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Contents

Hidden Communities

Critical Issues in Researching 'Hidden Communities'

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> **hide** *vb* **hiding, hid, hidden or hid 1** to conceal (oneself or an object) from view or discovery. **2** to keep (information or one's feelings) secret. **3** to obscure or cover (something) from view.
>
> **community** *n, pl* **–ties 1** all people living in one district. **2** a group of people with shared origins or interests. **3** a group of countries with certain interests in common.
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> (*Collins Concise Dictionary & Thesaurus* 1995, p. 174 & 442).

This Special Issue of eSharp represents the culmination of a two-year postgraduate initiative revolving around research with 'hidden communities'.[1] The focal point of this initiative was a two-day postgraduate conference, entitled *Critical Issues in Researching 'Hidden Communities'*, at the University of Glasgow in October 2008. The event was interdisciplinary in nature, bringing together both postgraduate students and experienced academics from anthropology, criminology, sociology and political science. The articles in this Special Issue represent a sample of papers presented at the conference. In doing so, this created a space for postgraduate students to operate at the cutting-edge of ethical debates within social research.

The Hidden Communities project evolved from a series of conversations between the three of us in relation to our respective doctoral research projects with right-wing extreme groups, youth gangs, and refugees and asylum seekers. We found ourselves

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[1] The conference was organised, led and chaired by postgraduate students, for postgraduate students. Our decision to publish these papers via *eSharp*, itself run by postgraduate students, was a direct result of this principle. The Hidden Communities organising team would like to thank *eSharp*, particularly Laura Tansley, for their support and expertise in helping to make these papers available to the postgraduate community at the University of Glasgow and beyond.

confronting an increasingly similar set of methodological dilemmas and problems in and around issues of research design and access; emotions and personal challenges; power, politics and the role of the researcher. Notable exceptions aside, we felt that published accounts in our respective areas often glossed over these fundamental problems, skating around the messy realities of conducting research in difficult situations.[2] With the sheer bewilderment and inexperience felt by many postgraduate researchers (us included), these issues are hidden from those who perhaps need it most. Therefore, we utilised the concept of 'hidden communities' as a means of capturing our diverse interests, as well as creating a focal point for much needed dialogue and debate within the wider postgraduate research community.

For the purposes of the conference, the phrase 'hidden communities' was used (loosely) to refer to social groups that are difficult to access for the purposes of social research; where issues regarding access, emotions, power, and the politics of representation were particularly sharply posed. For example, right-wing extreme groups may have a constructed public persona which is very different from the back-stage reality, and this can create particular problems which go beyond most standard methodological textbooks. While this definition served as a useful starting point, the concept prompted postgraduate engagement which went well beyond our initial ideas – extending to research on political elites, football hooliganism, child prostitution, drug addiction and homelessness to name but a few. We invited doctoral students and established academics from a range of disciplinary backgrounds to give personal accounts of their research

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[2] For further discussion of these issues see Coffey 1999; Hobbs & Wright 2006; *Journal of Contemporary Ethnography* 2007 36(2); Lee-Treweek & Linkogle 2000; Nordstrom & Robben 2007; Temple & Moran 2006.

journeys, using the concept of 'hidden communities' as a point of intersection between their disparate interests.

While there were significant commonalities of experiences between researchers at different stages, in different fields, and from different backgrounds, the concept of 'hidden communities' also found wider resonance with key sociological and anthropological debates surrounding power, politics, and ethics which went beyond the research site. Just as methodological challenges were posed in ways which went beyond standard accounts, so the theoretical bases of these dilemmas were also brought into sharper focus. It is in this spirit — sharing practical guidance which goes beyond most textbook accounts, and showcasing the talent and energy which will hopefully inform the future of research with 'hidden communities'— that we present the following set of papers.

