

## **How biography influences research: an autoethnography**

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## How biography influences research: An autoethnography

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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 and 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 and 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 and 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 and 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'.<sup>1</sup> 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).

In light of this nascent trend in criminological research, this article tells the story (Ellis, Adams and 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 and 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 21<sup>st</sup> 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

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<sup>1</sup> 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).

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 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 and Sterk 1987, Ditton and Hammersley 1996, Griffiths et al. 1993, Avico et al. 1998, Hammersley, Khan and Ditton 2002, Notley 2005, Potter and Dann 2005, Shewan and Dalgarno 2005), although not unequivocally (Goode 2000). It has also been used to build 'community based' samples of drug users (Cohen and Sas 1994).<sup>2</sup>

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 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:

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<sup>2</sup> For a detailed discussion of snowball sampling and its variants, see Waters (2015).

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 and 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 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-presentations by interviewees at face value (Douglas, 1976).<sup>3</sup> However, past research does suggest that drug users tend to provide reliable accounts of their behaviour (Ball 1967, Maddux and 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

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<sup>3</sup> 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).

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 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 naiveté 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 become 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).<sup>4</sup> 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 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?

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<sup>4</sup> 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 and Flint 2001) whilst simultaneously overlooking isolated individuals who are not connected to networks, even where those individuals possess the desired characteristics (Van Meter 1990).



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.<sup>5</sup>

#### *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.

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<sup>5</sup> 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.

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. Interviewees mentioned or discussed a range of cultural products, including music, TV shows and films, and many discussed whole 'subcultural' movements such as a 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 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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