminology on autoethnography (2014) resonated with us as we worked on the special issue, looks forward to the future of the burgeoning field of autoethnography in his afterword. For Wakeman, autoethnography provides a means through which both theory and practice
can be critiqued and created anew. His piece serves as something of a call to arms against an unreflective and often leaden criminology and is a fitting way to end the issue. As he puts it, “the real beauty of the autoethnographic approach then is that it can provide a means by which both our discipline’s dominant theories - as well as the dominant, privileged voices that propagate and protect them - can be challenged and transcended.”

We would like to thank the British Journal of Community Justice for hosting the special issue and in particular Jess Bamonte for her valued support. A number of anonymous reviewers helped us to sharpen the articles prior to publication which we are grateful for. We would also like to thank Professor Yvonne Jewkes and Dr Stephen Wakeman for supporting the special issue and for their fantastic contributions. Finally, I would like to thank the contributing authors for their patience as we pulled everything together and, of course, for their insightful articles.
References
FOREWORD: ENTERING THE FIELD AND LEAVING INTACT

Professor Yvonne Jewkes, Research Professor in Criminology, University of Brighton

In July 2013 I was invited to give a plenary talk at the British Society of Criminology conference, the theme of which was ‘Criminology on Trial’. I am honoured that my presentation that day has helped to inspire this special collection of thought-provoking articles written by colleagues at relatively early stages of their careers. In my talk, I decided to put ethnographic fieldwork on trial and I discussed why I think that many established academics do a disservice to new scholars contemplating entering the field for the first time, by writing up their methodology and findings as if everything has gone completely smoothly. They have entered the field without any problems concerning access or ethics, they have gained a rapport with every one of their participants, elicited rich and novel data, and left the field with it (the field), and their own mind, body and self entirely intact. But as this collection testifies, anyone who has conducted criminological ethnography knows this to be untrue - and the tricky process of ‘getting in, getting on and getting out’, as ethnography is so often characterized, does not necessarily get much easier as one makes the journey from fledgling researcher to ‘quirky professor’ (see Carl, this volume). No matter how ‘established’ we are in our careers, how well connected or experienced, or how resilient we imagine ourselves to be, we all face obstacles (sometimes insurmountable) in the course of our research lives, and we all confront challenges in the analysis and writing-up phases of our studies (Fleetwood, this volume). Yet rarely do we discuss them in our work.

I also put ethnographic research in criminology on trial because, in contrast to many other social sciences, criminology has largely resisted the notion that qualitative inquiry has emotional dimensions, often reflecting aspects of the researcher’s own biography and life experiences. Not only do subjectivity and the self nearly always inflect our research, to the extent where ‘fieldwork is, in part, the discovery of the self through the detour of the other’ (Hunt, 1989: 42) but, when done properly, reflexive, self-attuned ethnography has the capacity to reach and uncover data that we might not otherwise have access to; indeed Liebling (1999) has gone so far as to state that ‘emotions constitute data’ (my emphasis; see also Liebling, 2014, Rowe, 2014). Examples of recent scholarship that illustrate the point - with a frankness that goes well beyond classic sociological studies (Becker, 1967; Merton, 1972) of ‘insider/outsider’ positionalisities, or deciding whose side we are on - include Wakeman’s (2014) account of participant observation with heroin and
crack cocaine users and dealers, as a former user and dealer himself; the contributions in the 2014 special issue of *Qualitative Inquiry* 20(4) on ‘Doing prison research differently’; and the chapters in *The Pains of Doing Criminological Research*, edited by Beyens et al. (2013).

Criminological ethnographers face a multiplicity of emotional challenges, including; being superficially judged by our research participants (Waters, this volume) and our peers, ‘bearing witness’ to the frequently painful experiences of others, and dealing with the intended and unintended consequences of the research after the data has been collected and the research published (Sparks, 2002; see also Liebling, 2014; Reiter, 2014). Such tensions illustrate the contested terrain on which we operate. Typically, though, the researcher does not record their feelings about such matters, or even how they navigated these difficult obstacles (if they do provide such reflection, it is conventionally consigned to an appendix). Their presence in the field is seen as an intrusion, and admissions of fear, anger, shame, disgust, excitement or empathy are understood as being in tension with objective thought, reason, or strategic self-examination (Katz, 2002). Consequently, the process of *doing* criminological research in all its daunting, exhausting, exhilarating, responsive, opportunistic, on-a-wing-and-a-prayer messiness (words that will never appear on a university ethics application form!) does not feature in written up, published accounts, which is why this special issue on ‘Entering the Field of Criminological Research’ - with its particular emphasis on the relationship between research and *self* - is so welcome. The articles contained in this issue avowedly reject the possibility of emotionally objective ethnography and discuss how working in criminological settings and listening to the accounts of offenders, victims, politicians and peers inevitably intersect with our own experiences, so that each cannot help but shape our interpretation of the other.

While qualitative research in many criminological fields, including studies of imprisonment, is undeniably enjoying a revival (see, for example, Drake et al., 2015) - if indeed, it ever experienced an eclipse (Wacquant, 2002; Crewe & Jewkes, 2010) - there are many more obstacles to entering the field with a sense of academic freedom than existed when I began my career in the 1990s. University committees that make judgments on the ethical dimensions of our research, government review panels that exist to ensure that our ideas and projects meet their strategic objectives, and funding councils whose ‘dark arts’ decision-making processes can make the pursuit of knowledge seem like a lottery with little chance of success, have all played a part in neutralizing the complex human relationships, potentially risky (or outright dangerous) situations, and emotionally-charged topics that used to be our stock-in-trade. Moreover, the ‘impact’ agenda, which has been embraced by HEFCE and RCUK, and looks set to gain further momentum in the run-up to the next Research Excellence Framework (REF), has made the pursuit of knowledge for knowledge’s sake a thing of the past. In the current age, knowledge must be practical, applicable and, ideally, technically useful. More importantly still, this type of knowledge has a monetary value and is generated in order to be sold. The unfortunate result is that those university departments which fail to turn ideas into profit are simply closed down (Ishiyama & Breuning, 2011).

Against this backcloth, there is a sense in which researchers become machines, doing the research, *finding the findings*, that will be most useful and palatable to the specialist...
officials in government departments and public and private sector agencies (Hope, 2008). While it is still true that academic scholars write for each other, as well as for our paymasters, we now do so with the specific requirements of the REF in mind, and early career researchers are under particular pressure to abandon all thoughts of quirkiness (which might actually equate to principle and passion) in order to be submitted to a given unit of assessment. This bureaucratization and commodification of our research results in a very particular kind of publicly accessible knowledge, whereby acknowledging emotional investment in one’s research might present a barrier to getting published. As Drake and Harvey (2010) have argued, there is an unspoken understanding that if we disclose the emotions that underpin and inform our work, our colleagues will question its ‘validity’ and perhaps even our suitability to engage effectively in criminological research. In order to ‘get in’, ‘get on’ and ‘move up’ in one’s career as a criminologist, one quickly learns to repress emotions, or at least to express them in language and tones that convey professionalism, not passion.

For me, this is puzzling in a field in which many of us spend our careers studying stigmatized or vulnerable ‘others’ in settings where differential indices of power, authority, vulnerability and despair are felt more keenly than most. Expressions of unease at practices of power and punishment, and desire for separateness from those administering them, more-or-less go with our territory, as Becker remarked back in 1967. Indeed, such feelings are among the reasons why many of us are drawn to the study of deviance, crime, criminalization and punishment in the first place. Yet, once in the field we put these feelings to one side. Writing about prisoners’ auto/biographies, Joe Sim (2004) argues that criminology’s fixation with methodology, objectivity, restrained language and appropriate form means it does not know what to do with prisoners’ reflexive narratives. Equally, it might be argued that these fixations discourage any form of biographical or emotional intrusion by the researcher (Jewkes, 2012, 2014).

For some ethnographers, however, the greatest challenge comes from reconciling the personal contradictions inherent in the process. How do we account for the presences and absences in our research, the unintended consequences, and the fact that the neatly wrapped-up, published presentations of our work are manifestly incomplete (see Preiser, this volume)? How do we resolve the inevitable tensions that arise from positioning ourselves, or being positioned by others, on one side or another? How do criminologists resolve the personal, ethical dilemmas associated with ‘keeping on side’ with gatekeeping officials and authorities (even if only to gain access to the field), while being critical of the structures, processes and organisations that those individuals support? These kinds of issues go to the heart of criminological inquiry and many of us grapple with them constantly in our research lives. On recently being informed by a colleague that something I had written had ‘upset’ some members of a prison estates board at the Ministry of Justice, I assumed I should feel admonished, but I did not. In publicly criticising the design of recently built prisons in England and Wales - these anodyne, ‘value-engineered’, pile-‘em-high-and-build-‘em-cheap, warehouses that rack and stack medium-security prisoners in high-security conditions (a violation of human rights that is somehow sanitized by the euphemism ‘future-proofing’) - I have found myself in the curious position of being denied access to some of those prisons, while being asked to advise numerous individual and collective stake-holders about how future prisons can be designed and
planned differently in order to achieve positive outcomes for prisoners and staff. My belief that those elements of human environment and existence that have transformative power must over-ride the desire for consumption and profit, have not always been popular with the bureaucrats who are answerable to ministers and private share-holders. But neither have they been received kindly by some reformists and abolitionists, who have reductively characterized reform through design as a superficial or futile exercise, akin to applying lipstick to a pig. Furthermore, the idea that the goal of good prison design should be to give prisoners hope of a meaningful future, in order ultimately to drastically reduce the prison population, is sometimes received with ambivalence by those mindful of the promises made to economically lagging communities, such as Wrexham in North Wales, where the new prison due to open in February 2017 will bring over 1,000 jobs and £23million per year to the local area. Our research is often conducted within a political landscape full of potential minefields (Liebling, 2001), but if we do not successfully navigate this awkward terrain, we risk producing partial, irrelevant or even Utopian research that is produced solely for the benefit of a small number of fellow academics (Sim, 2004).

The criminological research process is, then, an inherently personal, political and partial endeavour that involves numerous challenges, including from our own community. For me, objectivity and balance may not only be impossible and impractical goals, but may also be undesirable if these qualities neutralise important issues and lead to what Sim has described as ‘theoretical reductionism and political timidity’ (2004; 113). I have argued elsewhere (Jewkes, 2012; 2014) that a more honest and reflexive approach to qualitative research would provide a benchmark for others trying to process their experiences and feelings about the research they undertake, and this is probably the reason why my plenary at the British Society of Criminology conference was well received by scholars who had recently entered the field for the first time, or were contemplating doing so. The contributions to this special issue are testimony to my belief that emotional experiences can serve as a powerful intellectual resource, resulting in the research experience being positive and ‘unusually life affirming’ (Jewkes 2012: 66).
References
ENTERING THE GLOBAL FIELD: TALK, TRAVEL AND NARRATIVE PRACTICE IN ECUADORIAN PRISONS
Dr Jennifer Fleetwood, Lecturer in Criminology, University of Leicester

Abstract
Prisons in Ecuador represent a globalised field. In this fieldwork confessional I outline my place within this field (characterised by global inequalities), and describe the ways in which I gained entry to the community of foreign nationals. In particular, I focus on the construction of being foreign as a specific membership category, as well as the role of narrative and storytelling in bridging international and social divides, fostering a shared sense of community, and the role of visitors as listeners for inmates' stories. This narrative practice made researching drug trafficking possible, however such stories require careful interpretation to avoid misinterpretation.

Keywords
Narrative; fieldwork; methodology; prisons; globalisation.
Introduction

Before I was an ethnographer, I was a backpacker. In 2002, I spent two months in Ecuador learning Spanish and 'travelling', like many other middle-class, white British kids. During a month long stay in Quito, I heard I could visit Brits imprisoned for drug trafficking. With little deliberation, I noted down the instructions and the following Wednesday set off for the men's prison with my passport, the name of the inmate I would visit, and a carrier bag of cigarettes, chocolate, and toilet paper (these being the things I thought a prisoner from home might like).

Garcia Moreno prison is downtown, high up on the side of the valley about ten blocks from the presidential palace. The San Roque neighbourhood is poor and rarely visited by most Quiteños, let alone tourists. On a clear day, the views across the city are spectacular; small comfort for the long queue of visitors lined up outside under the baking midday sun. I had never visited a prison before, but was soon ushered through bag and body searches, jostling alongside prisoners' friends and family members, most carrying packages (one family even carried a mattress). Guards painstakingly unpacked suitcases full of clothes and stabbed birthday cakes and tubs of food for contraband. Finally, near the main door, we handed over our identification and the name of the prisoner we were visiting was carefully entered, by hand, into a hefty ledger. Finally, our credentials checked (we were stamped on our forearms for every search), we were finally ushered inside.

Built in 1875, Garcia Moreno prison is star shaped, like London's Pentonville. The walls are thick stone with small windows: its corridors dark and cold in contrast with the equatorial sun. We passed the kitchen where inmates stirred cauldrons of slop over massive fires, and quickly arrived in the centre of the 'star'. Here, visitors enter the prison. There is no visit room. A short, scruffily dressed man found me in the crowd and asked who I wanted to visit: we quickly established that the man I planned to see had left and instead he took me to El Britanico, the British man. He led me up two flights of stairs, dirty with stagnant water and rubbish and along the second floor balcony to the end, banging noisily on a heavy cell door, which was locked from the inside. A small metal grate slide to the side, and two eyes frowned. 'I've come to visit', I said, holding up my bag of gifts. A moment or two later, the door clanked open. 'I'm Paul', he said.¹

This article offers a 'confessional' about how I came to undertake fieldwork with drug traffickers imprisoned in Ecuador with the aim of 'explicitly demystifying fieldwork... by showing how the technique is practiced in the field' (Van Maanen, 2011:73). Fieldwork for my PhD was begun during my undergraduate degree, finally published some years later (Fleetwood 2014). As the above account shows, getting into prison was remarkably straightforward. In fact fieldwork for my undergraduate dissertation was undertaken during visit days. I eventually sought formal research access, but only many years after

¹ Of course, all names have been changed to protect respondents. Identifying details have also been removed.
access had been established informally. Although methodological issues around formally gaining access to criminal justice institutions are important, this article is instead concerned with the sociological and cultural dynamics that enabled me to enter prisons in Ecuador as an outsider. This fieldwork, researching international drug trafficking, offers some insights into entering a global field, by examining the ways in which new categories of in/out are constituted. Drawing on field notes, I consider how entry was gained, especially storytelling as a central aspect of the culture of drug traffickers in prison and the backpackers (like me) who visited them. My account is prefaced by a review of how ethnographers have successfully gained entry into global fields that are criminal or deviant. This article is mainly concerned with how I gained entry into the men's prison. Although my fieldwork spanned three prisons in the end, this was the first site and so represented my initial entry.

**Entering global research fields**

The influence of globalisation on crime and criminal justice institution is firmly established (Aas, 2013; Bowling & Sheptycki, 2011; Findlay, 1999). Yet, criminologists tend to undertake research at 'home', or to undertake projects bounded by the nation state. In contrast, sociologists such as Burawoy call for ethnography that engages with, and seeks to understand processes of globalisation (2000). Doing so means rethinking fieldwork, 'combining dwelling and travelling' to engage with fields that stretch across the globe (2000:4; see also Büscher & Urry 2009; Letherby, 2010). Rather than engaging with one place (as with traditional ethnography), Burawoy argues for fieldwork that is multi-sited, but also grounded in local history (2000), thus able to grasp the macro sociological processes that shape daily life. Rather than studying globalisation itself, Sassen offers two ways of thinking about global/globalised sites. The first is the trans-nationalised local, in other words local practices that now have a transnational or global dimension (2007). The second is the globalised sub-national (the globalised local): spaces, places and social practices profoundly re-shaped by processes of globalisation (ibid).

Such global fields arguably present particular challenges to establishing entry, yet, many of the same issues are still in play. Ethnographic researchers' tales of 'getting in' are a recognisable sub-genre of the fieldwork confessional, and advice for the novice ethnographer abounds (Brewer, 2000; Hammersley & Atkinson, 2007). For ethnographers of crime and deviance, prior connections count for a great deal, ensuring the researcher can be trusted, or at least that he/she is not police (see for example Pearson 1993; Hobbs

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2 I acknowledge the Programa de Estudios de la Ciudad, Facultad Latinoamericana de Ciencias Sociales (FLACSO) sede Quito, Ecuador for hosting me and negotiating research access during fieldwork in 2006. I acknowledge the Dirección Nacional de Rehabilitación Social, Ecuador for formally granting me access as a researcher. In 2014, Garcia Moreno prison (and the other prisons in which I undertook fieldwork) closed for good, replaced by US style high security jails in which visitors spend limited time with family in a public visit room. The porosity of Ecuadorian prisons (before 2014) allowed families to meet regularly, talk at length, eat and even sleep together. It also enabled me to adventure into prison as a backpacker tourist, bring my friends and eventually undertake fieldwork (before I was granted official access). The reader may rightly conclude that I abused this privilege for my own purposes. The closure of the jail (and de facto end of backpacker prison visiting) allows me to write frankly about prison tourism. I hope this confessional account does not bring embarrassment to those (past or present) involved in running prisons in Ecuador.
1993, 2001). On the other hand, ethnographers without personal connections must typically negotiate social distance and power imbalances as a prerequisite to building trust and rapport (see for example Goffman, 2015:215). An array of relationships to the field have been typologised, from complete participant (i.e. complete member) to complete observer (Gold, 1958). Adler and Adler propose several membership roles that ethnographers can adopt (1987). What kinds of roles can global ethnographers adopt? How do ethnographers gain entry into global fields? In a globalised world, do personal connections matter?

Entering transnational fields is especially challenging for ethnographers interested in crime and deviance. Transnational crime, by its nature, happens in many places at once, stretching across the globe. An excellent example of multi-sited ethnography is Scheper-Hughes' ethnography of organ trafficking (2004). As well as contacting and interviewing those who had donated or received organs, she also posed as an organ-buyer as a way of gaining entry to medical establishments (ibid). This methodology reflects the kinds of temporary social interactions that characterise the international organ trade, as well as Scheper-Hughes’ status as a US citizen, allowing her to adopt a membership role, albeit temporarily. Similarly, Brotherton and Barrios' transnational ethnography of Dominican deportees (2011) draws on fieldwork undertaken in New York and the Dominican Republic. They traced Dominican deportees’ experiences of social exclusion through deportation (and in some cases their illegal return to the USA), producing rich insights into how social exclusion (via the criminal justice system) operates transnationally. Both researchers had a degree of 'insider' status owing to personal biography: Barrios migrated to the USA from Puerto Rico as a child and had 'insider' contacts in the Dominican community in New York. Brotherton (also an immigrant to the USA from the UK) also undertook ethnographic research of the legal processes that facilitate deportation while acting as an expert witness (2011: 27).

Ethnographers of crime and deviance have more commonly researched globalisation at the level of the sub-national (i.e. the globalised local). Nonetheless, the distinction between local and transnational may not be so clear-cut. Hobbs' decades-long ethnography began as a distinctly local study of criminal sub-cultures in London's east end (1995). Nonetheless, his ethnographic fieldwork elucidates the ways in which local and transnational crime is connected, noting that transnational crime is 'local at all points' (1998: 419). Likewise, ethnographic research on international drug trafficking has made ample use of local 'hubs' in the transnational drug trade, for example Patti and Peter Adler's classic ethnographic of the international cocaine trade (1993). Full membership roles were not appropriate, and so they became peripheral members. They note that entry was gained via persistence, and drawing on aspects of shared values and identity. Damian Zaitch's ethnography of Colombian traffickers in the Netherlands drew on his biography as an Argentinian. He notes: 'Neither Dutch, nor Colombian myself, I often felt fortunate to be in the intermediate position of a "quasi-native" researcher' (2002:13). Despite being 'quasi-native', he reports engaging in the kinds of activities typical of ethnographic field researchers: 'They asked me all sorts of favours and things: juridical advice, translation of documents, housing, money, books, email and mail addresses to receive post, or jobs. I was also expected to learn salsa and to drink and smoke with them. I did most of these things and some more.' (2002:15).
Both Adler and Zaitch undertook ethnographic fieldwork 'at home'. Researching globalised sites abroad presents particular issues for gaining access due to global social inequalities. Hoefinger undertook fieldwork on the bar girl scene in Cambodia (local women who undertake transnational sex - sometimes - with foreign men) (2013). She carefully describes the ways in which she and her respondents' similar ages and common participation in a shared, globalised culture, enabled rapport, although not as equals:

"The powerful influence of my hegemonic, western culture and [respondents'] desire to emulate certain characteristics of it (the liberalness, the consumptiveness, the spending power, the fashion styles, the dance styles) speaks to the inherent (neo-) colonial nature of structural hierarchies that still dominate the foreign-local social landscape of Cambodia and in most countries of the global south... We were from the same generation and therefore we shared not only similar mutual interests in style of music, for example, but also life goals that crossed boundaries of class and race." (2013:55).

Ethnographic research is arguably well placed for engaging with global fields of crime and justice. What follows is my account of gaining entry into prisons in Ecuador, considering how biography and globalised hierarchies can be problems as well as resources for gaining entry.