The first paper by Kathleen Blee, on access and methods, offers a masterclass in practical guidance for postgraduate researchers interested in researching right-wing extreme groups. Drawing on her extensive experience of research with racist groups in the United States, the paper offers a valuable introduction to anyone interested in researching hidden groups, outlining a range of techniques on securing access, avoiding 'disingenuous and evasive talk', and overcoming front-stage performance. In addition, Kathleen offers valuable insights into the nature of 'hiddenness', questioning *who* and *what* is hidden in so-called 'hidden communities', and the right of a researcher to penetrate this 'hiddenness'.

The second and third papers, by Jennifer Fleetwood and Christopher Kidd, speak to the practical and lived realities involved in the 'lonely furrow' of doctoral research. Offering frank and candid accounts of their emotional journeys, the authors share valuable personal experiences which will resonate with many. Drawing on

her experience of researching men and women in the international drugs trade in Ecuador, Jennifer outlines the impact of emotionally engaging with the prison as a hidden institution, and with prisoners as a 'stigmatised' community. Making explicit reference to her status as a PhD student, Jennifer draws on fieldnotes to offer an 'anatomy' of emotions in the field; giving an invaluable insight into her own personal struggles and emotions during her prolonged engagement in solo fieldwork, which necessitated a degree of 'emotional distance'.

From an 'engaged anthropological' perspective, the third paper by Christopher Kidd addresses similar issues of emotional engagement and detachment. Drawing on his doctoral fieldwork with the Batwa community in South West Uganda, whose marginalisation and discrimination are hidden from public view, Christopher gives a personal account of his political and professional role within the research site. In describing his fractured engagements with Western conservation workers alongside his relationships with the Batwa, Christopher develops existing debates within sociology and anthropology relating to objectivity and subjectivity, arguing from personal experience that these are in fact false dichotomies. For Christopher, debate should move beyond these divisions, to recognise the social relations which research with 'hidden communities' demands of the researcher.

Building on the emotional journeys which form the foundation of these doctoral accounts, the fourth paper, by Laura Piacentini, reflects on over a decade of research within Russian prisons. Drawing on experiences from her own doctoral research and beyond, this paper focuses on the often contradictory sets of power-relations involved with simultaneously studying 'up' (with prison officials) and studying 'down' (with prisoners). Connecting these personal and political struggles with a discursive approach to the issue

of 'hiddenness', Laura moves the focus to wider debates in sociology and criminology, questioning the conventions of Western scholarship, and their applicability to non-Western settings. Laura's rich engagement with history and culture in Russia lay the foundations for a more insightful and reflexive understanding of research with 'hidden communities'.

From a perspective rooted in political science, but drawing on ideas from anthropology, psychology and sociology, the final paper, by Jeffrey Murer, broadens the methodological focus beyond the qualitative and ethnographic accounts which predominate in research with 'hidden communities'. Jeffrey draws on his experience of applying mixed-methodological approaches with political activists in Hungary, and youth activists across Europe, to outline some of the strengths and limitations of mixed-methods approaches. While these approaches allow for a greater level of representativeness, and cross-cultural comparison, there are also difficulties of translation, cultural comparability, and depth of focus. By examining these issues, Jeffrey also broadens the theoretical engagement with 'hiddenness' to consider the subjective interpretation of the researcher – questioning the definition of membership within a 'hidden community'.

This collection of papers unearths many common challenges, issues and dilemmas encountered by postgraduate students and experienced academics researching 'hidden communities'. As well as considering *how* a community comes to be hidden, the authors expose the multifaceted nature of 'hiddenness' by questioning whether it is communities, individuals, institutions or practices that are hidden. By analysing their personal experiences across time and space, the authors offer suggestions as to how to negotiate our own biases, identities, preconceptions and emotional responses in what are often unexpected and testing situations. By drawing on both

ethnographic and mixed method approaches, the contributions demonstrate how doing research with 'hidden communities' connects us with the phenomena that we have chosen to study and how this connection permeates across different disciplines and areas of social inquiry.

As a whole, the papers make a significant contribution to the literature on research with 'hidden communities'; offering honest and pragmatic guidance on the myriad unique and sensitive issues involved with such research. This advice is most apposite for postgraduate researchers, who may feel isolated and alone in confronting these dilemmas. Over and above this postgraduate audience, however, we feel that the papers make a contribution to more esoteric debates relating to research on sensitive subjects, opening up the concept of 'hidden communities' in ways which went well beyond our initial thoughts and ideas. The papers connect first-hand research experiences with social science debates on the interplay between engagement and detachment; objectivity and subjectivity; reflexivity and transparency; and the increasingly complex relationship between the global, the national and the local. While each author treads their own path, what unites them as researchers is their engagement with the community they have chosen to research, and their efforts to render the lives of those within it in a way that is at once personal, sensitive and authentic.

Finally, while there are numerous issues relevant to the study of 'hidden communities' not covered in this issue – historical, documentary/archival, or internet-based methodologies, or the practical and ethical challenges of analysis and dissemination – the papers are intended not as the last word on any of the substantive areas covered, but rather as an invitation for further debate, dialogue, and reflection. We offer these papers to postgraduate students, as a

resource to draw on in their own research journeys, and to scholars of 'hidden communities' more generally, as a starting point for progressive methodological and theoretical agendas. Our hope is that the papers presented will contribute to ongoing debates whilst prompting new avenues for further discussion, both within the postgraduate community and beyond.

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Access and Methods in Research on Hidden Communities: Reflections on Studying U.S. Organized Racism

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Introduction

Sociologists have long been drawn to the hidden aspects of social life. They examine a variety of social groups that exist outside the visible mainstream of society, which are variously conceptualized as subcultures, deviants, marginalized populations, sects, and secret societies. Commonly, studies of hidden social groups use fieldwork methods. Interviewing, observation, and participant–observation methods have allowed scholars access to those who are reticent or hostile to being studied (Duneier 2000; Simi & Futrell 2006; Venkatesh 2008).

The use of fieldwork to study hidden communities raises complex issues about the relationship between scholars and those they study. To explore these, I draw on questions posed by feminist ethnographer Marjorie Devault who writes,

> Fieldwork traditions have historically produced knowledge [...] that takes publics 'inside' other realities, helping 'us' to see 'others'. But the scare quotes point to persistent questions about our research processes and the reception and uses of our work: Where do we locate 'the field'? What kinds of knowledge do we seek there? On whose behalf? (2007, p.182)

Although relevant for all fieldwork studies, Devault's questions have particular salience for research on social life that is hidden from public view. In this article, I use her queries as a starting point to ask

three questions about research on hidden communities. First, where do we find what is hidden (in Devault's terms, 'the field')? Second, how can we generate valid knowledge in studies of hidden communities? Third, for whom do we generate knowledge or, put another way, what ethical considerations arise in studies of hidden social life? To explore these issues, I draw on two studies that I conducted on extremely hidden communities, women in U.S. racist groups in the 1920s and women in the U.S. racist movement today.

The Field: Where Do We Find What is Hidden?

Devault asks fieldworkers to consider how they locate the field that they study. For scholars studying hidden communities, the question is more complex. They need not only to identify a field of study but to understand which parts of the field are hidden from view and which are exposed. As importantly, scholars need to consider why and how aspects of the field are hidden. For some groups, hiding is a strategy. Criminals, religious zealots, and wealthy people often seek to remain invisible. Other groups are hidden because outsiders choose not to see them, even if they want to be visible. The experiences of such groups as abused women, people in same-sex relationships, and people with disabilities are among those that have been overlooked historically. Why and how a group is hidden affects how they are studied, as evident in two research projects that I conducted on racist group activists in the U.S.