A globalised field: hierarchies of mobility in prison in Ecuador

Travel is an 'integral part of a 'new world order of mobility' (Richards & Wilson, 2004:3). Along with new technologies, it has had a profound effect on contemporary life and global social relations. In 2005, 37 per cent of prisoners in Quito were foreign nationals, mostly charged with drug offences (Gallardo & Nuñez, 2006: 9, 27).³ They came from all over South America, but especially Colombia. 12% were from outside South America (ibid), (South Africa, Thailand, North America and all corners of Europe) reflecting the globalised nature of the flow of cocaine. Globalisation has not happened everywhere, nor has it happened evenly (Ray, 2006; de Sousa Santos, 2006). Bauman argues that globalisation produces new hierarchies of mobility, describing two paradigmatic types: the 'tourist' and the 'vagabond', both consequences of postmodern consumer society (1998). While tourists (the globally mobile) travel for adventure, to consume experiences, vagabonds are compelled to travel: for work, for survival, because they have no other choice (ibid). The question of structure and agency in drug trafficking is complex (Fleetwood, 2014), nonetheless it is true that few traffickers travel due to pure desire to see and experience the world. Many, but not all, could be considered vagabonds: those compelled to travel (Bauman, 1998).

In contrast to all this global mobility, prisons are 'factories of immobility' (Bauman, 1998); places for managing the 'waste' of postmodern consumer capitalism. Prisons in Ecuador were exactly such. Prisons housed many local, Ecuadorian men known as "polillas" (moths).

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³ The national average was 9.3 per cent (Gallardo & Nuñez, 2006:9). Prisons in Quito held a much higher average due to the proximity of the international airport.
Many were vagrants prior to their incarceration and continued to be so on the inside, making a bare living, often through begging (Nuñez, 2006). Recall the man who escorted me to Paul's cell: he spent visit days running errands for 25c a time. Prisoners received little more than a place to sleep (not a mattress, linen or an actual bed) and a daily ration of the food from the communal kitchen (but not a bowl to put it in, or a spoon to eat with). With long delays in sentencing and little prospect of education or training, Ecuadorian prisons are truly immobilising.

The polillas' bare existence seems untouched by globalisation. Poor, ethnic minority men and women have been imprisoned for drunkenness, theft and other minor offences in Garcia Moreno prison during most of its 139-year history (Herrera 2014). Yet, their contemporary incarceration - like foreign national drug traffickers - must be understood through the lens of geopolitics. In the 2000s prisons in Ecuador were profoundly impacted by the so-called 'war on drugs' led by the USA in the region. For example, in 2005 the Ecuadorian government signed a bilateral agreement with the USA agreeing to meet targets of a 10% increase in the capture of drugs, and a 12% increase in the capture of so-called narco-traffickers (Pontón & Torres, 2007). Since Ecuadorian drug laws did not differentiate between users, dealers and traffickers, nor between drugs, such policies encouraged a 'logic of quantification': vast numbers of poor people were arrested and imprisoned, mainly mules, dealers and local men and women possessing very small quantities of drugs (Edwards, 2003). During the 2000s, Garcia Moreno prison operated at near double capacity. Ethnographic research documents daily life in prisons pushed to the brink by overcrowding and lack of funding (Nuñez, 2006; Pontón & Torres, 2007; Pontón, 2008).

In contrast to inmate-vagabonds, I was a tourist, one of those who 'become wanderers and put the bitter sweet dreams of homesickness above the comforts of home - because they want to... or because they have been seduced by the true or imaginary pleasures of a sensation-gatherers life' (Bauman, 1998:92). My entrance to prisons in Ecuador reflects privilege on a global scale. Even as a student, international travel was within easy reach. Part of this is historical good fortune. In the early 2000s I was in my early 20s in Blair's Britain and being white and middle class, I could anticipate employment, and in the meantime engage in a degree of youthful hedonism and adventure on the back of my student loan. International travel was cheap and commonplace. Backpacking had moved from the realm of the 'hippy drop out' to the mainstream (Reilly, 2006). Travel was an enticing leisure activity with the potential to gain experiential knowledge of the world, to become more open minded, independent, in short, more 'worldly'. These values are summed up by sociological and anthropological work on backpacker subculture (Binder et al., 2004; Welk, 2004).

My formal entry into my field site is also paved by privilege in terms of class and ethnicity. Indeed, it is a long held critique that many ethnographers of deviance research 'down' (Hobbs, 2001). Coming from an 'old' university in the global north brought credibility

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4 Garcia Moreno closed for good in 2014.
5 See also the documentary 'El Comite' (Herrera, 2005).
helping me negotiate an honorary position at a local university⁶ and research access to prison. Being a pale, blue-eyed global elite was also advantageous in fieldwork. Guards tended to assume I was a member of the embassy, or a missionary, both roles reflecting long-standing international involvement in Ecuador. Nonetheless, I recorded a steady stream of phone cards, pens, chocolate, cigarettes and biscuits taken by guards, either because they were ‘forbidden’ or following polite requests.⁷ My age and gender didn’t mark me out as a distinguished professorial type. A female prison guard once accused me of being a prostitute after she saw me hug one of my respondents (and then “confiscated” my research pass!).⁸ Gender, class, sexuality and generation were important, but - as will be seen - my status as a European was paramount.

**Negotiating global hierarchies: identity, community and storytelling**

Bauman notes synergies between ‘vagabonds’ and ‘tourists’ claiming the vagabond is the tourist’s most ardent admirer’ (1998: 94). This synergy, as alter egos and admirers, facilitated entry into the field in unexpected ways, enabling me to establish the ‘mutuality’ necessary for ethnographic fieldwork (Hammersley & Atkinson, 2007: 72). Paul and Graham were my first respondents, and gatekeepers who introduced me to fellow foreigners in the men’s and women’s prisons. They often commented that they would never have spoken to me on the outside, partly since, as drug traffickers, they would not have spoken to a nosy outsider (Hobbs, 2001), but also because our paths would simply never have crossed. Underpinning this statement was a keen awareness of social distance and divisions that would have played out at ‘home’. Early on in fieldwork, I noted:

‘They are an extraordinary group of people in my life and perhaps me in theirs. Not in the sense that I am an extraordinary person, but because we are out of the ordinary to each other.’ (Fieldnote, November 2005).

Paul’s background is firmly working class; he left school early to work. After a stint in the army, he moved from job to job. His first visits to Latin America were to meet his wife’s family. While in prison, he drew, painted, carved, and his tattoos graced many bodies in Garcia Moreno. We are a generation and a class apart. We also hung out with Graham, a kid my age (mid-20s) from a North American city I knew partly through ethnography and film. Graham told me about selling drugs in school, working as a masseur in a health club, getting married in his teens and receiving packages of drugs in the mail. Our lives could not have been less alike: I worked in a haberdasher, and had played cello in the local orchestra. Although I had once been mistakenly accused of selling cannabis in school, vast social difference existed between us. But, in prison being Anglophone global northerners offered us a shared identity, which we expressed through the kinds of things we talked about, and how we ‘hung out’ together.

We all liked martial arts films and Reggaeton music; arguably part of globalised culture (like Hoefinger, 2013). Oftentimes we hung out in Paul’s cell drinking tea and eating

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⁶ See footnote 2.
⁷ I should state explicitly that I was never asked for, nor did I ever pay a bribe to enter prison.
⁸ Fortunately I quickly applied for a replacement which was quickly granted.
biscuits, listening to the radio, and playing cards. At least one pleasant Saturday afternoon was spent chopping vegetables with the one blunt knife available to make a chilli. In passing time together, we sought to create a common sense of 'home' (even though none truly existed). Being 'foreigners' offered us a shared sense of identity: one that comes into being in the context of Garcia Moreno prison as a globalised field.

**Constructing community: foreign nationals in prison**

As a 'foreigner', I was accorded entry into the small group of international drug traffickers in prison, comprised mainly of English speakers from North America and Europe, as well as some honorary members from Latin America (especially those who had lived in the Anglophone north). Although there were many kinds of foreigners in prison (from all over Latin America, West Africa, the Middle East and even China), this group set themselves apart, not only through language, but as would be 'tourists', and consumers within the prison.

Ecuadorian ethnographer Nuñez notes that the introduction of relatively well-educated, well-off foreigners had a profound effect on the prison through their introduction of capital and organisation (2006). Pavilion C was mainly populated by drug traffickers and housed most of the international drug traffickers, or at least those who could afford it. Each pavilion charged inmates an entrance fee as well as regular taxes that funded all maintenance of the wing as well as bribes for guards. Pavilion C was a world apart from the other wings; noticeably cleaner and better decorated. The introduction of foreigners resulted in a thriving internal economy (funded largely by the drug trade) (Nuñez 2006). In addition to the marketization of pavilions and cells, individual cells also ran as 'corner shops'. In the hallways, inmates paid rent on small food and drink stalls. A Russian owned kiosk functioned as the de facto headquarters of Europeans, selling lunches, including a delicious borscht, as well as tea and coffee to the foreigners who hung out there all day:

'Ryan and I... went and sat in the patio of C, very posh, chairs 'n tables and umbrellas too....We sat at a table with Michael [Canada] and the 'old boy' [UK] came by... Also the young Romanian guy Bee, Leonardo [Italy] (who is still trying to get me to sell footage of prison fights on the internet. I also met Johan, a German with several piercings on his face. Graham dropped in and out too. Santiago [Colombia] also dropped by to see what's up. Noting the variety of nationalities represented, he declared it to be a criminal United Nations!' (Fieldnote, October 2005).

Some foreign nationals worked within the prison. Graham spoke Spanish (and was voted by his pavilion to work in the prison administration), but many did not making it hard to find work, aside from perhaps teaching English. Some received regular financial support from family; others continued to traffic drugs. Those who could afford it were consumers within the marketised context of the prison: would-be tourists who distinguished themselves from vagabonds. As well as frequenting food kiosks (they rarely ate the slop

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9 The men's and women's prisons each housed groups of foreign nationals which were quite well connected. Wives and girlfriends in the women's prison could apply to visit their partners every Thursday. Many had also met while detained, or were connected through criminal cases.
Entering the global field: talk, travel and narrative practice in Ecuadorean prisons

provided by the prison), they bought a variety of goods and services: employing Ecuadorians to cook, do their laundry and even clean their cells. Foreigners were also targets for extortion within the prison, and many fell on hard times during their stay in prison: initial sympathy and donations from family members often petered out during long sentences.

Membership was automatically accorded on the basis of nationality. Whenever a new inmate arrived, inmates collaborated to connect them into existing networks. For example, Santiago was from Medellín in Colombia and owned a corner shop. Hearing that a fellow paisano had arrived, he immediately sent out a package of bread and cigarettes for him. Similarly, whenever a European arrived, fellow Europeans would try to find them a decent cell, and try to warn them about how to avoid becoming a victim of extortion. Our group of foreigners overlapped with other alliances, sometimes according to race/ethnicity, and at other times for business (including drug trafficking).

Becoming a member
As a foreign national who regularly visited, I was initially a peripheral member of the group, but became an active member (Adler & Adler, 1987). My role - as most regular visitor - brought obligations relating to the challenges faced by inmates (Adler and Adler 1987: 34). I was frequently called on to collect official paperwork or prescriptions, to contact home or the embassy or undertake any other 'emergency' duties that needed an outside contact. Being a visitor to both men’s and women’s prisons meant that I acted as a conduit between communities of foreigners in both prisons, often delivering messages, letters and gifts.

In 2002 (before I started fieldwork), I brought a fellow backpacker and Ecuadorian, Diego, to Garcia Moreno prison. Diego and Graham hit it off right away - both having lived in the USA and being of the same generation. In 2005, when I returned for PhD fieldwork, Diego was back in Quito and still visiting Paul and Graham. Between August and December 2005, I spent every weekend in prison, and some Wednesday afternoons too. I rarely visited alone, often visiting with Diego and we also brought flatmates and friends along. To be clear, visits were strongly encouraged by most foreigners in prison. Although having a 'once off' visit might have made them vulnerable to voyeurism (and even exploitation), visits were valued as an important contact with the outside world. The stark reality was that many would otherwise have received no visits at all.

I had little difficulty persuading fellow backpackers to join me for a visit to prison. Prison visiting closely reflects the typical backpacker values - including independent adventure, travel, experience, open-mindedness, meeting other people, and a commitment to counter-culture (Bradt, 1995, in Welk, 2004:80; Matthews & Matthews, 2012). Both imprisoned traffickers and backpackers are fluid, transient, and relatively un-institutionalised groups (Welk, 2004). Both groups can be understood as either literal, or would be, tourists (according to Bauman, 1998). This shared sense of identity as part of a community of foreigners) as well as a shared values were reflected in the ways that

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10 In later years, Diego would help Graham gain parole by giving him a work placement. Graham learned to cook professionally - a career he still pursues.
backpackers and inmates passed the time together. On an especially busy day, around 8 of us were crammed into Paul's cell:

'Paul’s cell was incredibly packed and noisy. People were playing cards and smoking, chatting and passing coconut around, and gaseosa and cups of coffee and biscuits. It quickly got smelly and smoky and sweaty...' (Fieldnote)

We crammed in, jostling for space and place in the card game, the air thick with smoke. Somebody rolled a joint and passed it around, and another cut lines of cocaine. Diego tried to translate for poor Chad (whose Spanish was developmental) over blaring reggaeton music. The scene hardly felt like prison, which was probably why inmates valued them so much.

Through long-term presence in prison, I moved from being a peripheral group member, to taking an active role in the community (Adler & Adler, 1987), especially in my role in connecting men and women to each other (as conduit) and in actively expanding the community by bringing in visitors. Although the presence of most of the visitors I brought in with me was temporary this fit with backpacker styles of interaction, typified by short, intense meetings. The following section considers my role in the foreign national community, especially in relation to storytelling.

**Storytelling**

Later that year, Paul’s radio was taken over by cockroaches, which changed the station at random. One found its way into Paul's ear in a terrifying incident quickly solved by another inmate pouring warm oil in his ear. Paul is an excellent storyteller, who turned the cockroach incident into a gory story about the brutality of life in Garcia Moreno, told sometimes for fellow inmates but more often for visitors. Criminologists have long noted the importance of talk and narrative in prison (Irwin & Cressey, 1962; Copes et al., 2013; Crewe, 2005; Ugelvik, 2015). Storytelling was especially important in prisons in Ecuador as a globalised field. Inmates told stories amongst themselves to get to know each other, establish their place in the prison hierarchy, explore future drug trafficking collaboration, to make sense of imprisonment, as well as for entertainment. Visitors offered an arena for a different array of stories. Storytelling is also a key part of backpacker culture: sociologists also note that backpackers are a 'garrulous folk', noting the importance of adventure for telling stories, and vice versa (Elsrud, 2001; Noy, 2004; Matthews & Matthews, 2012).

**Common storytelling ground: intoxication and travel**

Storytelling could bridge national divides and differences between foreign nationals in prison, and foreign visitors, especially through stories about common culture, interests and experiences. As fellow travellers, a common topic was places visited - not only in Ecuador, but the whole of Latin America. Especially fun was to work out where paths might have crossed, or even to discover friends in common. Stories say something about the speakers' identity; stories about travel positioned the speaker as someone who was adventurous, and - sometimes, worldly-wise. Stories about tattoos played a similar role; both inmates and backpackers had them and they were often revealed and admired. All recorded important life experiences, both serious and frivolous. They also told of the
narrator's life before prison: a portable record of experience and statement of identity. Backpackers and prisoners are a globally transient population; where accent and dress might mark out our class and culture, here tattoos represented a mobile marker of identity.

Stories about drug taking and intoxication were also common. This is a common kind of narrative, especially told by young people (Tutenges & Sandberg, 2013; Sandberg & Tutenges, 2015). Of course I have my own intoxication story (everyone does!), and participated in the exchange. Some stories of intoxication were highly polished, often hilarious, and there was an element of competition over who could tell the most outrageous one. Intoxication stories especially communicated shared values of living for the moment, hedonism and an appetite for adventure, as well as drug knowledge and know-how. Telling these kinds of stories was a way for visitors to demonstrate street-smartness and common ground, as a way of bridging social divides. Everyone was invested in presenting themselves as 'cool': backpackers often wanted to narratively distinguish themselves from 'straight' society, and prisoners as ordinary deviants (more below). Finally, as Tutenges and Sandberg note, telling intoxication stories is also a key part of getting intoxicated (2013). Although some backpackers came into jail with the explicit intention of buying and taking drugs, not all did. Arguably, the opportunity to take drugs in prison was as exciting as the story that could be later told about having done so!

**Traffickers' stories: Getting rich**

Inmates' stories about travelling were often about smuggling drugs. Often inmates played up to visitors’ expectations about meeting a 'real' trafficker, like those made famous in Rusty Young’s book *Marching Powder* (Young & McFadden, 2003), or in trafficker biographies such as *Mr Nice* (Marks, 1997) and *Snowblind* (Sabaag, 1997). There was a common saying in Garcia Moreno prison: trafficking drugs meant either getting rich, getting killed or going to jail. Some men had tales of getting rich and living the high life, selling to pop stars as well as near misses and narrow escapes. Trafficking narratives were sometimes realistic, but sometimes fantastic. Juan told of how he married the local cartel leader's daughter after they ran away together into the desert when they were just 16, only to be rescued by her fathers' henchmen. Needless to say, I loved being entertained by these stories, and no doubt respondents especially enjoyed telling them to a wide-eyed girl. Indeed, my presence was clearly an excuse to tell stories and show off:

‘Whilst hanging out in Santiago's room [which was also a working corner shop] his friend who owns the burger stall came by and chatted. He'd been collecting debts and had a grand [a thousand dollars] in his pocket which he took out and fanned himself with [the rest of us rolled around laughing]. He also showed me his bullet holes, even where a bullet was still in…' (Fieldnote)

Stories about being a 'real' trafficker also had to be balanced against the danger of being seen as a real/dangerous criminal. Researchers elsewhere report on hierarchies of crime in prison (most recently Ugelvik, 2015). As Paul often put it: 'we’re all nice people in here, we’re just criminals'. Talking about dangerous others was a way to narratively present oneself as one of the good (bad) guys:
'There was also a story told about the Chinese assassin who used to share with Lucho who was nicknamed Jet Li. He was rumoured to be in jail for a series of assassinations, including an entire family.' (Fieldnote)

Thus, stories about being a successful trafficker were a way to present oneself as street smart and authentic, whilst also avoiding the stigma associated with more apparently 'serious' forms of offending.

**Going to jail: stories about bad luck and hard times**

As well as stories about 'getting rich', stories were also told about 'going to jail'. Most men had a story explaining their arrest, and these were told regularly to backpackers (indeed I heard the same narratives retold regularly). Reflected the close connections between masculinity about crime, such stories tended to neutralise the arrest, rather than the offending behaviour. 11 Often pure bad luck was to blame. Juan blamed his arrest on the eruption of a volcano: since his flight was delayed, new airport staff came on shift, rather than those he had bribed to turn a blind eye to his bag. Bad luck stories made clear that the teller was not a bad trafficker, but rather a good trafficker who had some bad luck. Other such stories included incompetence of their collaborators, or even being double-crossed. More rarely, men told stories about hard times:

'Primo showed us photos of some amazing river shrimp and chatted extensively about how much they are sold for, how much they would sell for in the US, and here in Quito, exportation costs and profit margins. He said, I'm not really a trafficker; this is my business.' (Fieldnote)

Trafficking was a way to support Primo's legal business when it was failing. Unfortunately he had ended up in prison (rather than getting rich), but hoped to make international contacts that might enable him to send his shrimp to Europe for a better price.

**Surviving prison**

Like Paul's gory story about the cockroach in his ear, stories about hard times in prison were sometimes told to visitors. In part, this was a response to backpackers' expectations about visiting one of the 'most dangerous prisons' and hearing about it first-hand. Inmates' stories included light-hearted and even dark humour (recall Paul's cockroach story). Paul's repertoire of stories for visitors included showing them a photograph of himself many kilos heavier before his arrest. As well as showing off his trafficking aptitude (he explained that customs inspectors and air hostesses would never see the drugs strapped to his body, knowing it rude to stare), the same photo also illustrated the brutality of the prison regime: years of food poisoning had reduced him to a fraction of his former weight.