The 1920s Klan

 The first study focused on the Ku Klux Klan of the 1920s, especially its mobilization of hundreds of thousands of white, Christian, and native-born women into a racist, anti-Catholic, and anti-Jewish crusade for white supremacy. During its heyday, the Ku Klux Klan,

as an organization, was not hidden from public view. Both male and female KKKs operated brazenly in the open. They marched down the main streets of towns and assembled before the U.S. capitol in Washington, D.C. They sponsored carnivals, fairs, spelling bees, and softball tournaments, drawing large numbers of supporters and onlookers. Indeed, in the Midwest the Klan enrolled the majority of all white, native-born Protestants. In Midwestern communities, it had little reason to be hidden.

While the 1920s Klan organizations operated in the public eye, its members hid their identities by donning white masks and hoods. Yet, these masks and hoods were largely symbolic, at least in the small towns and rural areas in which this Klan was strong. In these places, people were well aware of who was in the Klan. Indeed, the essence of the Klan's power in the 1920s was not as much its acts of physical violence as its power to intimidate. Intimidation took the form of massive marches and burning crosses, the Klan's symbol of 'fiery Christianity' in support of white supremacy, but the Klan also intimidated by making sure that people knew just how many of their neighbours and acquaintances were members (Blee 1991; MacLean 1995). Klansmen and Klanswomen gained power through strategies of hiding and making themselves visible.

The Klan operated with menacing visibility in the 1920s, but slipped into the shadows in the decades that followed. When the 1920s Klan collapsed amidst sexual and financial scandals, including a lurid rape-kidnapping by one of its most powerful leaders, its members disappeared off the public stage quickly and most concealed this aspect of their biographies from their descendants. However, Klanswomen disappeared from historical memory more completely than did Klansmen. The histories that were written about the 1920s Klan generally paid little attention to the presence of Klanswomen,

regarding them largely as a curiosity (Rice 1972; Wade 1987). Subsequent Klans in the 1950s and 1960s, composed only of men, similarly had little interest in acknowledging that women had been active in a previous wave of Klan activity. They considered women to be irritants and distractions to the cause of white supremacy and made considerable effort to project an image that the Klan was – and always had been – a fraternity of white Christian men (Blee 2002; Blee 1991). The hidden nature of the 1920s Women's Klan was thus a complicated product of actions and agendas of different groups of actors. Klanswomen wanted visibility for the Klan in the 1920s, but tried to hide their personal involvement from future generations. At the same time, the role that women played in the 1920s Klan was made invisible both by Klansmen who sought to safeguard the male image of the Klan and by scholars who ignored them because they did not regard women as political actors. Hidden communities, as this history shows, can be hidden by different actors for different reasons.

Members of hidden communities may not want to remain hidden. Indeed, some may cooperate with scholars in an effort to shed light on what has been invisible to the public. This was true for the 1920s Klan. Although I was interested in why women joined this Klan, the few documents that survived, largely pamphlets published by the Klan and newspaper accounts of their activity, give little clue as to the motives of Klan members. But as I searched through archives and storage areas of historical societies, churches, and libraries, I became aware that some of its former members, including women members, were still alive. Others warned me that it would be futile to try to find them since they had spent a lifetime hiding their Klan pasts. But I set out to find these former Klanswomen, putting notices in every venue that might lead to them: history

center newsletters, small town advertising circulars, and church bulletins. To my surprise, a few women responded. With the promise of confidentiality, they agreed to talk with me, to expose some of what they had hidden for decades from family members and neighbors. They were hesitant about revealing their pasts, but hoped that my research would correct what they regarded as the unjustly negative reputation that their Klan had acquired. From the outset, then, I and the former Klanswomen I interviewed had radically different agendas. We each were interested in bringing women's role in the Klan out of hiding, but they anticipated a more positive depiction of the Klan than my research produced. Just as scholars and Klansmen both, for different reasons, obscured the history of women in the Klan, so too did my respondents and I have different motivations for making that history more visible.

The example of the 1920s Klan suggests that finding what is hidden is not simply a matter of a scholar's persistence or skill, but can depend heavily on the interests and cooperation of those who are hidden. Just as it is important to question who did the hiding, so it is critical that scholars of hidden communities understand the agendas of those who want to bring social life out of hiding.

Modern organized racism

The question of hidden social life also appeared in my study of women's roles in the modern U.S. racist movement (Blee 2002). Organized racism today is a loosely connected network of (1) small Ku Klux Klan groups that pursue the Klan's traditional emphasis on white, Christian supremacism and xenophobic patriotism; (2) a more active set of neo-Nazi groups, including affiliated young racist skinheads, that focus on Jews as the main enemy and reject allegiance to the U.S. government which they regard as Jewish-controlled, or a

ZOG (Zionist Occupation Government); and (3) miscellaneous white power groups, some of which live in isolated racist communities (Zeskind 2009).

When I began to study organized racism in the early 1990s, most racist groups and activists were not particularly hidden. They feared government surveillance and prosecution and, perhaps equally as much, exposure to rival racist leaders and groups. But they took few precautions to safeguard themselves from journalists or scholars. In fact, many racist groups sought publicity. They staged elaborate rallies and racist gatherings in places across the country and allowed outsiders access to racist compounds like those of the Idaho-based Aryan Nations.

The relative openness of racist group in the early 1990s allowed me to pursue a strategy of access that would be impossible with more hidden groups. Since I wanted to find a broad range of women racist activists with whom I could conduct life-history interviews, I designed a multi-stage sampling strategy. I began with a one-year collection of all publications and propaganda produced or distributed by racist groups anywhere in the U.S. From these data, I identified which groups had significant women members and drew a purposive sample of groups, creating a sample that varied by location, type of group, and characteristics of members. Finally, I contacted each of these to locate a woman activist who would talk with me. This provided me with a sample from which I was able to conduct life-history interviews with 36 racist women.

Although I successfully gained permission to interview racist women, my interviews were interrupted by an event which radically reshaped the relationship of racist groups to outsiders. In 1995, a federal government office building in Oklahoma City was bombed by a reputed sympathizer of racist militias, an attack that claimed 168

lives. In its aftermath, government surveillance of the racist movement sharply escalated and racist groups became increasingly hidden and reluctant to be observed by outsiders. This intensified more when the racist movement was identified as a source of domestic terrorism in the anti-terrorism campaigns that followed the 2001 attacks on the World Trade Center and other places by Islamic radicals (United States Department of Homeland Security 2009). Within a short time, racist groups shut off almost all public access to their members.

With the shift in the larger context, my study of organized racism moved from an examination of social groups that were mostly in the open to a study of groups that were hidden. Contacting racist groups became more difficult as they were now suspicious of the motives of anyone gathering data about them. As their more moderate members dropped out in the wake of government surveillance, racist groups also became more extreme in their ideologies and more dangerous in their actions. Over time, those members who remained were more dedicated to agendas of violence and even terrorism. My study had not only shifted from a subject that was public to one that was hidden, but from a study of those who expressed vile ideologies but posed little threat to researchers to a study of violent groups that saw all outsiders as enemies. Although it might have been prudent to end the project at that point, there was also danger in failing to follow through with racist activists that I had already contacted. If I failed to interview them after receiving their permission, they might conclude that I was a government agent which would put me in physical jeopardy. So I continued interviewing.

My two studies of racist groups are examples of the complicated issues that arise in scholarship on hidden communities,

particularly the issue of what/who is hidden and from whom. In the 1920s as well as today, racist movements are both hidden and open. They are hidden from authorities, and sometimes from scholars. But they need to be visible to potential recruits and to the public they seek to impress and intimidate (Blee 2002; Blee 1991; Mitchell 1993). Thus even organized racism has both hidden and open aspects.

Locating a field of study requires scholars to recognize what is open and what is hidden and to weigh the motives and agendas of those who hide social life or who seek to bring it in the open. Indeed, probing why and how social groups are hidden can reveal important features of these groups (Mitchell 1993). Social groups that are hidden by the acts of outsiders are likely to be relatively powerless. Those that purposely hide themselves have the power to manipulate their visibility (Currier 2007).