Stories were also told about violence in prison. During a three month long strike in 2005 no guards were present in prison. A story about the violent murder of an inmate was re-told many times:

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11 Similarly, Topalli reports that non-arrested drug dealers, car-jackers and robbers narratively seek to neutralize being good, rather than being bad (2005).
'They [Paul, Graham, Juan and Paco] told us one tale about a guy who had accepted a contract on a guy's life in return for a matchbox full of crack. The guy (who was supposed to be murdered)'s friends found out, [and planned a counter attack. They] cornered [the would-be assassin] in his cell, broke in the window and set fire to his cell and threw in a canister of CS gas. He was forced out and then 15 of them stabbed him to death. Some guy went to carry out his body and he groaned so was 'properly finished off' after all that. According to Graham he was cut up into pieces, boiled and put down the drain. According to Paul they found his genitals in the rubbish bin.' (Fieldnote)

Re-telling the story was perhaps a way of warding off a similar fate: being able to explain such brutality was a way to make sense of it, and to reassure oneself it could be avoided. Violence stories also demonstrated the toughness of the storytellers to survive such a place. This was especially so for backpacker audiences, and especially women:

'Pam [Diego's friend, a college student from north America] asked how safe [the prison] was for women [visitors]. Paul's reply was, more or less, "God forbid anyone should so much as scratch you to steal your earrings. You'd get your stuff back before you left the prison and someone would end up in hospital, or perhaps the morgue..." after reading this back to him [I recorded it in my notebook], he added, 'Possibly. We don't know how deep the spike would go in.' He then got out his 4" sharpened spike and his sharpened spoon and demonstrated how he would use them (I was sitting opposite him and found it kind of intimidating).' (Fieldnote)

Stories about violence could rarely incorporate victimisation. One inmate reported being tortured into making a confession, including having an electric current passed through his genitals. This story was rarely told in front of other inmates, however Diego reported having been shown the scars. As a visitor, he was an important listener (and believer) of this particular story. In the men's prison few openly admitted to vulnerability (either in lives before imprisonment, or their current situation). Although talking about drug trafficking was extremely common in the men's prison, being seen to talk too much, in the wrong way, or to the wrong person could result in being labelled as having a boca floja (loose tongue), with the potential for being seen as 'fake', or even for fatal retributions.

Thus, visitors also opened up the possibility of telling different kinds of stories, especially those which could not be voiced in the day-to-day life. Backpackers were eager listeners and consumers of these tales. They were considerably more credulous than other inmates might have been; this was not always a bad thing. Although these stories sometimes elicited gifts from visitors, the main purpose in telling them seemed to be listened to, rather than helped (after all, what help could backpacker visitors really offer?). To tell one's story to a visitor was also to have existed, to leave some trace on the world. Some foreign nationals received no visits from home during long sentences of six to 12 years. One foreign national inmate explained: 'you've got to be visited; you've got to be heard'. Isolation could be overwhelming; inmates often likened it to being buried alive.
Discussion

The process of getting in reveals much about the nature of the field site and its relationship to outsiders (Hammersley & Atkinson, 2007). Above, I have outlined the ways that prisons in Ecuador were shaped by global geopolitics (the war on drugs in particular) and the ways that social hierarchies in prison reflected global social inequalities. As a global northerner and literally as a 'tourist', visiting prison reflected travel as a privilege that contrasted sharply with those stuck there, such as the polillas. As a foreign national, I was quickly accorded a peripheral membership role, which over time developed into an active membership role (Adler & Adler, 1987), at least in relation to foreign national prisoner community (although not actual drug trafficking - to be clear). Whereas class and gender might have set me apart in other contexts, here being a western, Anglophone foreigner came into being as a category of membership.

Storytelling was a common social practice shared by both backpackers and prisoners who comprised a community of foreign nationals in prison in Ecuador. This community was made in/through storytelling practices, including storytelling about travel, tattoos and intoxication. Having visitors opened up space for inmates to tell stories about themselves, and drug trafficking that could not have been otherwise told; these included stories about getting rich and living the fast life as drug traffickers, but also about hard times in trafficking and in prison, including stories about violence. Visitors, myself included, played an important role as listeners (often quite credulous ones!).

Unlike other backpacker foreign visitors whose presence was usually temporary, I played a role in extending the community, and, through extended presence, I became part of the mythology of prison life. I was often around when things happened, and so was able to participate in the shared telling, and re-telling of events. One day, an inmate was assassinated shortly after I arrived. As I climbed the stairs I met Graham, stopping him for a cigarette and chat. The assassin had been right behind me on the stairs. Graham had been on his way to visit the murdered man and later attributed me with saving his life by delaying him. This was the most dramatic instance of becoming part of the mythology of prison life. I was often (falsely) attributed with having started backpacker prison tourism. I first visited as a backpacker in 2002, and so the practice was well established by then (although visits were probably less frequent). Nonetheless, I updated information already available and did bring in visits, so perhaps there is some truth in the myth.

I was also required to tell stories about myself as researcher, and as someone who would ultimately tell respondents' stories. Respondents wanted to know who I was, what kind of stories I would tell about them, and what was in it for me. Of course, my responses were all, inevitably, narrative in nature. Storytelling was also a useful way to establish myself as someone distinct from other visitors. Where some visitors tried to prove their street cred by showing off their drug knowledge, displaying my ignorance was a way to present myself as a researcher (rather than pleasure-seeker).

All of this storytelling, or 'narrative practice' (Gubrium & Holstein, 1998), made researching drug trafficking possible. Nonetheless, these narrative practices in prison
shaped the data in important ways: structuring what could, and could not be said. These
were not ephemera, but central to interpretation and analysis. To quote C. Wright Mills:

‘The language of situations as given must be considered a valuable portion
of the data to be interpreted and related to their conditions. To simplify
these vocabularies of motive into a socially abstracted terminology is to
destroy the legitimate use of motive in the explanation of social actions.’
(Wright Mills, 1940: 913).

Ethnographic fieldwork is uniquely placed to be able to appreciate the narrative practices
that shape the field (and thus data) so profoundly. Gubrium and Holstein refer to this as
narrative ethnography (2008). Narrative ethnography brings into focus the role of
storytelling in social life (drawing on ethnomethodology), and is ideally placed to
understand the 'language' of situations. In this context especially, drawing on interview
data alone would have ignored the particular narrative forms common in prison, and to
risk misinterpreting the subsequent interview data produced. Ethnographic presence in
prison revealed the importance of storytelling as a form of social action in this context
(Sandberg, 2010), and even the role of stories in motivating actions (Presser, 2009).

For backpackers, visiting prison was a way to produce adventure narratives (Noy, 2004).
Many accounts can be found in many of the blogs online about prison visiting (Fleetwood
& Turner, 2017). Their online stories about prison visiting describe daring, transformation
and risk-taking. Paul (and others) were aware of this - they played up to stereotypes,
telling stories about the prison and themselves. The stories told by prisoners to each other
had an extremely important role in prison life, including securing reputations, including a
reputation for violence and skill at drug trafficking. Lastly, the account presented here is a
particular genre of research story: a fieldwork 'confessional' (Van Maanen, 2011). Like
inmates, and prison visitors, my venture into prison was undertaken with the aim of
writing about it afterwards. One could ask to what extent my behaviour as a fieldworker
was motivated and sustained by the many fieldwork confessionsals I read with such
enthusiasm. That, however, is a question for another day.

Conclusion
Tales of entering the field abound in ethnographic research. Here, I have offered a
discussion of my entry into a globalised field, and how I 'carved out' a role for myself as
fellow foreigner, listener and storyteller. Here, listening to, and recording stories were not
merely about data collection, but were integral for my entry into the field. Prisons in
Ecuador were marked by global hierarchies of mobility, in which storytelling was a way for
foreign nationals to construct themselves as a distinct community in prison (of which I was
granted membership). As an 'active member' (Adler & Adler, 1987), I also played a role in
widening this community by bringing in backpackers, who played a lively - if transient -
role in the community. Foreign national visitors were especially valued by inmates since
they offered a forum for telling different kinds of stories than those which could be told to
foreign national inmates. This was especially true with regards to hard luck, and hard life
stories. Visitors were keen consumers of such stories, and were also highly credulous. All
of this storytelling was rich territory for me, as a researcher, hoping to elicit and record a
variety of stories about imprisonment, but especially about drug trafficking. As Hammersley and Atkinson note, the processes of ‘getting in’ say much about the field (2007). Here understanding the role of storytelling in facilitating my access to the field also revealed the importance of narrative practices in prison. Ethnographic fieldwork is well placed to understand and appreciate the meaning and importance of storytelling in prison. Whilst Van Maanen (2011) and Adler and Adler (2008) note the ways in which ethnographic texts are structured by genre, this paper also throws light on the importance of storytelling in the field, particularly as a means of entering the field.
References


Entering the global field: talk, travel and narrative practice in Ecuadorian prisons


EMOTIONS RE-VISITED: AUTOETHNOGRAPHIC REFLECTIONS ON A QUALITATIVE PHD THESIS USING SEMI-STRUCTURED INTERVIEWS. A TALE OF POLITICIANS, PROFESSORS AND OMBUDSMEN
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Abstract
Crewe (2014: 393) holds that ‘emotions, feelings, and subjective experiences [...] shape our research interests and decisions, and their documentation, therefore, illuminates the shape and findings of our studies’. This article examines the choices made for and during one particular PhD research project - through the course of which this author interviewed out-spoken politicians, persuasive professors and quirky professors. This is done to extend the traditional scientific writing of the published thesis to reveal those ‘bits that most readers want to know’ (Becker, 2008: 90), which - in accord with the aim of this special issue - is to allow early-stage researchers to anticipate ‘how they will “feel”’ (Jewkes, 2011: 64) during the research process and how their work may be influenced by their own subjectivity. Writing this personal reflection has opened up new self-perspectives. In inviting others to pursue these autoethnographic reflections, I hope to encourage early reflexivity and embolden others to embrace the external and internal challenges of criminological work. To this end this article concludes with a set of questions designed to assist early-stage researchers in their reflective process.

Keywords
Autoethnography; emotion; qualitative research; semi-structured interview; early-stage researcher
Introduction

When this author was approached by the then chair of the early- stages and post-graduate researchers working group (EPER) of the European Society of the Criminology with the idea of hosting an autoethnographic panel at the Eurocrim 2014 in Prague, some very careful review of the debate started, in the field of criminology, by Jewkes (2011: 63) preceded any commitment. In light of the ‘tradition of relative silence amongst researchers on the experience of undertaking in-depth ethnographic work’ in criminology and the resultant ‘gap in the methods literature on what to expect when carrying out research in the field’ (Sloan and Drake, 2010: 24) the demand for such a panel was easily apparent. The unprecedented level of attendance reaffirmed the thirst for guidance on ‘how to process and utilise their emotional experiences’ (ibid). As social research is a ‘craft occupation, in a large part “learned on the job” through apprenticeship, experience, trial, and error’ (Seale, 1999: 475), this knowledge gap exists principally for early- stages researchers.

This is where the value of autoethnographic writing as a way of knowledge transfer sets in. To recall a quote ascribed to Otto von Bismarck, ‘Only a fool learns from his own mistakes. The wise man learns from the mistakes of others.’ Accordingly, this author willingly offers up personal reflection for the advancement of others. To avoid the label of narcissistic navel-gazing often ascribed to autoethnographic writings, this fool aims to hop, skip and jump right over the ‘stage’ of evocative autoethnography with its goal of creating a ‘shared emotional space through autoethnographic stories, poetry, images or prose’ to analytic autoethnography with its ‘greater commitment to the critical and analytical spirit of realist ethnography’ that ‘[…] is not so much a method of self-investigation, but a technique of social investigation conducted through the self’ (Wakeman, 2014: 707).

Analytic autoethnography has been selected since this author neither intends to benefit her soul by means of a confessional tale (Peshkin, 1988: 17) nor to perpetuate a ‘sort of positivist fraud’ (Ferrell, 2012: 218) by clinging to the chimera of objectivity. Instead, this article aims for a middle ground along the long continuum that stretches between these two scholarly impossibilities (ibid) hoping to derive meaning by establishing a joint emotional space, created through sharing of lived experience, that forms ‘the basis of analysis rather than constitute[s] the account’ (Liebling, 2014: 484). Since ‘reflexivity is a process which continues long after leaving the field and completing the research’ (Lumsden, 2012: 4), it comes as no surprise that since the memento of academic self-reflection generated for the 2014 presentation in Prague, approximately two years after shipping the thesis manuscript off to the publishing house, this author has since progressed along the road of autoethnographic understanding that shall now be shared with the intent of benefitting others.

This article will first discuss the specific purpose and value of and approach to criminological autoethnography. This is followed by a brief depiction of the research type and an analysis of this author’s motivation for the conducted research. The scrutiny moves on to the revisited emotions experienced during and after the three semi-structured interviews conducted during the fieldwork. It will then go further by also
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including, as (self)-perspectives gained, reflections on research outcome concluding with a set of questions designed to assist the reflection process of early-stage researchers.

**Value, Purpose and approach**

Is it still necessary to defend the value of autoethnography? The answer appears to depend heavily on both the timing of the question as well as the quantitative or qualitative preferences within a specific scholarly discipline. While the autoethnographic turn reached social sciences such as educational sciences well before the end of the last century (Peshkin, 1988; Lumsden, 2012), “[w]ithin criminological research, the positivist roots of the discipline still assert a strong hold, and emotions such as confusion and empathy are more likely to be condemned than cherished. Anomalies and contradictions may well be ignored, made invisible or denounced’ (Farrant, 2014: 465). Due to the courageous interdisciplinary cross-fertilization efforts initiated by Jewkes and others this stronghold is currently under on-going attack.

The purpose of autoethnography, and with it the purpose of this piece, can be determined either from a researcher, or a research-focused perspective. While the former caters more to the interest of early-stages researchers, the latter serves to improve the quality of research itself. While every researcher and research situation is unique, reading the accounts of other researchers may serve as a preparatory instrument. This is due to the fact that ‘rich description is akin to actual experience’ in allowing to anticipate the own emotional response as well as the confusing blurring of professional and personal identities determined by and in turn influencing the “inner worlds” of fieldworkers (Liebling, 2014: 481; Jewkes, 2011: 65). Wherever this ‘discovery of the self through the detour of the other’ (Hunt, 1989: 42) breeches the level of the subconscious and is made explicit, it may prevent an over-emphasis on researcher voice, which still leaves much academic latitude for autoethnographies to ‘vary in their emphasis on the writing and research process (graphy), culture (ethnos), and self (auto)’ (Reed-Danahy, 1997: 2). The acknowledgement of subjectivity humanizes the emotionally sterile research process and enriches the analysis by accepting feelings as ‘reasonable - and hence rational - subjective judgements about objective experiential worlds’ (Yar, 2009: 8). As ‘authors use their particular knowledge and experience to illustrate problems with and failures in extant research’ this acts to fill ‘experiential “gaps”’ (Ellis, 2014: 262). Consequently, “[a]mbiguity, anomalies, complexities, and contradictions rather than being “matter out of place” [...] became fruitful sites of analysis and research practice’ (Farrant, 2014: 462).

The positive outcome of this process, that includes acknowledging the underlying motivations for research interest, is the generation of ‘honest’ knowledge. If we are conscious of how we and others ““I-witness” our own reality constructions” (Spry, 2011:706) it alleviates the inherent flaw that ‘[…] while ethnography can and should be carefully attuned to the dynamic of groups and situations, it cannot be made to be “objective” - it cannot be honestly divorced from the ethnographer’s own reflexive presence in the research process’ (Ferrel, 2012: 218).

How then is this meaningful reflection on the links between our emotional and biographical experiences and the outcomes of our research to be achieved (sic. Lumsden, 2012: 3) since subjectivity in itself is irremovable? While this author did not follow the
approach of extensive (personal) journaling during the fieldwork, the practiced method of continuous informal exchange with fellow researchers in various stages of their own career helped in seeking out one’s own subjectivity that ‘inescapably’ shapes one’s inquiry and outcomes (Peshkin, 1988: 17). Unlike Peshkin this author believes that active subjectivity-monitoring during the research process, while certainly a sensible activity, is not necessarily superior to retrospective analysis. Whilst being embroiled in fieldwork, personal blinkers frequently forbid stepping back even for the benefit of possible adjustments to one’s subjectivity. Restrictions on granted funding often forbid serious changes to research plans. Accordingly, this pre-dominantly (retrospective) self-analysis will combine Ellis and Lumsden’s suggestion to reflect deeply on the personal experience, social location and background (including gender, ethnicity, age, social class, religion, political beliefs and so on) that predetermined the selection of this particular research topic and how these circumstances ‘may have influenced various aspects of the research process’ (Ellis, 2014: 268; Lumsden, 2012: 3).

**Type of research**

The research underlying this autoethnographic reflection was conducted in Germany and the United Kingdom from 2009 to 2012 as a doctoral research project on the proliferation and implementation of prison ombudsmen. Due to the subject matter at hand the research was based on a comparative, qualitative design employing grounded theory methodology. In order to uncover all available information this author conducted semi-structured interviews with all ombudsmen and influential politicians available. The research findings are now published under the title ‘Proliferation and Implementation of Prison Ombudsmen. Comparative Analysis of the Prisons and Probation Ombudsman for England and Wales and the Justizvollzugsbeauftragter des Landes Nordrhein-Westfalen’ (Carl 2012).

As usual for qualitative research the number of available research subjects was rather limited. After trying to contact earlier incumbents of the two ombudsman posts and other politicians discovered by means of the literature review, the author was only able to conduct three interviews due to inaccessibility or ill-health of further research subjects. Reybold et al. (2012: 699) consider participant selection to be ‘one of the most invisible and least critiqued methods in qualitative circles. Researchers do not just collect and analyze neutral data; they decide who matters as data. Each choice repositions inquiry, closing down some opportunities while creating others.’ Reflecting on my own choices, this holds definitely true. While this author did track down the German central political figure for an interview, more of an effort could have been expanded on triangulating the results especially with members of the parliamentary opposition. In light of the introduction of the Prisons and Probation Ombudsman (PPO) as early as 1990, little effort was expended to track down British politicians of that time pointing at the twenty year time lapse as well as financial and time restraints. However, gaining access to an interview partner might have brought valuable information as to the legal transplant process of the institution. In retrospect, closing down these avenues of data collection may have inclined research results to show an overall favorable predisposition to the ombudsman institution. While purposeful selection may be a successful strategy for obtaining rich data, it leaves the young researcher open to unconsciously spinning or even
consciously manipulating the research findings that are constructed through researcher choice and interpretation (Reybold et al., 2012: 700, 713).

While all interviews were semi-structured, some variation as to the interview structures was unavoidable. Two of the research subjects were ombudsmen - one German and one British, with the only politician being a German. All interview situations highly differed prior, during and after the fieldwork. Due to the space constraints of this article the author selected those two interviews providing the richest ground for reflection. Since frequently not only the emotive researcher is lacking from the final research product, but the participants as well (Wakeman, 2014: 707) this author will, unlike in her published thesis, use rich description to mimic the actual interview experience.

Motivation
The research underlying this reflection was undertaken by a female in her mid-twenties freshly out of law school with a well-to-do upper middle-class background in which earning a doctorate degree was considered simply upholding the family standard. That the author chose to specialize in criminal law and criminology almost counted as living out a rebellious streak. And yet, since the thesis research took a comparative law approach and focused on prison ombudsmen, the resulting conglomerate of policy analysis, human rights and international perspective was considered suitable enough. The selection of the research topic resulted - as in many cases - from the arousal of curiosity through chance. In this case the author was, in a blind-process, set the question of the ‘Usefulness of Implementing a Prison Ombudsman in Germany’ as the take-home essay question for her criminology specialization. Albeit knowing that the person having set the exam had previously argued against prison ombudsmen in the German parliament (Bundestag), this author, after faithfully researching this matter for weeks, chose to make an argument in favor - earning not only a mediocre grade from the first examiner but also his scathing red-inked remark in the blind examination process “I am personally offended that the candidate has taken this standpoint”. While this is of course highly unprofessional behavior by the examiner, it ignited this author’s insistent opposition. In retrospect this might be due to the previous stance this examiner had taken during a very small seminar course of ten students on penal law, which he enjoyed running as his intellectual stimulation beside his everyday work as a prison governor. During that course he had benevolently smiled upon this author’s eager application for an essay and subsequent presentation on the set topic of ‘Religion in prison’. According to the examiner in his 20 years of teaching that exact course nobody had ever even considered the topic, but it ‘looked like a suitable topic for a Fräulein (young Miss)’. The excellent mark received for this piece could never blot out the stain of having been pre-judged based on at least gender and social class if not age and religious beliefs as well. Making the most visible, compelling case possible for prison ombudsmen may thus in part be construed as a personal vendetta. Three years of cooling off have eventually kept this author from gifting the resultant book to an unsuspecting recipient.

While umbrage is a strong motivator it would not have been enough to carry this author through the three years it took to complete this PhD. Another strong motivating factor
was certainly to prove self-sufficiency. A doctoral thesis cannot be bought or breezed through by means of plagiarism without severe and highly embarrassing consequences, at least in Germany. Thus, for this author, who is grateful to her parents for supporting her through law school without loans or overly extensive side-jobs, paying for and moving through this career stage was the first experience of ‘independent life’.

Two other motivators have to be acknowledged here. One is that by writing about penal concerns this author felt to take up the family’s active involvement with penal affairs. The latter abruptly ended with the aneurism of the pre-deceased grandfather, Adalbert Schäfer, who was heavily hunted by the German Red Army Fraction (RAF) for being the judge presiding over the Staatsschutzsenat at the Oberlandesgericht Frankfurt and who felt it his duty to keep in postal contact with any willing sentenced prisoners in the hope of encouraging them on their path to rehabilitation. This familial interest in prison life combined with the author’s life-long political curiosity to discover how individual humans shape (penal) policy in reaction to political life events. For an active party member from the earliest possible age (in Germany 16) and an engaged employee with a social media focus supporting a major election campaign, an opportunity to discover the inside story of an individual politician pushing through penal policy was not to be missed. This author now understands that all of these factors formed the motivational back-ground with which she set out on her research.