Generating Knowledge: Gaining Access to the Hidden

Scholars are able to gain access to many hidden communities, although often with difficulty and by taking risks. This is true even for hidden communities that strenuously guard their privacy.[1] If such access is possible, however, it also can pose problems for the unwary scholar. In many hidden social worlds, those who are most accessible are likely to be the wrong people to study. To return to the example of organized racism, the easiest people to identify in racist groups – and the people who are most willing to talk to scholars – are likely to be the self-proclaimed spokespersons and self-designated leaders who want publicity. Such people can be quite unrepresentative of most racist activists. Moreover, self-styled leaders and spokespersons may have little connection to the racist group for

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[1] Blee 2003; Blee 1993; Huggins & Glebbeek 2008; Jipson & Litton 2000a; Jipson & Litton 2000b; Lee 1995; Lee–Treweek & Linkogle 2000; Sehgal 2007.

which they claim to speak. Scholarship that relies on interviews with or statements by visible racists can give a misleading impression of the overall characteristics of organized racist groups (e.g. Swain & Nieli, 2003).

Access is problematic also because there is a tendency to conflate position with power in studies of hidden communities. Since it is difficult to gain entrée into hidden social worlds, it is tempting to focus on those who hold official positions in a group such as founder, president, organizer, or, as in the KKK, grand wizard. In fact, however, the actual practice of leadership in hidden communities, as elsewhere, can be exerted by those who lack official positions and titles. In racist groups, women often undertake the activities of leadership, providing group cohesion, strategies, and collective identity, although they seldom are given formal leadership positions. Nonetheless, their practice of leadership can be substantially more effective than men's. Male racist leaders generally assert themselves through threats, violence, and bravado. By these means, they secure obedience from members in the short run, even if, over time such practices tend to splinter racist groups and erode the base of the leader. In contrast, women's leadership practices in racist groups tend to be less directive. For example, women are typically in charge of molding new recruits to take on the proclaimed goals of racist groups, such as fomenting a race war, and reshaping the goals of racist groups to fit the capacities of the group's current members. Although women's leadership may be more influential than men's in some racist groups, easier access to racist men with ostentatious titles makes them more likely subjects of scholarly inquiry and media attention (Blee 2002). Yet they may not be in a position to provide the best information about this community.

Scholars of hidden communities, like any fieldworkers, also should be careful about using people they know or those to whom they are initially referred as their primary means of entrée. It is particularly tempting to use personal contacts for studies of hidden groups where access can be difficult to achieve, but such contacts may know only the most accessible members of these groups. Moreover, some hidden communities designate those who will talk to outside media and scholars and these official contacts may provide very selective access to the community (Blee 2002). The quality of fieldwork studies can be seriously compromised by forms of access that provide biased or very limited data (Denzin & Lincoln 2005).

Methodological suggestions

From my research on organized racism, I have three methodological suggestions for studying hidden communities. None of these are unique to the study of hidden communities, but they take on special salience in this context. First, it is important to let those being studied speak for themselves, something that can be difficult when there is a large disjuncture between the worldview of scholars and those of the members of a secretive group (Blee & Taylor 2002; Blee 2000). A robust understanding of hidden social worlds requires that scholars be cautious in assuming that they fully understand what members say and the meanings they attach to their actions. As an example, when I interviewed modern-day racist women about how they entered racial extremism, many framed their life story as a dramatic tale of personal conversion. They told of a personal event that changed them from weak and racially naive to a strong, committed racial warrior. Conversion narratives are common in mainstream society, found in the stories of recovering alcoholics, gay and lesbian activists, committed Christians, and feminists. Thus, it is