**Emotions revisited**

In revisiting her emotions the author aims to open up the discussion on how her gender, class and age impacted on her fieldwork - in particular access to the field, the interview situation itself and post-fieldwork negotiation (c.f. Williams and Treadwell, 2008: 66). The reflection will extend to how she ‘used knowledge of the self or the topic at hand to understand what the interviewee was saying’ (Ellis 2014: 268).

**Interviewing the former Minister of Justice**

Accessing a Minister of Justice (MoJ) is always difficult, a situation that is compounded further in the case of a former MoJ fully retired from political life. Consequently, all interview requests to the Ministry or the Länder parliament went either unanswered or were denied. With no googlability beyond the Minister’s years in office, all official routes were closed. However, being a member of the same political party, it was not too difficult to identify a personally known, former fellow MP, who would forward a personal request. That interview request was instantly approved. All of the involved politicians as well as the author were female and while it has never been thus pronounced by the former MoJ, it seemed to the author at the time that the interview was a personal favour granted due to an unofficial female mentoring network, common background in law and joint political affinity.

While the setting in the conference room of the former MoJ’s private law firm radiated a stiff atmosphere and the author had correctly anticipated and matched the professional outfit of the interviewee, which indeed very much adhered to the suit choices in politics and law firms all across Germany, the overall feeling of the interview situation was that of a non-academic, familial atmosphere. Noting the author’s pregnancy the MoJ sprang into immediate mothering mode with the remark that during her confinements she always was
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grateful for every opportunity to pee (sic!) and that the bathroom was right through the door. Even in retrospect this author cannot determine whether this was simply one friendly MoJ or one highly unprofessionally conducted interview. Replacing the situation on a professional footing was definitely outside the competencies of this interviewer. Consequently, after that beginning even with the academic interview structure answers always flowed freely and the interview transcripts show joint laughter in several instances. Whether tactically right or morally wrong, this interview led to some very rich data.

The partial immersion in the community was arguably greater than in all other conducted interviews. Retrospective reflection of this interview case makes it very clear that only the author’s connections to the studied community facilitated access in this instance. The superior understanding of the processes involved in parliamentary work and political realities, including e.g. the concept of a MoJ’s political responsibility for prison deaths that frequently severely endangers any future political career, combined with the extensive literature research done in advance of the interview resulted in a free flowing interview. There was certainly a broad base for emotional identification and possibly even admiration with this particular interviewee that might have been visible for the MoJ, but which proved to be ‘a positive and powerful stimulus in the formulation of knowledge’ (Jewkes, 2011: 69). This interview in particular provided with its openness and the author’s full release of catching quotations astonishing insight into the why and how of ombudsmen implementation and proliferation. At the time the positive atmosphere during the interview, with it being the first of albeit only three interviews, gave this author the confidence to look forward to the other two interviews that were conducted with two ombudsmen incumbents in Germany and the UK.

Interviewing the JVB

The second interview was conducted with the Justizvollzugsbeauftragte des Landes Nordrhein-Westfalen (North Rhine-Westphalian Prison Ombudsman, JVB) and caused very little problem of access. Since the incumbent’s term had ended just before, two potential interview partners were available. While the previous officeholder refused an interview on the grounds of ill-health, the new incumbent was very willing to be interviewed after a period of some months of getting acquainted with his new role. Before taking the new position, the then new, but now deceased JVB was a well-renowned professor for criminology and penology at the University of Cologne, Germany.

In compliance with best practice standards, this author outfitted all interview requests with an explanatory note addressing the intended interview partner and stating what the research was for, that tape-recording was intended and that these interviews would be quoted in the thesis. This was not an issue for the former MoJ or the PPO. The JVB did not comment upon agreeing to be interviewed. On the day of the interview, this author was surprised that per decree of the JVB, who introduced himself using his title of Professor, the interview was to be conducted with the entire staff present. In retrospective this might be attested to insecurity in the early months of his new position, albeit none can be found when reviewing the interview transcripts. The JVB always replied personally, quickly and in-length. However, the unexpected interview situation caused some hopefully covered-up emotional upheaval with the author feeling overwhelmed, under-prepared and out-numbered 4:1. After having the polite request for a
personal interview denied, the author decided to go ahead with the interview in hope of finishing her data collection.

The interviewer began with an in-person re-statement of what the research was for, that tape-recording was intended and that these interviews would be quoted in the thesis. This was acknowledged with a benevolent smile of the grandfatherly figure and the permission given to the ‘girl’ (sic!) to go ahead. While the JVB very willingly followed the structure of the interview, the author felt at the time due to the ‘teaching style’ of the incumbent and the lengthiness of the answers to have entered a remedial criminology class focusing on penal policy. The interview overran the allotted time limit of one hour - proved adequate in both other interviews - by two hours. The transcript shows whole pages with no (successful) input of the interviewer. Even at that time this author was constantly aware that the interview situation was clearly outside of the control of the interviewer. The author has to confess that unlike the first interview with the MoJ - which was left with an added spring in the step of a job, if not well done then at least successfully completed - this interview situation was left feeling exhausted, incompetent and entirely unsure of the quality of the data collected. Closer analysis of the data showed that there was some invaluable information as to the political processes involved in the remodeling of this institution including e.g. the renaming the “Ombudsmann”, so purposefully named by the former MoJ, into the more traditionally German “Beauftragter” in order to reduce institutional independence. All adjustments were influentially shaped by the well-meaning academic opinions of the new incumbent.

After typing up the data into a transcript, this author, following academic standards provided the transcript to the interviewee highlighting the eleven quotations that were intended for inclusion in the thesis. Due to her clear and repeated statements of her intended use of the interview, the author was very surprised when the JVB - unlike both other interviewees - replied that he considered the interview to be confidential and granted for educational purposes only and denied the use of any quotes. At this time the author was well into typing up her research and felt like the last three years of her life were being pulled from under her. In retrospective it might have been possible to write the thesis under the exclusion of this interview, but its argument was definitely strengthened by the inclusion of the nine quotes for which permission was finally given after a week-long struggle involving many emails and uncomfortable phone conversations between the interviewer and the interviewee. Looking back it seems that the interviewee got carried away in this situation falling back into old teaching roles and giving out too much inside information - especially on how much the implementing politicians shaped a young institution to suit their ends using the ideas of a retiring academic willing to take on one last position.

(Self-) Perspectives gained
The reactions and emotions at the time of the interviews were described above. They are much too small and subconsciously enacted to be considered ‘reflections’. However, this author has now twice re-visited her emotions and may thus legitimately be asked about (self-) perspectives gained from this exercise.
The first reflection in 2013 unveiled the extent of the self-deception as to this author’s independence in life and research and has been expounded on above under ‘motivation’. It took another reflection to realize that the predetermination of the author’s academic journey does not negate the purpose and value of her research, but demands it be placed in the proper context so that the readers of said research may judge its merit based on a full account of methodology, challenges of the research and predilections of the researcher. This angle has encouraged this author to extend the description of the research process included in the official PhD publication by means of this article. In listening to the story her research tells about herself, this author gained a better understanding of herself (Jewkes, 2011: 68). Accordingly, the self-analysis has led to a private reassessment of life goals - with some being discarded and others reprioritized.

Concluding with this reflection on self-perspective would neglect the reader’s interest in what may be taken away from this ‘intensely personal and highly individualized experience’ (Jewkes, 2011: 72). What then can be derived from an analysis of the re-visited emotions? How have the experienced emotions during the different interview situations impacted on the findings and their analysis? What are the long-term effects of emotional responses in interview situations?

Reflecting on the MoJ interview, it can be concluded that the emotional connection between the interviewer and the interviewee led to richer results. As to an impact of the emotional dimension on the analysis, it might be considered that the author considered the MoJ’s quotes in a generally positive light by placing them in the favourable context of political realities. Reconsidering the analysis, there would have been equal room for critiquing the noticeably lackadaisical approach to the transplant of a legal concept as a process undergone with the primary intent to save the MoJ’s personal career instead of with any real strive towards prison reform. While this author still believes that the findings were appropriately represented in her write-up, the emotional component certainly affected a limitation on the critique of the individual politician and consequently a possible underrepresentation of the influence of the individuals involved in the introduction on the success of legal transplantation. A long term effect of this interview might be that during her further career this author always strove to commence interviews on a decidedly professional footing before allowing small emotional connections in favour of enriching the data.

The rather more negative emotions experienced during the interview with the JVB had quite a different impact. It certainly made for a more critical analysis of the data collected. The intense discussion involved in organizing the release of the quotations motivated their extensive use to demonstrate the individual and systemic political failings involved in legal transplantation processes. While even in retrospective this analysis seems to be the only possible mode of interpreting the data, it may be assumed that there is a difference in the tone used during the write-up of the analysis. Interestingly, this tone was not itself emotional or critical but rather more formal and professional. This interview had a lasting effect on the author’s approach to the formalities of data collection. While previously the informational protocols were firmly followed, the author has come to understand that these formalities not only shape the expectations and protect the rights of the interviewee but also those of the interviewer. Consequently, when since conducting interviews the
author has made interviews conditional on the pre-signed agreement of (where appropriate anonymized) release of research findings. Additionally, the author has made a point to explicitly inquire whose presence to expect and to diligently research all interviewees’ backgrounds.

**Conclusion**

From this autoethnographic account the early-career criminologist may conclude that whilst emotions impact findings, this can be equally enriching the data and discouraging to the researcher. However, emotions experienced during data collection and analysis, if recognized, must not negatively influence the quality of the research. To increase objectivity, a conscientious effort must be made to step back from the specific research situation. This can be achieved by allowing for some time to lapse between repeated analyses. A loss of data richness can be circumvented by a high-quality early write-up of the data collection process. Due to the above-described experiences, this author encourages an immediate first analysis and a later second (possibly a third) analysis. The eternally ill-fated strive for objectivity may be aided by these questions: How did the interview situation deviate from the ideal scenario? What was my personal influence on this? In what way, on what level did I connect with or felt repelled by the interviewee? How can I avoid/repeat this in the future? How do I feel about this data collection process? What emotions were involved? What findings did I expect? How does the collected data deviate from this? How do I feel about that? What is my first interpretation of the data? Are there other, possibly contrasting lines of interpretation? How would another PhD student, my supervisor, the interviewee or even an uninvolved third party (a radical but helpful image tends to be one’s grandmother) interpret the data? How do I feel about that? Can I perceive an impact of my emotions on my findings or my analysis? Of course, this set of questions neither claims completeness nor represents an obligatory sequence, but may prove a helpful guideline for (self-) reflection. This author’s aim was to set a scene that allows the careful reader to transform the presented personal reflections into an again exceedingly personal ‘understanding of the processes, pleasures, and pitfalls of qualitative inquiry’ (Jones, 2005: 765). She hopes to have at once encouraged the early-stage researcher to embrace the emotional dimension of their research and provided some small assistance in recognizing and handling emotional pitfalls.
References


Carl
HOW BIOGRAPHY INFLUENCES RESEARCH: AN AUTOETHNOGRAPHY
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Abstract
Inspired by Professor Yvonne Jewkes’ plenary speech at the 2013 British Society of Criminology conference, this paper focuses on the experiences of a researcher setting out on her journey into the field of criminological research. In the burgeoning tradition of autoethnography, it will tell the story of how aspects of the researcher’s own ‘biography’ affected the data collection process in a study of ‘hidden’ older illegal drug users in the UK. The research employed snowball sampling and semi-structured interviews and at the outset great difficulties were encountered which were caused, in part, by the researcher’s age, nationality, professional standing and cultural awareness. However, as the researcher’s own personal biography developed over the long duration of the study, some of these problems were alleviated. In the process something of the nature of the participants themselves was revealed, namely their reticence to be too revealing to an ‘outsider’ about their illegal pastime, the manner in which their illegal drug use took place in a specific cultural milieu, and the extent to which they saw their drug use as being an unremarkable and normal part of their life. By the conclusion of the study, a by now older, more experienced and acculturated researcher was able to see this with far greater clarity. As criminology increasingly takes account of researcher biography, the argument presented here suggests that not only can it have significant impacts upon the research process, but even over the duration of a single study developments in the biography of the researcher can alter the nature of their relationship with research subjects and contribute to a greater understanding of them.

Keywords
Research methodology; autoethnography; personal biography; illegal drug use
Introduction

Encouraged by the crisis of confidence in the social sciences engendered by postmodernism, many scholars in the 1980s began to question, amongst other things, the nature of the ‘facts’ and ‘truths’ that they had supposedly ‘found’ (Ellis, Adams & Bochner, 2011: 1). The impact of the researcher’s own background assumptions on the research process, which had hitherto been largely unacknowledged, was belatedly admitted, along with the possibility that so called ‘grand narratives’ (Lyotard, 1984) were not only impossibly difficult to construct, but actually undesirable. It was in this context that the autoethnographic approach, which ‘recognises the innumerable ways personal experience influences the research process’ (Ellis, Adams & Bochner, 2011: 1), began to emerge.

Autoethnography can be loosely defined as ‘an approach to research and writing that seeks to describe and systematically analyze (graphy) personal experience (auto) in order to understand cultural experience (ethno). [...] A researcher uses tenets of autobiography and ethnography to do and write autoethnography’ (Ellis, Adams & Bochner, 2011: 1). It is through this combination of autobiography (retroactively and selectively writing about past experiences) and ethnography (studying the relational practices, common values and beliefs of a culture) (Ellis, Adams & Bochner, 2011: 2) that the approach can contribute to the production of ‘thick descriptions’ (Geertz, 1973: 10) of particular cultures and social phenomena.

When compared with other branches of the social sciences, criminology has arguably been rather reluctant to grapple with the idea that qualitative inquiry has autoethnographic dimensions, and that there are intimate links between the biographies of researchers themselves and the outcomes of the research projects that they engage in (Jewkes, 2013). For Jewkes (2011: 65), the ‘fixations of criminology’ identified by Joe Sim (2004), namely methodology, objectivity, restrained language and appropriate form, discourage any form of biographical or emotional intrusion by the researcher. As Wakeman (2014: 707) puts it, ‘the crux of the matter is this: for various reasons, and despite significant advances in recent years, many criminologists remain hesitant to include much detail of themselves, their life histories and their emotive processes in the presentation of their research findings’. Recently however, there have been signs of a shift in this regard. For example, Phillips and Earle (2010) have candidly outlined the manner in which their own biographies, identity and memories framed their study of prisoner identities. More recently still, Wakeman, in the course of his argument for a ‘lyrical criminology’, suggests that an autoethnography which focuses upon the ‘researcher’s biographic and emotive self’ can potentially ‘significantly enhance criminology’s methodological repertoire’ (2014: 705).

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12 For many academics, uncomfortable in the limelight, this perfectly suits their disposition. Sparks (2002) and Jewkes (2011) have both recounted their reluctance to engage in writings that may have come across as self-absorbed or narcissistic. Of course it is also often defensible to relegate oneself to the margins; Crewe (2009: 488) was reluctant to place himself near the centre of his ethnography, ‘not because my identity was irrelevant to the study, but because my identity was not what the study was about’. Thus it is important to keep in mind that the central principle behind ‘analytic autoethnography’ is not to foreground oneself for the sake of it, but to consider how ‘emotions, biography, and their intersections through research can enhance understandings of any given subject’ (Wakeman, 2014: 709).
In light of this nascent trend in criminological research, this article tells the story (Ellis, Adams & Bochner, 2011: 3) of how aspects of my own personal biography impacted upon the research that I undertook for my PhD and beyond on ‘hidden’ older illegal drug users. Jewkes (2011) has discussed how academics often present their fieldwork and findings as if it has gone smoothly in a way that does a ‘disservice’ to those that follow them, for it obscures the often messy business of doing research. As the autoethnography presented here will show, the progress of this particular research project was far from serene. The next section will introduce the research project and the sampling and data collection techniques that were employed. Following this, I present the story of what happened during the fieldwork, paying particular attention to how my own biographical characteristics seemed to impact upon the sample building and data collection phases of the study. I will explore how, as time passed and some of my own biographical characteristics developed, a number of the difficulties involved in conducting the research were alleviated, at least to a degree. Ultimately, I was able to reflect on my earlier struggles in a new light and see more clearly how they helped to reveal something about the nature of those I was attempting to study (Ellis, Adams & Bochner, 2011; Wakeman, 2014). As such, the article will go on to briefly consider some of the lessons I learned about the target population through this often testing and occasionally rather demoralising process. My experience suggests that developments in the biography of a researcher can, over time, alter the nature of their relationship with research subjects and contribute to a greater understanding of them, and that this process can occur over the duration of a single study.

The research project
The roots of the work that I have been engaged in, on and off, for well over a decade, lie in a PhD (Waters, 2009) that was started in the early years of the 21st century. The sense that inspired this work, derived from personal contacts and experience, was that there are a considerable number of older adults using illegal drugs quietly and under the radar. These people are not the ‘junkies’ of popular imagination, but what Cohen and Sas called ‘mainstream citizens’ (1994: 72) who combine their drug use with a range of other more ‘conventional’ pursuits such as work and family. At the outset of the project, I felt that such individuals’ historical experience with drugs would likely coincide with the flowering of various post-war ‘spectacular’ subcultures (Hebdige, 1979). These erstwhile hippies, punks and the like, whose drug use had begun in the 1960s and 1970s, might well have interesting tales to tell about how they had sustained their use over the long term, how they had integrated it into their lives, and how their use had changed as they grew older. The plan was to locate and interview adults aged 40 and over who had used an illegal drug on at least one occasion in the preceding 12 months, and who were not in contact with either the criminal justice system or treatment agencies regarding their use. Such users are tremendously underrepresented in the vast majority of drugs research, where the focus tends to be upon those who are much younger, or those who are institutionalised in some way and whose use is seen as ‘problematic’. It was hoped to build a picture of these individuals’ drug use ‘careers’, explore fluctuating patterns of use over the life course and examine how drug use had interacted and continues to interact with various aspects of people’s lives such as family, health and employment. Fundamental to the research was the decision to not make contact with potential participants through criminal justice...
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agencies or treatment centres but instead to find ‘hidden’ users. This was because such people are not only the most seldom researched, but are also most likely to have evolved ‘non-problematic’ drug-using careers alongside otherwise ‘conventional’ lifestyles. Ultimately, the aim was to discover something about the nature of hidden, ‘normal’ (Hammersley, 2005; 2011) drug use that has been sustained over the long term and combined with an otherwise conventional lifestyle, and in so doing address something of a blind spot in the drugs research field.

Snowball Sampling
Snowball sampling seemed to offer the best chance of building a sample of ‘hidden’ older users. For Noy (2008: 330), ‘a sampling procedure may be defined as snowball sampling when the researcher accesses informants through contact information that is provided by other informants. This process is, by necessity, repetitive: informants refer the researcher to other informants, who are contacted by the researcher and then refer her or him to yet other informants, and so on. Hence the evolving ‘snowball’ effect’. As Biernacki and Waldorf (1981: 141) argue, the method is well suited for a number of research purposes and is particularly applicable when the focus of study is on a sensitive issue, possibly concerning a relatively private matter, and thus requires the knowledge of insiders to locate people for study. Snowball sampling has been used with some success in the study of illegal drug users, particularly younger users (Kaplan, Korf & Sterk, 1987; Ditton & Hammersley, 1996; Griffiths et al., 1993; Avico et al., 1998; Hammersley, Khan & Ditton, 2002; Notley, 2005; Potter & Dann, 2005; Shewan & Dalgarno, 2005), although not unequivocally (Goode, 2000). It has also been used to build ‘community based’ samples of drug users (Cohen & Sas, 1994).13

Thus, it was hoped to build a snowball from a starting point of a non-probability, convenience sample of individuals who fit the research criteria. Various initial contact points, chiefly friends and colleagues, were contacted in order to locate potential participants to start off the sampling process. In addition, information was sent out to as many people as possible to advertise the research. Colleagues also sent out ‘feelers’ to their own friends and contacts in the field of drugs research. Six to ten initial contact points from which to snowball were sought, on the assumption that this would provide a strong base to work from. Once individuals who met the criteria had been located and interviewed, the researcher asked these people if they knew of anyone else who might be willing to partake; the researcher then chased up these leads. Interviewees were not asked to ‘deliver’ subsequent participants due to a lack of resources (specifically, the lack of a ‘shop front’). In addition, due to the highly sensitive nature of the research topic, it was felt that it was more appropriate for the researcher to be the one in control of the evolving sampling chains, so that discretion could be assured. The use of ‘privileged access’ (Griffiths et al., 1993) or ‘indigenous’ (Power, 1994) interviewers was also considered, but this was not pursued due to a lack of suitable and willing candidates for the interviewer role. The lack of resources at the disposal of the project would have also rendered this option difficult; Griffiths et al. (1993), for example, recruited a team of 23 interviewers for their Department of Health funded study of heroin users, all of whom

13 For a detailed discussion of snowball sampling and it variants, see Waters (2015).
were paid. As Biernacki and Waldorf (1981: 156) suggest, a chain can be continued until it either comes to a natural end or reaches saturation point.

Used in this manner, snowball sampling ‘has proved useful in generating samples of individuals who it would be difficult, if not impossible, to access in a more conventional way’ (Griffiths et al., 1993: 1619). Despite the various problems associated with snowball sampling (inter alia, its reliance on the researcher and its labour intensive nature, the potential for distortion, issues regarding the control of referral chains, and the difficulty of accessing isolated individuals (see Waters, 2015)), it did appear to be the most appropriate sampling method for this research. This was chiefly because it had the potential to provide an effective means of building a sample of drug users without relying on institutional referrals. As outlined above, the need to avoid users engaged with treatment programmes or the criminal justice system was crucial to this research due to the differences ‘between subjects who were in contact with treatment agencies and those who were not’ (Griffiths et al., 1993: 1623).

**Semi-structured ‘life story’ interviews**

Once individuals had agreed to participate, they were interviewed using an in-depth semi-structured ‘life story’ interview. Given the cross sectional nature of the study, and the fact that participants would be met only once, it was decided that a research instrument of this type would be the most efficient way of obtaining detailed, rich information regarding each participant’s drug use both now and in the past. The practicalities, advantages and disadvantages of using such interviews are much discussed (see e.g. Crow & Semmens, 2008) and so what follows is a brief discussion of the most pertinent issues for present purposes.

The interview was divided into two parts. The first consisted of demographic questions, using the same wording as the British Crime Survey (now the Crime Survey for England and Wales). The second part explored interviewees’ illegal drug careers, consisting of broad open-ended questions about past and present illegal drug use, and future intentions regarding drug use. The reasons behind and attitudes towards drug use were also explored. This latter part of the interview was loosely based on the ‘life story’ interviewing method (MacAdams, 1993) with some adaptations rendering it suitable for the purpose at hand. Such an approach has been used in drug research before, for instance by Hammersley and Dalgarno (2013). The semi-structured format was adopted to permit interviewees a great deal of freedom to discuss that which was of importance to them around the issue of their own drug use. It was also felt that such interviews would allow the participant greater control over what they chose to reveal about what is clearly a sensitive topic, not least due to its illegality; participants were taking a risk in speaking to me and affording them some control was one way of alleviating this risk and gaining trust. However, the interviews did actively but sensitively seek to ascertain how drug use had been incorporated into the participants’ lives at various stages.

Of course, interviews of any kind present a range of methodological difficulties. In the first place, as Maddux and Desmond suggest, the reliability of information gathered by interviews is closely linked to the ‘features of the interview itself. […] These include the place of the interview, the legal status of the subject, the feeling of the subject about
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giving information to the interviewer, the skill of the interviewer, and the procedure’ (1975: 93-4). Furthermore, interviews that concern an individual’s life history can be especially problematic. Indeed, it has been argued that researchers should not take voluntary self-presentation by interviewees at face value (Douglas, 1976). However, past research does suggest that drug users tend to provide reliable accounts of their behaviour (Ball, 1967; Maddux & Desmond, 1975; Weatherby et al., 1994; Rosenberg, 1997). The participants in this study did not at all seem to be deliberately misleading or obfuscatory, although clearly this assertion is merely based upon the researcher’s own unreliable subjective impressions and intuitions. Even if my gut feelings about the sincerity of the participants were correct, this still does little to address the issue of honest but mistaken recollections. Yet there is an even more fundamental point here. Because research participants who are discussing their own lives are not disinterested observers of those lives, there is a sense in which any self-presentation is always suggestive of the way an individual has at that moment in time come to view their own personal biography and lifecourse. This applies even to accounts which are somehow inaccurate. Life events are constantly reassessed depending upon one’s current situation; as Clausen argues, ‘the past tends to be reviewed in terms that make it congruent with present circumstances’ (1998: 196). As a result, we might argue that any self-presentation, even one which deliberately or otherwise contains errors of fact, can tell us at least something about the attitude of the participant to their history and to their present. In the end, life stories themselves are constructions, and the interviews that seek to elicit them are ‘social performances’ that can only ever result in partial accounts (Brannen, 2013).

The interview was designed to last approximately an hour; participants were giving up their time for free and so I was conscious not to take up too much of it. Most ended up taking about this amount of time, but a handful were shorter (the shortest, with a particularly reticent participant, being around 25 minutes) and many were longer (the longest clocking in at over 90 minutes). One telling sign of the duration of the project was that at the outset cassette tapes were used for the recordings as although digital devices were becoming increasingly commonplace it remained standard practice to use analogue recording machines. The switch to a far more convenient and user-friendly digital recorder was made part way through the project. Towards the end of the research process, to have remained with the tape recorder would have seemed wilfully perverse.

What happened?
At the outset of this project, when my PhD supervisor cheerily suggested I would find it easy enough to obtain 50 interviews in the data collection phase of the research, I was an inexperienced female researcher in my early twenties embarking on my first substantial

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14 To go beyond an unquestioning acceptance of what an interviewee says can in fact be vital in attempting to draw something of value from this type of research. As Hammersley and Dalgarno have suggested ‘a common misapprehension about qualitative research is that it cannot address issues of cause and effect. Qualitative research can address cause and effect, but it does so by using a structural logic in the data analysis, not by statistical or other comparisons. Part of this logic is not to accept people’s stories at face value, but to analyse them systematically by coding what is said, to identify interrelated issues and themes that were not necessarily apparent to the participants themselves’ (2013: 22).
fieldwork project. I had moved to the UK from my home country of Canada the previous year to undertake MA level studies and had decided to stay on to read for a PhD at the same institution. Aside from a couple of people who had also stayed on to read for a PhD, a couple of tutors and my housemates, I knew very few people in the UK. However, in what may have been a combination of ethnocentrism and plain innocence, my experiences in Canada had led me to believe there were sure to be plenty of ‘hidden’ older illegal drug users simply waiting for the opportunity to be interviewed regarding their proscribed pastime. Many of my acquaintances in my home town in western Canada, including those defined here as ‘older’, were enthusiastic and open users of illegal drugs. Cannabis in particular, though illegal, is something of a cultural norm in Canada, where it has been argued that some types of illegal drug use have become a form of ‘tolerable deviance’, technically illegal behaviour that is seldom officially challenged (Hathaway, 1997: 104). It was with this combination of youth, innocence and naivety that I ventured out into ‘the field’.

Unfortunately, I encountered great difficulty in locating and interviewing potential participants. The first 24 months of the research were spent talking to contacts and distributing information about the research. I dropped the age limit from 50 to 40 years of age in a bid to attract more potential participants, on the basis that I had found some willing participants in their forties and justified by the fact that there is something of a ‘cliff edge’ beyond which little drugs research focusses on those aged 40 and over. This two year effort resulted in only a handful (nine) of initial interviews. Only two of these initial interviews actually led to a subsequent interview and could therefore be considered as ‘snowballs’, or at least the beginnings of snowballs. However, these subsequent interviews did not lead to any further interviews, meaning an abrupt end to the sampling chain. These eleven interviews formed the basis of my PhD (Waters, 2009).

It should be said at this point that it had quickly became quite clear that there were indeed many individuals who fulfilled the criteria for participation; illegal drug users over the age of 40 who are not in contact with the criminal justice authorities or treatment agencies regarding their use do exist, and they exist in some number. Over the entire course of the research, over 100 potential participants were identified, and some participants referred in their own interviews to others who fulfilled the criteria. Yet, in general, the interviewees were reluctant to pass on names and when they did, their friends and acquaintances were even less likely to agree to be interviewed by a stranger. Interestingly, some people were willing to be interviewed by their initial contact, but not by myself. This variant on ‘privileged access’ or ‘indigenous’ interviewing was not pursued, as noted above, due to a lack of willingness and suitability on the part of the potential interviewers, and a lack of resources on the part of the project. Ultimately, rather than utilising snowball sampling, it began to feel that the research was proceeding along the lines of what Groger et al. (1999: 830) termed ‘scrounging sampling’; a series of ‘desperate and continuing efforts, against mounting odds, to round out the collection of individuals with relevant types of experiences we know to exist but have not been able to capture’.

Such was the duration of the project that over its course, gradually my own personal circumstances began to change. In the end I was in a significantly different position to when I began. A decade on from the beginning of my PhD, and now employed as a full
time academic, I was neither quite so young nor quite so Canadian. I had married a local and gained British citizenship. Perhaps the most significant aspect of this with regards to the research was that I had a larger network of colleagues, students, friends and indeed family in the UK. Although I had been awarded my PhD, I had not completely ruled out the possibility of returning to the project. I would occasionally be given the details of prospective participants, and I managed to complete two further interviews. However, these two interviews did not lead to any snowballs. Then, a student at my institution, who I came into contact with through extracurricular work unrelated to the topic of the research project, revealed that they were a member of a group campaigning for the legalisation of cannabis and knew a number of people who fit the criteria for the study. More importantly, her UK-wide group had a social media presence, something almost beyond comprehension when the study started. With this student acting as a go-between and providing a reference, I issued an advert on the groups’ social media pages, and within a few days I had 17 willing participants. I conducted interviews with these individuals within a relatively short period of time for a final sample of 30.

Why did this happen?
As the research progressed in the fitful manner described above, naturally I began to consider the reasons why this was the case. Although several things contributed to the problematic nature of the research (Waters, 2015), it did begin to seem that some of my own particulars were having an impact. Most notably, the intersecting factors of my age and nationality seemed to combine to dissuade many from partaking in the study, or at least compound the other reasons they may have had for not participating. As a PhD student I also had little professional standing or profile, and my cultural awareness of a country that I was still very much a foreigner in was decidedly limited. These factors were also often a hindrance during the interviews themselves. When I returned to the project following the completion of the PhD, progress was much smoother. Developments in my own biography seemed to contribute towards the relative success of the sample building and the process of interviewing, as the following will explore.

My age was particularly problematic when attempting to initiate the study and begin the process of sample building. When the study began, as noted above I was in my early twenties and as a result I was not part of the same peer group as the potential participants. The age gap may also have exacerbated issues of trustworthiness. Indeed, this has been raised in previous research in relation to snowball sampling (Wright et al., 1992). Potential interviewees may have been more likely to trust somebody closer to their own age, with greater experience of life and possibly even drug use. They are less likely to wish to participate in an interview conducted by somebody considerably younger than them, particularly on such a sensitive issue. However, when I returned to the study, I was in my mid-thirties and suddenly just a few years younger than the youngest potential participants. As well as being closer in age, I had managed to acquire a measure of respectability as I could point to the fact that I was a full time academic with a small but growing publication record. Indeed, the advert that I placed online contained a link to my profile on my institution’s website. This seemed to reassure potential participants that the research was being conducted for benign purposes and that I was who I claimed to be.
Several of my final group of participants told me that they had looked up some of my previous work online and had only been persuaded to participate having read it. The second aspect of my biography that seemed to cause problems in the first instance was the fact that I was a foreign national. There may well have been understandable suspicion on the part of a prospective interviewee as to why a foreigner would be interested in their engagement in an illegal activity. Furthermore, as a consequence of being a foreign national, I had no older family members or family friends living in the research area at the time. As a result access to the older age group was again limited, and I had nobody able to ‘vouch’ for me. Accessing participants through family or friends may have also increased the level of trust and confidence a potential interviewee enjoyed regarding the study. As I was limited to recruiting participants through academic contacts in the first phase of the research, the potential diversity of the sample was therefore limited as there was little variance in the seeds that I was able to utilise. As Griffiths et al. have argued in relation to academic studies of drug users, ‘middle-class drug users may be over represented in such studies as they often exist on the fringes of the research community and therefore provide roads of access into the behaviour by exploiting existing friendship networks’ (1993: 1618). By the end of the project, there had been significant changes in this regard. Whilst I was no longer a foreign national, having acquired British citizenship, it was the more subtle changes engendered by over a decade of life in the UK that were most significant in the process of sample building. I had built a much larger network of contacts, academic and otherwise, who were able to recommend potential participants and vouch for me. Indeed, as noted above I was able to add significantly to my sample with the help of a student. Furthermore, my Canadian accent is now much less pronounced. Friends and family back home often comment on how I sound increasingly English, and this undoubtedly helped in the initial exchanges when I was attempting to persuade an interested but cautious individual to participate, for it was one less thing to explain and one less obstacle to overcome.

The early stages of the interviews presented some significant cultural barriers that made effective conversations difficult. Here were people who had once identified as hippies, punks, ravers and so on, whose drug use had often blossomed in these subcultural movements, and who wished to talk about cultural phenomena that I, as a relatively young, middle class Canadian, had very little knowledge of. Frequently participants mentioned bands that I had never heard of, or referenced what I later discovered to be cultural staples, be they print publications, TV shows and so on. Often I failed to follow up interesting leads, as I simply had no idea what the participants were talking about. For example, one participant discussed the work he had carried out in the 1970s for the so called ‘alternative information centre’ movement, of which I had no knowledge whatsoever. He was also involved in what became Nicholas Saunders’ Alternative Guide to England and Wales, which I also had no knowledge of. Thus, a chance to find something out about the intersections of illegal drug use with this individual’s political activism was

15 Not only can this make it difficult to generalise findings, but it also tends to over–emphasise the cohesiveness of the group under study (Griffiths et al., 1993; Atkinson & Flint, 2001) whilst simultaneously overlooking isolated individuals who are not connected to networks, even where those individuals possess the desired characteristics (Van Meter, 1990).
lost. Witness also this fairly typical exchange where a participant was discussing how he and his group of friends became punks in the late 1970s:

‘Participant: You’ve got to remember this was sort of ’77 and we were all into punk. So although we had dope, actually the drug of choice was speed. My mate’s older brother was the heroin addict and could get his hands on anything, so we used to just have speed to go to concerts and stuff, drinking. So actually at the time we used a lot more speed than dope, cause that was the drug we wanted.

Interviewer: How did you first get into speed?

Participant: Er, probably by reading about it in the NME and things.

Interviewer: The what?

Participant: The New Musical Express. I mean, I mean it was the sort of drug that was associated with punk music, so we wanted it.’

In addition, different slang terms for drugs were also an issue and I had to ask for clarification about exactly what substances were being discussed. This did not help in the building of rapport and trust which is vital if snowballing is to be a success. On a number of occasions I also failed to hide my incredulity that participants had smoked their cannabis with tobacco for many years, something that is almost unheard of in Canada. Yet, once again, by the end of the study, these issues were not quite so problematic. Living in the UK for over a decade, becoming steadily more immersed in the culture and au fait with local slang, and marrying an Englishman meant that aspects of life in the UK that were alien to me at the outset of the study were at least a little more familiar.

There were also more subtle issues that hindered some of the interviews in the early stages of the research. Earlier, we saw how it has been argued that interviews are ‘social performances’ that can only ever result in partial accounts (Brannen, 2013). Indeed, it has been argued that interviews are inherently collaborative (Gubrium & Holstein, 2009), or as Reismann (2008) suggests, ‘co-constructions’, and that what the interviewer brings to the table can be as important as what the interviewee says. Throughout the interviewing process, I was extremely conscious of how difficult it had been to recruit each and every participant, and how wary the participants often were of openly discussing their illegal hobby. As a result, and somewhat unconsciously, I generally started the interviews ‘gently’, not wishing to upset my participants. Yet by treading carefully in this way, in some of the interviews there was a distinct lack of energy and it was difficult to get the participants to open up. Trust was often slow to build, which obviously did not assist the process of snowballing. Upon listening to the recordings of the interviews I often seemed shy and standoffish at the beginning of interviews. Compounding this were the aforementioned cultural barriers (for instance, the use of different slang terms for the same drugs), which made trust and confidence even more difficult to build up. Yet once again, by the end of the study, these issues were far less prominent. I was older, more experienced and confident, and with a little more in common with the participants than before. No longer a PhD student embarking on my first tentative steps into research, I was now in possession of a doctorate and in reasonably secure employment. In addition, by this stage I was conducting the research purely out of intellectual curiosity. There was no funding body or sponsor to please and little pressure on the work as there had been when
the interviews were conducted to form the data collection component of my PhD. With comparatively little riding on the work, the interviews seemed more like an opportunity to enjoy a conversation with an interesting individual, a release of sorts from the ‘day job’. This undoubtedly made them proceed more smoothly.\textsuperscript{16}

**Autoethnographic lessons about the target population**

As Wakeman has forcefully argued, ‘analytic autoethnography is not an exercise in narcissistic self-absorbed reflection. […] It is a method by which further consideration of emotions, biography and their intersections through research can enhance understandings of any given subject’. Wakeman suggests that the key question autoethnography should be directed towards is ‘what can the intersections of field experience, biography and emotions reveal about the subject under investigation’ (2014: 709). Three things about my target population are immediately apparent on the basis of my own autoethnography.

Firstly, it is clear that in a project of this nature, there are significant barriers to individuals being willing to participate even before any biographical characteristics of the researcher are taken into account. People were being asked to talk openly about something that they normally keep well hidden, and something that is illegal. Hidden illegal drug users are a potentially vulnerable group who through any disclosure of their illegal activity open themselves up to potentially serious consequences. However my own biography likely hardened these attitudes. If some reticence to participate was both expected and unremarkable, when faced with a stranger with no track record of research, much younger than them, and from a different country, this reticence often turned into a firm refusal to participate. I was told several times by fellow academics that it should be very easy to build a sample of hidden older drug users, and that they knew several who fitted the criteria. Yet time and again, such people refused to participate. One or two potential participants were actually affronted that they had been referred to me in the first place, even where the referrer had been used as the ‘go between’. The desire of hidden older drug users to remain well ‘hidden’ was very strong indeed.

Secondly, something else that became clear as the research progressed in light of my own biography was that this is a group whose drug use is intimately bound up with a wider cultural milieu, which was distinctly outside of my realm of experience. Interviews mentioned or discussed a range of cultural products, including music, TV shows and films, and many discussed whole ‘subcultural’ movements such as hippy, northern soul, punk and acid house. The connections between these cultural forms and the participants’ illegal drug use were clearly significant but often understated and merely hinted at in passing. I lacked the shared language and understandings to probe these links as deeply as I would have wished. However, on the other hand it is debateable whether an older, native researcher would have seen this in the stark terms I was forced to. Previous research has

\textsuperscript{16} In an environment where the ‘impact’ of research is foregrounded and its presentation tends to suggest rigorous, careful planning and execution, it is not only difficult to admit to weaknesses in the data collection during the early phase of the study, but it is also surprisingly difficult to relate how I continued the research study largely down to personal interest and inquisitiveness, and had no real idea where the research would lead.
suggested that snowball sampling has worked well in instances where the researcher is part of the group being studied (Browne, 2005; Duncan & Edwards, 1999). Yet, in this instance, being an ‘outsider’ was of value in that the assumptions and reference points of the participants were novel to me and therefore very easy to see.

Thirdly, both as I attempted to construct the sample and during the course of the interviews themselves, it became abundantly clear that illegal drug was seen as a completely routine and unremarkable part of life for my participants. Potential participants could not understand why I wished to speak to them regarding their drug use, and during interviews it soon became plain that being an illegal drug user, for the overwhelming majority of the participants, was not part of their ‘master status’ (Hughes, 1945; Lloyd, 2010). This resonated with the earlier findings of Pearson (2001), whose ethnographic work on ‘normal’ drug use featured people aged from the early thirties to the mid-fifties: ‘A crucial feature of these men’s lives (and those of their wives, partners and girlfriends) is that they did not think of themselves as ‘drug users’ - it is merely something that they do, or do not do, as an ancillary to other aspects of their lives, whether work or leisure - and who only rarely, if ever, gather together for the purpose of consuming drugs. This was not a ‘drug subculture ‘ in which drugs were a central feature of people’s lives; rather these were people for whom drug use was a peripheral but ‘normal ‘ aspect of life’ (Pearson, 2001: 173). The participants were openly incredulous that somebody would be interested in their drug use, and this incredulity was heightened because I was not part of the same peer group, but a younger, foreign researcher. They were bemused that somebody who was, to them, an ‘outsider’, would be interested in what they regarded as a pretty insignificant part of their life.