tempting to take the conversion accounts of racist women at face value and conclude that these women entered racial groups because they were transformed by dramatic personal events, like being in a car accident. Yet, that would be an inaccurate reading of their narratives. On the contrary, the stories of racist women actually *conceal* how they got involved in racist groups. In fact, as other data on these women make clear, they generally entered racist groups in a way that was much more incremental and less dramatic than conversion stories suggest. And their entry always involved meeting a racist recruiter who introduced them to racial activism. What their stories of dramatic conversion reveal is not how they became racist activists, but that they learned a new story of their life once they entered organized racism. Being in a racist group taught them to think of themselves in a new way. It created for them a sense of personal identity that is radically different than their past, drawing on the sharp, dichotomous understandings of society preached by racist groups for whom the world is divided between white and non–white, ally and enemy. Their stories of conversion are thus a *result* of being associated with racist groups, not an accurate description of how they joined (Blee 2002).

A second suggestion for studies of hidden communities is to pay attention to everything that members express in interviews and conversations. Again, this suggestion applies to all research (Holstein & Gubrium 1995), but it is particularly pertinent when studying hidden communities in which talk can be disingenuous or evasive. When I interviewed former members of the 1920s Klan, some remembered their time in this hate-filled crusade in the most banal terms, as 'fun,' an innocent time of joviality and sociality. However difficult it is to accept these memories of a time of racial brutality, such sentiments provide important clues to how racism can become

the fuel for a movement as large as the 1920s Klan. Their memories show that even the most virulent forms of racism can become mundane to its possessors and that racist violence can be motivated by the most ordinary and pedestrian of sentiments (Blee 1991).

A final suggestion is to look for the cracks, what doesn't fit, in the talk and actions in hidden communities, a technique that the microhistorian Jacques Revel calls the 'method of clues' (Revel 1995; Blee 2006). Although useful in many studies, the method of clues is particularly useful for hidden communities in which members tend to simply parrot the official ideas of their groups as their own when queried by a researcher (Blee 2002). By probing for cracks in their stories, it is possible to move beyond their rote statements and explore whether the ideas of individuals actually mirror those of their groups. When asked to talk about their own experiences, for example, the racist women I interviewed eventually confided beliefs to me that were profoundly at odds with the ideologies of their groups, even if their initial comments made such differences seem unlikely. They talked of taking their children to Jewish doctors, having abortions, and maintaining friendships with lesbian friends and family members. Other studies of racist groups similarly find that activists' private views can deviate significantly from the public tenets of their groups.[2] Such findings provide important insights into the multiple dimensions of life within organized racism that are not visible from the outside, demonstrating that members can hold ideas quite at variance with the positions of their groups.

For Whom? Ethics of Studying Hidden Communities

Devault's last question concerns for whom we generate knowledge. This is an important issue in the study of hidden communities. Such

2 Aho 1990; Billig 1978; Ezekiel 1995; Fielding 1981.

studies produce particular questions of ethics because those we study may not want to be known. Should scholars set limits on how much access they will seek into hidden communities? Are there conditions under which researchers should decline to study those who seek to be hidden? Are there parts of hidden social life that should be protected from scholarly inquiry? Do some social groups have an absolute right not to be known, a position asserted by some Native American communities vis-à-vis non-native researchers (Smith 2005)? Do benefits that will derive to scholarship or to the hidden community outweigh the costs of exposure? What principles should be used to decide when it is appropriate to probe into hidden social life and when a scholar should back away? Such questions are not often discussed among scholars since there is a presumption that all social life should be accessible to study. But this is not clearly the case for hidden communities in which the desire of people to shield their lives from scrutiny is contrary to the interest of scholars in understanding the broadest range of social life.