Conclusion
This article builds on the blossoming literature on autoethnography and the impact of researcher biography in criminology. It has detailed the difficulties experienced in recruiting and interviewing ‘hidden’ older illegal drug users by a younger, inexperienced, foreign researcher. The snowball sampling and semi-structured interviews utilised by the research were compromised by these facts of my own biography. Yet the difficulties that I experienced in these regards revealed something about the nature of the group under study, namely their reticence to be too revealing to an ‘outsider’ about their illegal pastime, the manner in which their illegal drug use took place in a specific cultural milieu which was figuratively and literally foreign to me, and the extent to which they saw their drug use as being an unremarkable and normal part of their life. Whilst these insights did not come about solely because of my biographical characteristics, those characteristics made it easier to detect the evidence for them. Thus, we can agree with Wakeman that ‘the relationship between the researcher and the researched [is] of significant potential regarding the ways in which we come to know our subjects’ (2014: 719). Yet that was not the whole story. The long duration of the project covered changes in my own life as I grew older and over time this subtly altered my relationship with potential research subjects. By the end of the project, I was more experienced as a researcher and an academic, I had developed a greater network of contacts and acquaintances in the UK, and I had become more culturally attuned. This not only helped me to surmount some of my earlier difficulties in the sample building and data collection phases of the research, but it also
brought home to me precisely why the going had been so tough at the outset of the project, and concomitantly sharpened my appreciation of the lessons I had learned about my research subjects through my earlier difficulties. Through my early failures I had paradoxically come to know something of my research participants, a fact that I only truly realised once I had experienced a little more success. This suggests an additional dimension to criminology’s nascent relationship with autoethnography, which thus far has centred on the claim that researcher biography can significantly impact upon the research process: The impact of evolving researcher biography on research studies should also not be underestimated. The process of personal change that an individual researcher might experience - even within the bounds of a single study - may potentially shift the relationship between researcher and researched in subtle ways that can contribute to a deeper understanding of those subjects.
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CONDUCTING OPEN PARTICIPANT OBSERVATIONS OF BOUNCERS - NEGOTIATING (IN)VISIBILITY IN FIELDWORK

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Abstract
Conducting research on nightclub bouncers involves fieldwork with actors who have limited interest in having the details of their work become visible to third parties. Conversely, it is the specific interest of ethnography to make the invisible visible. Thus, the research process is a constant negotiation of two potentially conflicting logics of (in)visibility. Furthermore, it is shaped by potential risks and the requirements of ethical codes. Focussing on the study of an inexperienced researcher ‘entering the field’, this article provides insights into the themes of 'risks' and '(in)visibility' and shows how the two were interconnected throughout the whole process of the project, from fieldwork through to writing and publishing. It also shows how the researcher became an overt, but discreet participant observer whose fieldnotes contained intended and unintended blanks. The article suggests that ethnographic data always stays incomplete as the researcher partakes in a balancing act regarding what is revealed and what remains hidden. Intentional blanks help to confirm the researcher’s trustworthiness in the eyes of the research subjects, and so are vital in making such research possible in the first place.

Keywords
Bouncers; night-time economy; participant observations; fieldnotes; research ethics.

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Introduction

What should you do if your code of ethics requires you to keep your participants from unforeseen consequences due to your research, while public and academic discourse tell you that your field - nightclub bouncers - is a field full of risks? Similarly, what is the correct approach when important publications within that and related fields suggest that you must put yourself at risk in order to gain valuable insights (Ferrell & Hamm, 1998; Calvey, 2000; Westmarland, 2000; Winlow et al., 2001; Marks, 2004), while others remind you to take your own risks into account (Scarce, 1994) and exercise caution? In such circumstances the task at hand becomes one of negotiating various interests and the logics of the work - on site, at the desk, and in publications.

My doctoral research involved 60 nights of participant observation with bouncers working at three nightclubs in two German cities. I experienced the action-packed side of the night-time economy - and the boredom of endless hours outside these places of amusement. I wrote 500 pages of fieldnotes. Yet arguably the tensions inherent in the work are at their starkest now, as I reflect on the experience of my research subjects and seek to reap the fruits of my labour through the process of publishing. Two antagonistic logics have to be balanced during this process. On the one hand are the interests of the bouncers who allowed me to access to their night shifts but sometimes found themselves conflicting with the law and, so, understandably wished to stay invisible. On the other hand, it is the logic of ethnography to make the invisible visible, and to reveal what is otherwise hidden. It is the nature of our system of research ethics to reduce potential harm for research participants as much as possible - a tool, a filter or a logic gate which helps us negotiate these antagonistic logics. The result of this negotiation is therefore an inevitably partial representation that focuses on some aspects of experience whilst leaving others out; in other words, we are left with a balance of (in)visibility.

This article aims to provide a series of insights into the process involved in creating this balance. Although there is a vivid body of literature on doing ethnography (e.g. Hammersley & Atkinson, 1983; Ferrell & Hamm, 1998; Pawluch et al., 2005; Fetterman, 1989) and on writing ethnographic works (e.g. Clifford & Marcus, 1986; van Maanen, 1988; Emerson et al., 2011), the literature tends to be particularly silent on the interplay between the field under study and the collected data that provide the basis for further analysis and the final representation of the subjects of the ethnography. Thus, the ethnographic process of making the invisible visible tends to stay largely invisible itself. In what follows, the focus will be on the actual process of doing research, and the piece will seek to communicate concrete aspects of my own ethnographic practice. Three sections will follow. In the first, the focus will be on the themes of ‘risks’ and ‘(in)visibility’, and how they are interconnected throughout the entire research process, from the initial forays into fieldwork right through to the publication of results. It will be argued that the interplay between risk and (in)visibility hinges on two basic prerequisites: the need for discretion and the need for trust. The second section will explore how the dimensions of openness, discretion and participation are interconnected at the actual physical site of the ethnography. The final section will show how some of the tensions apparent in the field are recreated and played out within the fieldnotes that are taken. Fieldnotes are often

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18 http://www.soziologie.de/de/die-dgs/ethik/ethik-kodex.html
used as illustrative depictions of research, but it is important to ask what is absent from them, and why. Ultimately, the article will show how an individual ‘entering the field’ of research dealt with the challenge of balancing a number of competing logics in the process of ethnography. Furthermore, it will suggest that ethnographic data by its very nature is incomplete, but that in certain cases it is actually desirable for this to be the case. This is because participants will only agree to take part in such studies and develop the trust in the researcher necessary for a successful project if they know that not everything will be revealed. Ultimately, the central balancing act in research of this nature concerns what is revealed and what remains hidden.

**Becoming familiar with the logics of the field**

At the outset of the research, I was inspired by the literature on methodology and the night-time economy, and a particular set of ethnographic studies that had greatly enthused me (e.g. Simon, 1991; Barley, 2000; Wacquant, 2004; Fassin, 2013). I also had the advice of my supervisors and the code of ethics of the German Association of Sociology to heed. I took the early decision to conduct overt participant observations. This was rooted in pragmatism, as there are only a small number of nightclubs of a very particular type that employ female bouncers, which would have limited my chances of success had I decided to research covertly. Employment of a different nature (such as in Rivera, 2010) in nightclubs would have been at the cost of social and spatial proximity to the bouncers. The decision to research overtly also had an ethical dimension, since the code of ethics under which I worked requires explicit informed consent if possible.

Some researchers have stressed that carrying out participant observations when embedded within bouncer communities is a risky endeavour (Calvey, 2000; Winlow et al., 2001; Monaghan, 2002; Sanders, 2005; Rigakos, 2008). Meanwhile others seem barely concerned at all with this issue (Rivera, 2010; Søgaard, 2013; van Liempt & van Aalst, 2016). Despite this difference, the research draws attention to an archetypal bouncer: a 20-45 year old male, hailing from various socio-economic and ethnic backgrounds, with a certain penchant towards using violence which they are given the opportunity to do through their occupation. The prominence of violence in the existing literature is enough to make one quite intimidated.

The manner in which researchers have legitimated their decision to engage in covert participant observations (Calvey, 2000; Winlow et al., 2001; Monaghan, 2002) is perfectly coherent, and yet I have always wondered how these researchers came to the basic assumption that bouncers were a group that would be inaccessible with overt participant observations. With the sense that my way into the field would yield initial insights into its basic logics (Breidenstein et al., 2013) I decided to challenge this assumption. In short, I simply knocked at the doors of three nightclubs and was not rejected at any. Initially, I picked a random nightclub on a random night to have a try. As it turned out, the first

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19 Only nightclub #1 employed female bouncers. Nightclub #2 never had a female bouncer in its more than 10 years of existence and nightclub #3 only employed female bouncers for particular events.
bouncer I talked to was not only the head bouncer but also a sociologist. Three months later I began my participant observations at that nightclub. I gained access to the second and third nightclubs through bouncers who connected me to the respective teams with a positive recommendation. What was particularly surprising was that I was permitted access to the second nightclub, as I knew that some of the bouncers working at that venue were also local gang members. Even in this case, and despite the perceived risks of the work, it still proved readily possible to gain informed consent in all three nightclubs.

Each of the nightclubs were quite different from one other, but during preliminary discussions and negotiations regarding the research the potential risks were of paramount concern in all three venues. Of particular concern was my risk of receiving an injury and their risk of being held accountable for it. In addition, the venues and the bouncers were concerned that my data might be used against them by third parties, mainly the police. Together, we clarified that they would not be held accountable for my risks and we agreed that I would be prohibited from taking pictures, making videos or tape-recording anything. This went for all three teams, who were all happy for me to take pseudonymised fieldnotes.

Despite this basic agreement, each of the teams had quite different approaches in dealing with their risks with regards to the research. The bouncers in nightclub #1 requested an official letter of informed consent. I offered the same to the bouncers of nightclub #2 and #3 - they laughed fairly hard at me. It was more relevant for them that somebody they trusted vouched for me. Each club employed its own methods of acknowledging risk and ensuring that I was not tempted to cross certain boundaries. For example, the head bouncer of nightclub #2 told me several ‘best of’ stories that sent a very clear - although never explicitly addressed - message during our first personal encounter: ‘We also beat up women, if necessary’. Meanwhile, the bouncers in nightclub #3 had an internal discussion after several night shifts on setting up an approval system of sorts in order to check whether every team member was okay with my presence.

Thus, my route(s) into the field immediately revealed two of its basic logics: Bouncers are familiar with working in plain view of hundreds of revellers (and potential witnesses). But they are simultaneously aware of the fact that some parts of their routine work are potentially of interest to the police, which can have negative consequences. As a result of this, bouncers are extremely keen to remain invisible as individuals whilst executing those parts of their job that might lead to collisions with legal regulations and authorities. Bouncers have a set of tactics designed to minimise the chances of such collisions, such as changing clothes, performing certain activities in dark corners, or quickly disappearing from scenes, which have been described in other studies on bouncers (Lister et al., 2000; Monaghan, 2004) and were also encountered in the course of my own research. I was allowed to ‘look over their shoulders’ and see those aspects of the job that they hide on the condition that I also used cover-up tactics to hide their identities. Simultaneously, my way into the field revealed the importance of trustworthiness, which was assured

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20 Who told me months later: "I have to admit, I’m a bit jealous that I didn’t come up with the idea of writing a PhD thesis about bouncers by myself. I guess it was just too... obvious."
through both the official consent letters, internal techniques of testing and the demands of gate-keepers such as the head bouncers.

**Becoming an overt but discreet participant observer**

It quickly became clear that in the process of negotiating (in)visibility, the dimensions of openness, discretion and participation were extremely important and interconnected at the actual physical site of the ethnography. This section will deal with each in turn.

I saw openness - and transparency - as being vital to the success of the project from the outset and I aimed to ensure that my research process engendered these traits as much as possible. To each bouncer I worked with on site, I explained who I was, how I would collect data, how I would pseudonymise the data, and assured them that they would be allowed to read my fieldnotes to check the pseudonymisation. Some were interested in this information whilst others were not, although I always told them anyway. What did seem to spark more interest was when I told the bouncers that it was not my purpose to evaluate the quality of their work. This can be understood in the context of a public discourse on bouncers in Germany that is focused on violence, (organised) crime and discrimination against particular revellers. Most bouncers found my general research interest in social order and space rather abstract and rather “boooring”, but they seemed to value the fact that I was attempting to look beyond the popular subjects such as violence and the selection practices at the entrance to the venue. I kept the bouncers informed of the dissemination of my work such as my participation in workshops, colloquia and conferences, as well as of my progress with articles and my blog in order to give them a sense of my work on their work.

On the other hand, the fact that I was collecting data through fieldnotes alone reduced the possibility of the bouncers being able to actually see when I was collecting data and what kind of data was being gathered. Nevertheless, there seemed to be a general agreement amongst the bouncers that I too was at work - somewhat - during our mutual nightshifts, although there were some blithe jokes such as “We know that you aren’t writing a book. You just enjoy hanging out with us.” Sometimes bouncers were irritated by the amount of time it would take until “the book” would be published, or they confused my fieldnotes with “the book”. The comments of those who read my fieldnotes ranged from “You’re...creepy. How can you remember all this stuff?” to “200 pages?! So you actually did work the whole time.” In moments like these, it became apparent that to most of the bouncers the very notion of ‘research’ remained vague and somewhat abstract, despite my efforts at remaining as open as possible about the process. Yet my openness regarding my role and my fieldnotes - crucially, what they would not contain - was essential in the construction of trust.

Discretion also proved to be of great importance and the subject of a balancing act of sorts. I realised very quickly that there are subtle nuances in the manner that stories are related, depending on the space and the company in which they are told. The following section from my fieldnotes illustrates this:
Mille [bouncer] says, *I’ll have an appointment with the cops soon, because of this incident when that guy threw a bottle.* Edu [bouncer] looks at him without understanding, *I don’t know what you mean. People threw so many bottles at us last year, which one are you talking about?* Mille tells him some details, Edu answers, *oh yeah... the incident when you smashed up that guy?* Mille darts a disgruntled glance at him, *of course I did not do that.* When Mille tells me about the incident in a dialogue later that night, he confirms that he knocked a guy over. I get the impression that these things are not supposed to be told in public. [X-RAY, night #12]^{21}

What is important about this in the present context is the difference between a public version of the story in which the bouncer in question “did not do that”, and a privately told version in which the detail that had been denied before is reintegrated into the story. So, the story depends on who is asking whom in the presence or absence of whom. Silences and absences are vital here. This happens mostly in the context of stories that contain potentially incriminating information from a legal perspective. Furthermore, there are topics that bouncers avoided discussing even when asked directly. Both of these traits can be interpreted as a matter of discretion and of keeping their control over stories. This is also mirrored in the experiences of other participant observers when they tried to conduct interviews with their research participants (Simon, 1991; Marks, 2004).

This tendency to discretion forced me to adapt, and I tried to ask questions without giving bouncers the feeling of being interviewed, to observe people without staring, to listen carefully while sometimes looking in another direction, to delay until a more private moment to ask questions. In many cases I simply waited for people to talk about situations by themselves and then asked them inconspicuous questions in order to keep them talking. Simply put, I tried to be as discreet as possible and adjusted my methods to a field in which discretion plays an important role. As we will see in the next section, this sense of discretion fed into the fieldnotes themselves. Nevertheless, while I communicated very openly about the general process of my research, I veiled the moments of *doing* research. This meant that during my overt participant observations there were undeniably moments in which my own visibility as a researcher was reduced. Once again, I was involved in a balancing act of sorts.

The process of participation itself also raised issues. The bouncers did not know what being a participant and hosting a participant observer would entail, and I did not know what being a participant observer would entail, so we had to find out through practice. Three teams, three times. I stood next to them while they carried out their work, tried to follow them as much as possible, chatted with them, asked questions, felt tired, cold, hungry, and bored with them. Participation also meant to become familiar with each other. It was the quiet, subtle shift from “Will you come back next weekend?” to “Where were you last weekend?” It meant that bouncers got used to having me around and I learnt to find a good position to observe from without blocking the way. It also meant to become a node in the web of relationships on site. Of course, building relationships is not

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^{21} All places and persons are pseudonymised; X-RAY and DYSTOPIA are the pseudonyms of two nightclubs.
a single-sided process (Hammersley & Atkinson, 1983). Research participants are observing, too, especially when observations are an important part of their business. Bouncers also got to know me, be it on a professional level - How are her reactions in certain situations? Is she trustworthy? - or on a personal level. It was fundamental to the success of the project to make myself visible, to give personal insights, to share thoughts, and to accept mistakes and jokes at my expense. The bouncers’ comments about me and aspects of my life were sometimes closer to the truth than I would have liked and they clearly showed that I was not the only observer on site. Becoming a trusted part of the network of relationships also required me to keep my distance in some moments, to stay behind a little in heated situations so that nobody would need to take care of my safety. I also distanced myself if bouncers wanted me to take a position in internal conflicts. And I firmly refused to give information in some cases: Two teams worked in the same city and they asked questions about each other, so I told them that I would not be able to answer their questions due to data protection. All of this was fundamental to my role on site.

However, the fact that I had clearly communicated the purpose of my research and positioned myself as somebody with a somewhat detached professional interest provided me with some advantages. Whereas the few female bouncers I met during my research had to struggle in the predominantly male teams (see also O’Brien et al., 2008), I enjoyed the privilege of fools, the privilege of not needing to fit in as a colleague (Marks, 2004). I kept Barley’s (2000) ironical pragmatism in mind; the researcher would most likely be seen as an eccentric coot, and I expected to and accepted that I would feel embarrassed, clumsy or ‘uncool’ on a regular basis (which I did). I did not aim to establish a particular role but rather to simply establish myself as trustworthy. Taking this approach helped me to relax and become the “trusted outsider” (Bucerius, 2013). And, unlike in Bucerius’ case, my gender was hardly made a subject of discussion, especially in the all-male teams. 22 I tended to be addressed as an observer at work. Sometimes this was in the sense of an extra pair of eyes that might provide additional information, sometimes it was in the sense of an extra pair of ears that could be fed with all the stories everybody else on site was tired of hearing, and sometimes it was in the sense of providing an external perspective on one's work.

In hindsight, one of the most difficult situations occurred when the question was raised by some bouncers in nightclub #3 as to whether I had the potential to be an indiscreet and thus harmful observer. I had already spent several nightshifts with some bouncers at that nightclub and had showed up for another night. Unfortunately, no bouncer on that particular shift knew me in person, even though some had already heard of me. This caused some confusion, so one of the bouncers asked me to name him all of his colleagues who I had already met in person. Then he left the entrance area, whipping his cell phone out. Shortly afterwards:

22 A bouncer shrugged his shoulders when we were discussing this: "Come on, bouncers are not a bunch of guys that had been living on a lonely island for months. Bouncers work in nightlife, there's plenty of women. And you were obviously not interested in us as individual men but in our work and us as a group."
Demir [bouncer] comes back and says loudly, *she's alright*. He grins at me, *sorry, but I had to counter-check this, not that you’re a cop in civvies*. He goes back on his position and seems to receive a message shortly after, reads it, nods again and turns around to me with a grin, *JC [bouncer] says hello to you. No offence, but you could be a cop or someone from the press.* His phone will ring several times in the course of the night and it will always be bouncers who know me already. When Jan [bouncer] is on the phone, he just says, *the issue is over, Jan! Thanks!* and hangs up. He explains to me, *I rang everybody you named.* [X-RAY, night #4] 

It probably took only a few minutes until Demir came back, but they seemed endless to me. I felt deeply uncomfortable and embarrassed and tried to console myself with gallows humour: “Well... deal with it. At least this is interesting data that came into being right now...” Then, in a stroke of bad luck, it was reported that a female agent’s cover had been blown in another city and the case gained some media coverage. Despite this development and the events of [night #4], the team decided to accept me as a participant observer. Still, this resulted in weeks of comments and jokes about me as “cop in civvies” which were meant to tease me but also to remind me that although I was the observer, I was also being closely observed. I emphasized several times that I would leave immediately if somebody ever felt uncomfortable with me, ensuring that the ball was in their court, but nobody took me up on the offer. So, I laid down my arms and dealt with the jokes.

The fact that several other members of the team had already spent quite an amount of time with me and had come to trust me certainly helped as I could rely on those relationships that had been built during the nights before. I also considered my openness and the strength of my relationships with the teams as a crucial part of my self-protection. I was sure that my network outside the field would not be helpful in the case of threat, but that there were always bouncers on site who would put in a good word for me. Furthermore, I assumed that those bouncers who might have the potential to become a threat would either not agree to take part in the first place, or that they were less likely to become a threat if they had been given the opportunity to consent to being part of the research project. As my analysis has shown (Preiser, 2016), the feeling of being tricked is one of the main factors in bouncers turning to threat and violence, so my openness was an appropriate choice to help prevent this feeling.

**About blanks in fieldnotes**

Writing is always a process that involves decisions on what to include and exclude (Clifford, 1990; Marks, 2004) and protecting research participants is - among other things (van Maanen, 1988; Emerson et al., 2011) - one of the main factors that shape such decisions in this type of work. The main concern of the bouncers was that my fieldnotes could fall into the hands of the police or others who could do them harm of some sort, so I used this concern as a ‘vanishing point’ of sorts while writing the fieldnotes. Consequently, I pseudonymised all places and persons immediately while writing and did not include information that was a part of criminal proceedings, whether settled or ongoing. The quoted episode in the above section is an example of this. Whilst including an outline of
the incident in my fieldnotes, I excluded all details regarding the forthcoming criminal proceedings as well as any details that might have affected the process and outcome of those proceedings. I wanted to avoid the possibility that somebody may be able to decode my pseudonymisation. Thus, I deliberately left blanks in my fieldnotes to minimize the risk for the research participants, which itself is an acknowledgement that simply partaking in this research carried an element of risk for the bouncers.

The following two episodes also contain blanks:

Arthur [bouncer] and I stay for quite a while outside in front of the door and talk about ideas for the future, our biographies, concepts of lifestyles, gender equality and having children. [DYSTOPIA, night #9]

Mille, Edu [bouncers] and I are sitting around the mushroom heater, Nando [bouncer] is leaning against the counter, his arms folded on the counter, his chin on his arms. Mille and I had been talking about books for a while, then the conversation switched to relationships and became pretty private because of a question Edu had asked Mille. We talk in the circle - mainly Mille and me, spiked with comments of the other two bouncers - about relationships, experiences, failure, and expectations. The till is closed in the meantime and Edu sits behind the counter, but the conversation is not disturbed by this interruption. We talk about an hour like this, sometimes guests leave, or two, three new guests arrive, that's it. Finally, the conversation returns to jocularity. [X-RAY, evening #20]

The fieldnotes in both examples provide details on the protagonists involved, the spatial positions of the protagonists, the context of what is happening simultaneously (in the second example), the fact that nothing else seems to attract the bouncers' and the researcher’s attention at this point, and the topics the protagonists are talking about. It is also possible to gather something about the nature of the relationships between the protagonists as they are involved in conversation about intimate subjects. Yet at the same time, the fieldnotes stay silent on the details of the conversations and the personal opinions and experiences of the persons involved. These conversations are both part of building relationships and manifestations of those relationships. Publications on bouncers have tended to focus on the action-packed aspects of their nightly routines (e.g. Winlow et al., 2001; Monaghan, 2002; Hobbs et al., 2003; Calvey, 2008; O’Brien et al., 2008; Preiser, 2016), but a lot of hours “working the door” have the distinction of being uneventful and boring (Sanders, 2005), such as in the two episodes above. It is these hours that leave room for extended, sometimes very intimate conversations, either between the research participants or between research participants and the researcher (Marks, 2004).

Over time, I learned much about each team. This was not only regarding their routines and working practices, but also some potentially sensitive details about indictable incidents, personal information, job-related information, informal work hierarchies and conflicts within the team, and information from other spheres of life; love, family, sorrows, hobbies and problems at the daytime job (see also Marks, 2004). The fieldnotes aimed to show the relationships and the topics that were covered in conversation, while at the same time
handling the private information with which I had been entrusted, in a sensitive manner. Thus, the fieldnotes are highly detailed on some aspects of the fieldwork, whilst remaining vague on others. While the blanks in the episode quoted in the previous section exist to protect persons as legal subjects, the above two episodes are redacted to protect people's privacy more broadly. In addition, the blanks are also there to be recognized by the bouncers, to further establish my trustworthiness even after the fieldwork on site and to show that their credit of trust was justified. On occasion this led to confusion: One bouncer who had read my fieldnotes offered to fill in some of the intended blanks until I explained their purpose.

There are also blanks of another type within the fieldnotes as the next example shows:

I had been roaming with Erik [bouncer] in front of the door, we stayed outside for a short time and then I follow him back inside. Simon [bouncer] paces up and down in front of the counter. I exchange a smile with Pete [bouncer] and am distracted for a moment. As I turn back, Erik is gone, he's in front of the 361° [neighbouring nightclub] where a jam has formed and two men have been brought down to the floor. One of them is lying on his belly and is held down by one of the 361°'s bouncers, the other one is lying on the side, while another 361° bouncer has one knee on his side. [DYSTOPIA, night #10]

This episode shows how quickly situations can seemingly change from the routine and normal to red alert. However, this is always from the point of view of the observer, who has a necessarily partial view of matters. The speed of the incident in this example could have been a real phenomenon, or it could simply have been that the observer had failed to notice that something was going on until it had boiled over. Thus, the fieldnotes depict a reading of the incident, but not the reading of the incident. In many regards they remain frustratingly vague. The researcher (even having being witness to the incident) and the readers of any subsequent publications are left with many open questions based on the lack of information: Who are the two men? How and when did Erik take notice of the incident? When and why did the incident start? Who brought the two men down to the floor and how? Does Erik even know much more than the observer? On the other hand, the fieldnotes provide fairly detailed information: the number and spatial positions of the main protagonists, the imbalance in control and power between the protagonists, the potential cooperation between the bouncers of two different nightclubs.

Of course, it is inevitable that a single observer cannot capture every detail, and that their perspective is necessarily a partial one. This inevitability was however exacerbated by the conditions on site and the fluid and often simultaneous nature of events and number of protagonists involved. In many cases I joined an encounter after it had already begun because I had followed another bouncer somewhere else. I could not clearly hear what was being said because of the noisy environment. Other people blocked my line of sight or attracted my attention. Persons disappeared from the scene and came back. After some time I realized that bouncers dealt with the same problems. This became apparent when bouncers needed to orient within the venue, discussed incidents afterwards, and exchanged ideas and interpretations of how and why a situation had started and who was implicated. Thus, my own difficulties mirrored those of the field I was studying, even
though many of the bouncers were definitely better than me at making sense of the jumbled environment.

Thus, unlike the earlier episodes referred to, the blanks in the previous episode are unintended, and result in large measure from essential features of the fieldsite as well as the partial perspective of a researcher working alone. This means that certain things remained invisible in the fieldnotes because I simply could not do better. I realized in the course of writing that my memory was unconsciously trying to fill in blanks and it was challenging to leave them as they were. However, the passing of time was an important factor in making the data more complete (Schatzmann & Strauss, 1973), as similar situations would repeat, resulting in similar reactions, even though each incident was unique in its details. Additionally, some situations had an aftermath, be it in actions or in discourse. Time made it possible to depict situations and their course.

**Conclusion**

It is clear that participant observation is shaped in a constant interplay with the environment of the field under study, an interplay that starts with the first assumptions and first interactions with potential research participants and continues on through to the publication of results. Previous work from various countries (e.g. Calvey, 2000; Winlow et al., 2001; Monaghan, 2002; Hobbs et al., 2003; O’Brien et al., 2008; Rigakos, 2008; Søgaard, 2014; van Liempt & van Aalst, 2016) has shown that a range of quite different approaches to participant observation with bouncers are available. Since I chose to conduct overt participant observation for practical and ethical reasons, the first task was to find out whether this was feasible and under what conditions. To my surprise, many bouncers were happy to let me take part in their professional lives and also opened up on a personal level in the course of time. However, they disapproved of any kind of digital recording, be it photographs, videos or audio recording, and were only comfortable with allowing pseudonymised fieldnotes. In other words, bouncers did not want to be clearly identifiable as individual persons (which would be possible with digital recordings), because this could potentially have consequences in the event that they became legal persons. As a result, the sole data source for the project was fieldnotes written from the personal memories of the ethnographer.

At the same time, by adapting to the conditions of the bouncers it was possible to become part of the scene and to collect rich data. As other researchers have shown time and again, relationships of trust play a crucial role in this field (Calvey, 2000; Winlow et al., 2001; Monaghan, 2002; Hobbs et al., 2003). Those who decided to conduct covert participant observations had to negotiate how they would be trustworthy colleagues and ethical researchers, while constantly fearing the risks that accompanied their deception of their research participants. In contrast, by using openness, discretion and participation I was able to largely protect myself from some of the risks that other researchers have faced. Of course this did not save me from the risk of receiving an injury on site, but this was relatively low compared to the risks faced by those who have researched covertly. Indeed, it was part of my trustworthiness as an observer to learn and to know when to better stay behind.
Finding a role and becoming a node in the web of relationships on site was an essential facet of the research. It was just as important to safeguard some of the knowledge that went along with these relationships. This meant that I included intended blanks within my fieldnotes in order to protect research participants as legal persons. Furthermore, intended blanks helped to protect the intimate aspects of these relationships and to reassure the participants that they were not risking exploitation (Marks, 2004). Yet these blanks also brought into sharp relief some other important aspects of the work, such as the necessarily partial view of the researcher. Ultimately, the aim was to ensure that none of the research, including outputs such as this one, could contribute to any negative impacts upon the research participants.

Finally, in negotiating the rather broad requirements of the ethical code of my relevant association, an aspect inherent in all ethnographic studies was revealed: ethnographic data always needs to stay incomplete. Partly this is because the researcher cannot do better, but also because the researcher needs to leave intended blanks in order to protect privacy and ensure that, as far as possible, no harm is inflicted upon participants. A willingness to adapt to the logics of the field and to apply cover-up tactics made it possible to do overt research within it. Only a few people will be able to fill these blanks - and hopefully they will feel satisfied in their trust in me. The entire process - right through to the publication of results - is a finely tuned balancing act that involves bringing many things to light, while leaving others in the dark. That some things must remain in the dark may be seen by some as a weakness in the approach; I would argue the contrary, it is the very thing that makes research such as this possible.
References


AFTERWORD: THE CASE FOR CRIMINOLOGICAL AUTOETHNOGRAPHY
Dr Stephen Wakeman, Liverpool John Moores University

It is with great pleasure and a deep sense of honour that I offer you the following thoughts to close this special edition of the *British Journal of Community Justice*. In the spirit of autoethnography I offer them to you here in the first person, and it is pleasing to know that I have to make no apology for this - the fact that you have read this far is testament to our shared belief in the place of the self in criminological writing. In some respects, this is a daunting task; how exactly is one supposed to follow the fantastic collection of essays that precede this brief afterword? It is not an easy job, but one that I am genuinely honoured to do. My goal here is not to review or summarise the contents of the essays, but rather to see if I can offer some thoughts as to why they matter collectively to contemporary criminology and its methodological groundings.

I will not make the mistake of assuming we have gleaned the same insights from the works collected in this issue, they are far too diverse for that, and ethnography - as practiced by the scholars in this volume at least - is too complex to have any sort of fixed meaning ascribed to it. But I can be confident of one thing I hope; that this volume presented its reader with a *learning experience*. As I was taught it, this is what good ethnography (what *real ethnography*) does best - it provides an intellectual space through which the ‘telling about’ of one cultural context enables critical thinking about it, but also about other cultural spaces too. While reading here about recreational drug users, inmates of an Ecuadorian prison, bouncers and political figures, I was struck by the amount of time I spent thinking about heroin addicts in rehab, about mixed martial arts cage fighting, and about casino gambling. In fact, I lost count of the number of times where one of the tales told here guided me into thinking a bit differently about both the group in question, and another (often unrelated) one at the same time. And this right here, this is the beauty of ethnography when it is done well. When presented in the way it is in this special edition, as critical and analytic autoethnography, it permits - it encourages even - its reader to think about their research areas/interests in new and alternative ways. In this respect, the collected authors from above (and certainly the editors too!) have my eternal thanks.

But why is an afterword even needed here then? Why does it matter what I think about these works now? What makes my thoughts so important? Am I not just making the fatal mistake that too many would-be autoethnographers make and drifting off into some sort
of hazy bubble of non-productive, self-obsessed navel gazing? Surely the research projects we have just read about - like all the others we read about in our journals and hear about at our conferences - are not about me, and neither are they about their respective researchers either? Surely, they're about the participants, about the stories that need to be told to change the world? Well, yes and no. For me, ethnography is about telling stories that all too frequently go untold, yes. And it is about helping change the world too, of course it is. But, it is also about me too. It is about its practitioners' worlds, their understandings of them, and the ways in which they might be able to impact upon them for the benefit of others. I think this sort of thing matters, and I think that it matters that we acknowledge it too. The ways in which the authors of the articles in this volume have done so here illustrate why exactly this matters like it does. The pretence towards objectivity that - whether acknowledged or not - all too frequently characterises criminological research needs to be abandoned. This can best be achieved through thinking about ourselves more in our research, and through telling about ourselves in it too; through thinking about ourselves in terms of the good, the bad, and the ugly. It can be achieved by thinking about the ways in which our biographies, emotions, hopes, dreams, fears, antagonisms and desires have coloured our experience in the field, and how this in turn has coloured our understandings of what we saw there and how we learnt from it.

If there is one thing that is made clear in this volume, it is that we can do this as researchers; we can think critically about who we are as people and how this impacts upon our work. Granted, it is not always easy, but at the same time it need not be that difficult either. Let me break another of our outdated writing conventions and pose a question to my reader - did you notice in Carl's essay the brief remarks about studying criminology possibly being rooted in a 'rebellious streak'? Likewise, did it register when Fleetwood opened up her article by quickly letting us know she was travelling abroad as a 'middle-class white kid'? Maybe it did, and maybe it did not, but either way the point here is the same: by simply revealing these things about themselves the authors tell us something of their world that structures their research and in so doing, they invite us to ask similar questions of ourselves and what structures our own. It is the answers to these questions that makes autoethnography the method that it is. It is the answers to these questions that allows autoethnographers to challenge some of our discipline's long-established ways of doing things.

By way of an example, I will revert to my own work rather than make impositions on the biographies/experiences of the authors in this special edition. Asking questions about my past addiction to heroin, in the light of my observations of its use in ethnographic fieldwork, allowed me to attack prevailing theories of drug addiction (see Wakeman, 2014). Because of a willingness to look at the intersections of biography and research, I was able to develop my theory in ways which challenged some of the orthodox academic perspectives on the subjects I study. Whether or not my theory is in the end superior to the established positions I critique is largely irrelevant (and certainly not for me to judge) - the point is, it is different, it is challenging of intellectual hegemony, and it is genuinely dialectical. Tell me, is this not what progressive critical scholarship should be all about? If your answer to this questions is 'yes', then permit me one more - does it not then hold that autoethnography is the ideal method of critical criminological analyses?
But it is more than this still - autoethnography does not just challenge theory, it can also challenge some of our discipline’s long-held power structures as well. Recently I was lucky enough to be asked to speak at a conference on ethnography, and during my talk I asked the audience a quick question. I asked them how many of the voices they hear talking about crime at criminology conferences would they describe as ‘authentic’ (authentic in that the speaker had direct personal experience of what they were talking about)? Everyone laughed. They laughed because the number was so small, and because in the cold light of day, there was perhaps little else to do other than laugh. We laughed together through our shared recognition of the fact that we seem to have built ourselves a discipline so characterised by what Jock Young (2011) called ‘physics envy’ that right from day one, it discourages any incursions from the messy worlds of authentic selves. Our first-year undergraduates are told to refrain from writing ‘I think’ in their earliest teaching sessions, and the process snowballs from there to the point where journal articles contain only brief footnotes on researcher biographies (sometimes not even that), and monographs usually only contain something tokenistic at the back in the form of a ‘methodological appendix’. The knock-on effect of this is reasonably straightforward, yet hugely problematic - the dominant voices in our field become all too frequently detached from the lived realities of its subject matter as understood through experience and research. They also appear to become disproportionately middle-class, White, and male (and it is perhaps for this reason that some of the sternest opposition I have come across to the use of autoethnography has come from White, middle-class men - read into this as you will).

Even with the above in mind however, I do not think this future is all bad. After reading the essays collected here, I actually feel a sense of hope for the future of criminology. If there is one end point to make in this respect then, it has to be this: criminological autoethnography can provide a productive way forward for any criminologist willing to give it a try. It can not only act as a counterweight to some of our field’s core problems, but it also offers us the opportunity to embrace new ways of knowing about our subjects. The real beauty of the autoethnographic approach then is that it can provide a means by which both our discipline’s dominant theories - as well as the dominant, privileged voices that propagate and protect them - can be challenged and transcended. In this respect, and as so comprehensively demonstrated by the essays collected here, it is perhaps the method of a genuinely progressive critical criminology.

The case for criminological autoethnography has been made. Over to you.
References
New justice secretary
Following the cabinet reshuffle by the new Prime Minister, Theresa May, Liz Truss has been appointed as the new justice secretary and Lord Chancellor. There has been speculation in the press about what type of Justice Secretary she will be, following the divergent approaches of her two immediate predecessors, Michael Gove and Chris Grayling.

A search of her voting record reveals that on the vast majority of issues she votes the same way as other Conservative MPs (the exception being hereditary peers in the House of Lords). Liz Truss has, for example, previously voted for ending financial support for some 16-19 year olds in training and further education, for merging police and fire services under Police and Crime Commissioners, for mass surveillance of people’s communications and activities, for a stricter asylum system and for restricting the scope of legal aid (They Work for You, accessed 31st August 2016).

It has been reported that Truss will "press ahead ‘at pace’ with her predecessor Michael Gove’s radical prison reform programme" (Alan Travis, The Guardian, 19/7/2016). However, more recently her commitment to a key element of Gove’s planned policy implementation - problem solving courts - has been questioned.

Problem solving courts: An evidence review
In December 2015, the then Justice Secretary Michael Gove announced the creation of a working group on problem-solving courts. This working group was to ‘examine models of problem-solving courts and advise on the feasibility of possible pilot models to be taken forward in England and Wales in 2016/17’.

An evidence review of problem solving courts has been produced by the Centre for Justice Innovation (24 August 2016). Problem-solving courts aim to bring together the authority of the court and the services designed to reduce reoffending and improve outcomes. The best known example of this approach in England, the Community Justice Court in North Liverpool, was closed by the Ministry of Justice in 2014.

In summary, the report suggests there is strong evidence that adult drug courts reduce substance misuse and are particularly effective with offenders who are presenting a
higher risk of reoffending. Family drug and alcohol courts appear to help reduce parental substance misuse and reduce the number of children permanently removed from their families.

Evidence on the ability of domestic violence courts to reduce the frequency of a perpetrator is ‘promising’. International evidence suggests community courts can improve compliance with court orders.

There is also ‘promising’ evidence to support the application of key features of problem-solving courts to female offenders at risk of custody and young adults, groups for whom multiple and complex needs have been identified.

The report acknowledges practical problems, such as inadvertently drawing more people into the court system (‘net widening’) and inappropriate interventions. It also acknowledges that problem-solving courts do not constitute magical solutions.

However, the review concludes: "Across a range of outcomes, problem-solving courts have demonstrated their ability to make a difference, with the strongest evidence being on drug courts but encouraging evidence elsewhere, notably on mental health and domestic violence."

A reason why problem solving courts are thought to work is to do with procedural fairness: the idea that people involved in the system perceive it as fairer and therefore tend to comply better with court orders.


**HMIP Chief Inspector of Prison, Annual Report**

The new Chief Prison Inspector, Peter Clarke, has produced his first annual inspection report (19 July 2016). The report refers to inspections undertaken during the period of his predecessor, Nick Hardwick.

Clarke introduces the report by stating he has found "that the grim situation referred to by Hardwick in his report last year has not improved, and in some key areas it has, if anything, become even worse". He highlights a number of indicators of the worsening situation inside our prisons.

- Assaults increased by 27%
- Self-harm rose by 25%
- The impact of the rise in New Psychoactive Substances (NPS) cannot be underestimated
- Too many prisoners with serious mental health problems end up in segregation units
- The majority of young offender institutions are unsafe
• Conditions in immigration detention centres are sometimes shocking

Finally, he concludes that the inspectorate needs more ‘teeth’. Clarke argues that individual establishments and government departments should both be placed under an obligation either to accept inspectors’ recommendations or to set out clearly why recommendations will not or cannot be implemented. He recommends that any such explanations should then be open to public and Parliamentary scrutiny.


Chair of the Parole Board on Indeterminate sentences of Imprisonment for Public Protection (IPP)

Professor Nick Hardwick, previously Chief Inspector of Prisons and now Chair of the Parole Board, published his plan at the end of July to address the injustice associated with Indeterminate sentences of Imprisonment for Public Protection (IPP). Approximately 4,000 prisoners remain in prison despite the sentence being abolished in 2012.

Some of his official statement is reproduced here:

"The IPP prisoners must serve a tariff, the period of imprisonment set by the sentencing judge as the punishment part of the sentence, but can only then be released if the Parole Board is satisfied it is no longer necessary for the protection of the public that they remain in custody. In effect that means they can only be released when they can prove they are no longer a risk rather than the Parole Board having to prove that they are a risk.

Concerns that IPP sentences were being used more extensively than intended led to its abolition in 2012 but this was not applied retrospectively to those already serving an IPP sentence. About 4,000 of these prisoners remain in custody today.

The previous Justice Secretary asked me to examine ways in which more of these prisoners could be quickly and safely released. I have not yet had an opportunity to discuss options with his successor and look forward to doing so.

Any changes need to be made carefully. Some IPP prisoners are clearly high risk and likely to remain so for a long time. Prisoners who present a very real risk to the public should not be released. Others present a very low level of risk and should be able to be released as quickly as their cases can be heard under the current arrangements. There is a middle group that it might be possible to release if effective arrangements are made to reduce their risk and manage them in the community."
The Parole Board can make progress on this issue without intervention by government by reducing delays in holding hearings and by working closely with the prison and probation services to ensure arrangements for preparing and managing the release of prisoners is improved.

We think that in this way we could reduce the number of IPP prisoners in prison to about 1,500 by 2020."

The statement goes on to outline legislative or policy changes that would be required if ministers wish to progress further or faster than this deadline.

To read the statement from the Parole Board in full: https://www.gov.uk/government/news/statement-on-ipp-prisoners-from-parole-board-chairman

**Her Majesty’s Inspectorate of Prisons report: ‘Life in prison - contact with family and friends’**

Her Majesty’s Inspectorate of Prisons produced a report at the end of August exploring prisoners’ contact with families and friends. It is part of a series which focuses on daily life in prisons and young offender institutions (YOIs). It summarises literature concerning the importance of prisoners maintaining relationships with the outside world and, in particular, with their family and friends.

The inspectorate’s report concludes with eight recommendations:

1. All prisons should have staff with a specific family support role and this should be overseen by a senior governor.
2. The rollout of in-cell telephones to existing prisons should continue as resources permit and all new prisons should incorporate in-cell telephones.
3. Prisoners should be allowed to receive incoming calls from their children or their carers on a risk-assessed basis.
4. A pilot should be undertaken allowing risk-assessed and supervised prisoners to have family contact through social media and/or Skype. The findings should be evaluated and the results published.
5. Restricted or limited family contact and/or support should not be used as a punishment for activities or behaviour unrelated to visits and/or family access.
6. Prisons should consult with visitors regarding visiting arrangements in order to improve the visiting experience.
7. Prisons should develop a strategy to help prisoners maintain and enhance their support networks.
8. Administrative delays in admission to visits caused by prisons’ own procedures and processes should not impact upon the time length of the visits.

**Recent probation statistics**

At the end of July, the Ministry of Justice published the latest quarterly Proven Re-offending Statistics for adult and juvenile offenders. The figures include people released from custody and people who received a community sentence between October 2013 and September 2014. They relate to the time period before the split of the probation service in the Transforming Rehabilitation initiative.

In terms of overall findings, in October 2013 to September 2014 around 496,000 adult and juvenile offenders were cautioned, received a non-custodial conviction at court or released from custody. Around 128,000 of these offenders committed a proven re-offence within a year. This gives an overall proven reoffending rate of 25.7%, a decrease of 0.7% percentage points compared to the previous 12 months and a fall of 2.7% percentage points since 2003. The rate has remained fairly stable, fluctuating between around 26% and 28% since 2003.

Around 403,000 proven re-offences were committed over the one year follow-up period, with those that reoffended committing, on average, 3.16 reoffences each.

The highest reoffending rate by age group is 39.5%, for offenders aged 10 to 14, the number of offenders in this cohort has fallen by 81% since 2003. The reoffending rate for offenders aged 15 to 17 is second highest, at 37.3%. Less than 1% of all proven re-offences committed over the one-year follow-up period were indictable only a small fall from 2003.

Adult offenders had a proven reoffending rate of 24.7%, representing a small decrease of 0.7 percentage points compared to the previous 12 months and a fall of 2.2 percentage points since 2003. This rate has been fairly flat since 2004 fluctuating between 24.5% and 25.5%.

Around 356,000 proven reoffences were committed by adults over the one year follow-up period. Those that reoffended committed on average 3.16 reoffences each.

Adult offenders with 11 or more previous offences have a higher reoffending rate than those with no previous offences - 45.6% compared to 7.6%.

The proven reoffending rate for adult offenders starting a court order (Community sentence or Suspended Sentence Order) was 33.2%, a fall of 6.7 percentage points since 2003, and a decrease of 1.0 percentage points compared to the previous 12 months.

Prison and probation staffing
The latest NOMS workforce statistics bulletin (4 August 2016) shows that, as at 30 June 2016, civil service employment in NOMS was 43,210 staff in post on a full time equivalent (FTE) basis. This comprises:
- 31,091 FTE in Public Sector Prisons (72% of NOMS Staff)
- 8,756 FTE in National Probation Service (20% of NOMS Staff)
- 3,364 FTE in NOMS HQ and Area Services (8% NOMS Staff)

Overall, there was a reduction of 319 FTE staff in post compared to March 2016 (0.7% reduction).

Regarding prison staff, the bulletin reports that across all prisons there is a shortfall to Benchmark staffing levels of 746 (4.0%) amongst band 3 to 5 Officers and of 554 (10.4%) among Operational Support Grades.

Regarding probation services staff, the number of probation officers in the National Probation Service increased by 101 (3.1%) on the quarter and 153 (4.7%) over the last 12 months to a total of 3,372. NOMS does not record the number of probation staff employed by private Community Rehabilitation Companies.

Interpreting the figures needs to be done in the context of the large decrease in NOMS staff in recent years. Since 31 March 2010, there has been a 23% fall in the number of staff employed by NOMS (equivalent to 10,321 individuals). This figure does not include staff transferred out of NOMS into the 21 CRCs.

To read the latest NOMS workforce statistics bulletin (4 August 2016):

Restorative justice
The House of Commons Justice Committee published a new report entitled ‘Restorative Justice’ at the beginning of September. The report was produced following an inquiry that commenced in November 2015 and which heard from 17 people and received 52 pieces of written evidence.

The inquiry considered the effectiveness of restorative justice (RJ) provision across the criminal justice system, focusing on the services currently available to victims.

The inquiry examined the evidence base for the effectiveness of restorative justice, concluding that there are benefits in both reductions in reoffending and in providing tangible benefits to victims.

The Committee’s main recommendations and conclusions are:
- Restorative justice is well embedded in the youth justice system, although there is further work to be done, particularly in improving victim engagement. They
recommend the Ministry of Justice looks to the example of youth conferencing used in Northern Ireland.

- Problems in data sharing have presented a somewhat intractable obstacle to the development of restorative justice. They recommend the creation and dissemination of a national data sharing template to help speed up the agreement of data sharing protocols.
- There is evidence of mixed compliance with the requirement under the Victims’ Code to make victims aware of restorative justice, and we recommend the introduction of a system to improve compliance.
- The entitlements under the Victims’ Code should be rationalised so they no longer vary based on the age of the offender.
- The Ministry should consult with Police and Crime Commissioners and stakeholders to ensure there is sufficient capacity to feasibly introduce an entitlement to restorative justice under the Victims’ Code.

It is too soon to introduce a legislative right to access restorative justice services but such a goal is laudable and should be actively worked towards. They believe a right to access such services should be included in the Victims’ Law but that provision should only be commenced once the Minister has demonstrated to Parliament that the system has sufficient capacity.


**Youth justice review**

The Taylor review of youth justice had been due for publication in July, but has been delayed. Justice Secretary Liz Truss has told the Justice Select Committee that she needs time to think before responding to Charlie Taylor’s review of youth justice. An article in *Children and Young People Now* magazine quotes a "senior figure in the youth justice sector" as claiming that the Taylor Report is being "rewritten so that it will no longer focus on secure schools".

To read the Children and Young People Now article in full: [http://www.cypnow.co.uk/cyp/news/2002370/truss-moj-needs-time-to-think-on-youth-justice-reforms](http://www.cypnow.co.uk/cyp/news/2002370/truss-moj-needs-time-to-think-on-youth-justice-reforms)

**Review of Islamist Extremism in prisons, probation and youth justice**

In September 2015 the then Secretary of State for Justice commissioned a departmental review to:

- assess the threat which Islamist Extremism (IE) poses to prisons and probation services; and
- assess the capability of the National Offender Management Service (NOMS) to manage that threat.
The main findings of the review have been summarised in a document published by the Ministry of Justice and NOMS. The review states that IE is a growing problem within prisons, and a central, comprehensive and coordinated strategy is required to monitor and counter it. It recommended that the present system under which TACT (Terrorism Act, 2000) and IE prisoners are dispersed across prisons should be reviewed, and consideration given to containment of known extremists within dedicated specialist units.


**HMIP Report on homeless children and young people**

Her Majesty’s Inspectorate of Probation (HMIP) have published Accommodation of homeless 16- and 17-year-old children working with youth offending teams.

The critical report states that one third of homeless 16- and 17-year-old children who were working with youth offending teams had been placed in unsafe or unsuitable accommodation. Inspectors were particularly concerned about the risks those sharing hostel or bed and breakfast accommodation with adult strangers were exposed to. The responsibilities on local authorities are clear due to the landmark ‘Southwark judgement’ in the House of Lords in 2009.

The Southwark judgement clarified the responsibilities of children’s social care services for providing accommodation and support to homeless 16- and 17-year-olds. It led to local authorities and others reviewing their procedures and improved the prospects for those children. This inspection suggests that, seven years on, significant work remains to improve the accommodation provided to some of the most vulnerable children in society.

HM Chief Inspector of Probation Dame Glenys Stacey said:

"...Support for these children needs to be more consistent, effective and in line with the expectations set by the courts, so that they can successfully become independent adults."


**Crime and Social Work Bill**

The Children and Social Work Bill is currently being considered by Parliament. The legislation was proposed as a way of implementing a range of additional safeguards. However, there are a number of ‘exemption clauses’ that would allow local authorities to seek permission to disapply a wide range of legal duties. Some legal professionals and
campaigning organisations, amongst others, are concerned that vital legal safeguards could be lost under the banner of ‘innovation’.

To follow the progress of the Children and Social Work Bill:
http://services.parliament.uk/bills/2016-17/childrenandsocialwork.html
BOOK REVIEWS
Edited by Jake Phillips & Anne Robinson

ETHICS & VALUES IN SOCIAL RESEARCH

Paul Ransome’s book represents a critical and throughout effort to stimulate the reader to reflect about how Ethics, Theory, and Values interact and interplay with the wider socio, historical, and political context. This book is very well structured and exhaustive, and with the very much needed intellectual honesty to make the debate both constructive and inspiring. This book would be beneficial for researchers at any stage of their career and should be a compulsory reading for every academic research method course.

Social research is inevitably connected or influenced by social or personal values and, as researchers, a certain dose of critical thinking and reflexivity is essential. There is nothing wrong to have a starting ‘value position’ or to be entangled in this web of social values; rather, Ransome’s well-supported thesis is that independent and ‘aseptic’ forms of social research are a mirage. The best social researchers can do is to recognise the values and ideals they hold and use them as a point of strength, rather than a limitation or weakness of their own research methodology.

The book is well-structured and, although each chapter could be read on its own, it is best appreciated in its whole: the main thesis of the book is accurately build up from chapter one till the end, bringing in examples from the research world (e.g. Chapter 2 on the Social Research and Professional Codes of Ethics), providing the theoretical and philosophical underpinnings to engage in this critical debate (Chapters 1, 3,4, 5). Chapter 6 enters into the specifics of Action Research and its relevance in the wider debate of the book. Chapter 7 instead looks at how research can be used and affect the social policy. The final chapter is a great conclusion to the whole book, and builds upon the previous chapters to support the reader in the final considerations about the role of the researcher in the wider socio-historical and political panorama.

In particular, the first chapter sets the basis for the whole book by articulating the concept of value and the social theories which underlie them. Ransome describes social research and the underlying values as the effort to reconcile the values drawn from the strong society thesis and the strong individual thesis. The main point of the chapter is that the
values underscoring social research have their roots in more universal moral values. Social researchers are the middle persons between society and academia and, because of that, they ought to develop a sense of intellectual (and moral) honesty.

This thesis permeates the book and the following chapters further develop and extend on it. Ransome does a great job in chapter 2 by exposing how the research ethics is actually influenced by and should take in consideration potent social factors like authority, the relationship between participants, the experience of the researcher, and the need to take responsibility for one's own decisions. Social researchers do not act in a social and ethical vacuum rather, they should consider the welfare and wellbeing of the persons they research as well as the research/professional community which they belong to. This debate is done with a truly admirable intellectual honesty which transpires in the book. For example, Ransome states: “However robust the codes of professional and ethical practice are, social researchers continually need to consider their own positions as social researcher” and that “ultimately it is the personal conscience of the social researcher that moderates ethical research activity” (p.53 italics in the original).

The following chapters discuss the specific cases of evaluation research, and the role of evaluation research in the wider ethical and value debate. Once again, Ransome does the right thing and critically examines the differences in the two approaches, and the advantages and disadvantages of both. His conclusion is that they both have their reasons to be and, in the setting up of the research, one should have clear in mind what their differences are, so that expectations are not disappointed. In fact, while social research responds to specific questions which are framed in a specific socio-historical debate, research evaluations live in a separate and limited time frame. The honesty in setting up the research helps managing expectations and understanding what can be gathered from specific forms of research.

In conclusion, the book of the late Paul Ransome is a great book. He has been virtuously capable of take on the challenge of embarking in the enterprise of discussing ontological, epistemological and methodological issues in a very much needed discussion. It is inevitable to appreciate the highly readability of the book as well as the masterful writing craft of the author in leading the reader through a set of concepts and reasoning otherwise difficult to grasp. It is strongly recommended that every social researcher should read this book at least once: to mature the type of conscience which is most needed to conduct honest and high quality social research.

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VISUAL METHODOLOGIES: AN INTRODUCTION TO RESEARCHING WITH VISUAL MATERIALS 4TH EDITION

VISUAL, NARRATIVE AND CREATIVE RESEARCH METHODS: APPLICATION, REFLECTION AND ETHICS

Visual methods are not greatly used in the study of crime, although arguably they have a great deal to offer criminological researchers, particularly those interested in drawing out new and fresh accounts of individual experiences. Gillian Rose’s classic text, now in its fourth edition, is, of course, a broad introductory sweep across the field, while Dawn Mannay focuses rather more on participatory approaches and the creation of images and personal narratives. The two complement each other in interesting ways and certainly together, along with the work of Claudia Mitchell (2011), should hugely advance the case for the use of visual methods in the social sciences and humanities.

What is immediately apparent from both texts is that ‘visual research’ refers to a wide range of approaches and techniques from simple drawing to reviews of mass images available on line. The focus of interest may vary as well, and Rose takes the reader through research examples involving the reaction to visual materials, analysis of found images, and dominant discourses (in this case with particular reference to museums, art galleries and their visitors). Indeed she sets out a critical visual methodology across 4 sites relating to production, the image itself (for example, its intended meaning), its circulation and what she terms ‘audiencing’. The model is further refined by overlaying three different aspects or modalities - technological, compositional and social, by which she means ‘the range of economic, social and political relations, institutions and practices that surround an image and through which it is seen and used’ (Rose, 2016: 26).

This model provides a framework for Rose’s journey through the diversity of visual methods and analyses. She even suggests that researchers might read the book selectively, concentrating on particular sites or modalities according to their interests and intentions in terms of conducting visual research. While this may be a pragmatic choice for some, it would be a shame to miss the breadth of discussion and the range of research studies presented throughout the book, which are fascinating in themselves, irrespective of their usefulness in developing future research. That said, Rose does clearly intend her work to be of practical use and it is a clear and effective introduction across the field, outlining the relevance of visual materials within cultural studies, humanities and social sciences. I must also note that the book has great visual appeal, not just in the images included, but in the use of text panels and simple colour to draw the reader’s attention to key debates and issues. Given that it is an introductory text, indicators of further reading are particularly helpful and there is a comprehensive companion website specific to this edition (which I confess I have not explored). Updated content means that there is greater emphasis on digital imagery and new technologies available for analysing large data sets, so it now speaks more explicitly to the contemporary visual and cultural context.
My main interest in turning to Rose’s book was to inform a research study that incorporates, but is not primarily based on visual methods. I suspect that is not too different from many potential readers, as Rose anticipates, and in that regard Mannay’s work is more immediately helpful. First, she is embedded in research that is about creating and co-creating images or narratives (the one being importantly the means of getting to the other). And then, her research adopts broadly ethnographic approaches and this enables her to reflect upon the impact of knowledge and relationships over a period of time on producing and interpreting images and other data. She looks beyond the purely visual and identifies writing, clay modelling and other creative techniques, as well as standard interviews, as possible tools for the ethnographer in his or her close-up explorations of the research setting. In fact, her ‘reflections of fieldwork have emphasised the need to avoid a ‘one size fits all’ approach to data production in favour of a more flexible methodology which centralises the importance of choice for participants’ (Mannay, 2016: 104).

It is not that visual methods promise a cure for all the ills of more conventional research methods for either of these authors. They both highlight the challenges of analysis and interpretation, even the problems of selecting from what may be a large volume of data. And both engage with what Mannay (2016: 5) describes as the ‘good, the bad and the ugly’ of doing research on the ground. That in fact is the strength in these books, because the authors impart their own enthusiasm for the visual, but at the same time illustrate with examples of empirical studies what engagement in these methods means in reality. Visual and creative methods may be exciting yet they still demand rigour and, in many instances, require a high level of researcher reflexivity, not least in terms of the power relations that play out in research relationships.

Visual research has to contend with many of the same ethical dilemmas as other qualitative methodologies, but raises additional questions about consent, confidentiality and anonymity, particularly but not exclusively related to dissemination. Rose covers a wider spectrum of ethical concerns, including copyright and legal issues, while Mannay is able to discuss in more depth the feelings of participants in her studies and the implications of their involvement long-term. Again, these feel like complementary debates that in both cases draw on the authors’ research experience.

These are very different books, each distinct in approach and presentation, but each of great use for the prospective visual researcher. I could not choose one above the other as they are perhaps aimed at researchers in different disciplines and, in Rose’s case, at a broader student readership. I hope that criminological researchers do start to explore the possibilities of the visual and extend their repertoire of methods, not for the sake of the particular techniques, but for the knowledge they might reveal. These two books will be invaluable resource material for those wishing to take that journey.

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References
REFLEXIVITY IN CRIMINOLOGICAL RESEARCH: EXPERIENCES WITH THE POWERFUL AND THE POWERLESS
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Lumsden and Winter offer an ambitious, refreshing and insightful collection of essays that span a myriad of research settings, disciplinary boundaries and continents. Through ‘warts and all’ (p. 77) accounts we sit alongside researchers in their close encounters with the powerless and the powerful; from ‘boy (and girl) racers’, football hooligans and those seeking asylum, to international policymakers and gatekeepers holding the metaphorical (and in some cases literal) keys to gaining access to informants on whose participation research depends. We travel from night clubs in Scotland to prisons in Italy, India, America and Argentina, to post-conflict communities in Bosnia-Herzegovina, the beaches of Brazil, Aboriginal societies and even into the online gambling world of ‘advantage players’.

The focus on reflexivity provides a valuable lens through which to examine criminological research methods and practice. The aim of the book is to show that reflexive praxis is not an end in of itself but that it can be put to work. The authors provide a window into the often hidden world of how qualitative research is actually done, in doing so it makes a stand against the resurgence of ‘positivist and normative criminology and crime science’ (p. 2). The authors’ deep, reflexive engagement with the research process brings to light the social character of knowledge production and how their research is more rigorous because of it.

As a doctoral researcher currently conducting fieldwork I found a number of chapters particularly helpful, and it is these which will be the focus of this review. The book is structured into six broad sections. In Part One O’Leary (chapter 2) provides a step-by-step account of how she negotiated entry to the field and the sensibilities required to maintain relationships both during research and in disseminating research outputs. Davies and Peters (chapter 3) highlight the importance of gatekeepers in both the selection of participants and in the ‘management information’ that they are willing (or able) to provide. Reflecting on the preclusion of certain respondents or data raises important questions about ‘who has knowledge, who governs it, and who are those who control which individuals are allowed to generate it’ (p. 44). These chapters provide an excellent overview of how to begin research; and for me highlight the importance of thinking critically about the factors which have shaped the data collected and how I interpret it.

In the next section authors candidly problematize their classed and gendered subjectivities, the impact this has on how they present themselves to respondents, conduct research and how this affects research quality. Poulton (chapter 6) and Brooks (chapter 7) reflect on their performances of the self in their research, why they selected to exhibit or hide certain aspects of their identities. I will draw upon these chapters when reflecting on my research with women experiencing advanced marginality. Le Grand’s contribution (chapter 9) is particularly brave as it reflects on his ‘failure’ to perform the role expected, that of an unemotional, impartial researcher and also expands what can be considered data through reflecting upon difficult or ‘failed’ research encounters.
Part Three considers intersectionalities of race and ethnicity, Glisch-Sanchez (chapter 10) highlights how reflexive practice led to a critical moment which ensured the research was meaningful to respondents and reduced self-selection bias, in research on TLGBQ Latinas/os experiences of 'hate crimes'. Bhatia (chapter 13) reflects on what enabled him to build strong rapport and trust-based relationships with his participants, including his shared cultural identity as non-white without 'Indefinite Leave to Remain', he also reflects on the emotional labour of conducting long-term ethnographic research in spaces of bare-life and in particular explores the difficulties of straddling both academic and volunteer roles. The section concludes with a reminder that we ought to remain critical of how the research agenda is set, a wariness that research does not contribute to epistemic violence in the social constructions of marginalized populations. Wearing (chapter 15) argues for a critical interpretive method in which researchers reflect on their positionality as agents or translators within systems and practices of surveillance and control (p. 199).

Part Four is the shortest section but considers the important topic of risks, ethics and researcher safety. Chapter 16 considers these themes by putting the PhD supervisor-supervisee relationship under analysis, illuminating not only the importance of support, trust and transparency but provides a rarely disclosed account of the unexpected challenges and dilemmas researchers face day-to-day in the field.

In the penultimate section Ferreccio and Vianello (chapter 20) and Lumsden (chapter 21) bring issues of access and gatekeeper behaviour to the fore, these chapters provide much needed advice for researchers hoping to navigate powerful institutions or gain access to hard to reach groups, both chapters challenge what we consider as data in research, reflecting on gatekeepers' behaviour and the process of negotiating access highlight important findings in themselves.

In the final chapter Graham and White (chapter 24) show the importance of a nuanced approach within international criminological research when assessing policy and practice. They argue that spaces in which innovative and progressive practice takes place, warrant our attention even if they sit within a broader system that may sanction inhumane conditions for the many, and their ineligibility to take part in what are still wonderful initiatives (p. 319).

In the current climate within UK academia, in which productivity in the form of measurable and immediate research impact, visible enterprise activities, knowledge transfer (p. 9) is fetishized, this book is an important reminder of the benefit of being present as you conduct research. Authors critically reflect on their reading of the social world to produce high quality research. It shows the value of slow scholarship and carving spaces of resistance to the hegemony of the productive, neoliberal university (Mountz et al., 2015).

I thoroughly enjoyed reading this book; my only critique would be that some chapters felt a little short. I would recommend it as an essential read both for junior researchers entering the field and experienced researchers looking to develop their reflexive gaze.

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References