Scholars need to consider the issue of limits on access on multiple ethical and personal levels. Ethical issues include the extent to which scholars should protect a hidden society from exposure in all cases. Scholars are divided, for example, on whether they are obliged to protect the privacy of those engaged in reprehensible practices, like drug pushers or racist activists (Calvey 2000; Lee-Treweek & Linkogle 2000). Ethical concerns exist as well about which members of hidden communities are recognized and promoted in the process of being researched (Kleinman 2007). Is a study likely to benefit existing power holders in a hidden community? Is this an ethically-defensible outcome? On a personal level, there are issues of a researcher's physical and emotional safety in hidden communities, especially those engaged in illegal, immoral,

or problematic actions. Scholars also wrestle with how prepared they are to empathize and develop rapport with members of a hidden communities whose experiences, ideas, and values are very different than their own.[3] Ultimately, resolving the conflicting interests of subjects and scholars requires informed and reflexive judgment on the part of researchers as well as sustained discussion in a scholarly community.

Conclusion

Fieldwork creates knowledge that brings the public into the reality of others, as the quote from Marjorie Devault (2007, p.182) at the beginning of this article points out. When fieldwork is used to study hidden communities, scholars have special responsibilities to ensure that the reality that is exposed is accurate and not harmful to those being studied. This paper makes three arguments about scholarly responsibility. First, scholars need to be sensitive to the reasons that a community is hidden and, in fact, can acquire useful information by finding what is hidden and what is open in a community. Second, access can be particularly complex in the study of hidden communities and contacts into hidden communities can create problems of bias for researchers. Third, those who study hidden communities must consider the ethics and personal risks of such studies. That scholars should acknowledge the pitfalls, problems, and dilemmas of fieldwork on hidden communities does not mean that such research should be avoided. Indeed, it is by studying the hidden that scholars can gain perspective on how power shapes social boundaries of marginality and centrality in modern society. But such studies need to be done with respect for the integrity and privacy of those who are its subjects.

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[3] Blee 1998; Blee 1993; Lee-Treweek 2000; Lee-Treweek & Linkogle 2000; Possick 2009; Sehgal 2007.

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Emotional work: ethnographic fieldwork in prisons in Ecuador

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Introduction

Researching hidden communities often takes us to places which are unpredictable, unimaginable and unknowable.[1] Whether researching in a rural village in Africa for a year or across the other side of town for a couple of hours at a time, fieldwork requires that we step out of our usual milieu, put on the guise of an academic (for many of us for the first time) and 'travel' to unknown territory. This 'travelling' is explicitly intellectual, professional and academic but it may also be physical, personal and sometimes emotional.

In this paper I offer an 'anatomy' (Mintz 1989) of the personal and emotional aspects of my doctoral fieldwork in prisons in Ecuador. Examining the messy realities of emotions in my fieldwork speaks back to contemporary debates about the role of emotions in research. As well as gathering legitimate, intellectual knowledge in the form interview data, fieldnotes and budding theoretical ideas, I left the field with a collection of other knowledges which were personal, emotional, unexpected and sometimes uncomfortable. This paper is a product of the ambiguous knowledge that emerged from my fieldwork

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Fieldwork was a mixture of personal, professional, hanging out, observing, interviewing and often just waiting for something to happen. It is tempting to try and make rational sense out of the chaos of fieldwork and to present oneself in the best possible light. Since rewriting tends to further obscure rather than reveal, whenever possible I have used fieldnotes to capture the 'messy reality' of fieldwork.

The role of emotions in research: contemporary literature

> Forty years ago there seem to have been more scientists; now there appear to be more selves. (Mintz 1989, p. 793).

Young and Lee (1996) trace changes in the role of the emotional in ethnographic research. They contend that the researcher's emotions are almost entirely absent in early sociological writings, for example Mead (1934) and Blumer (1969). Although the emotional and personal life of the researcher was noted in studies in the neo–Chicagoan tradition, they were largely relegated to the appendices of ethnographic accounts (see for example Whyte's appendix to *Street Corner Society*, 1955). Underpinning this 'school' is a conception that emotions are apart from scientific experience and knowledge. Although personal and emotional aspects of fieldwork were acknowledged, they were often understood as 'fieldwork troubles', an obstacle to be overcome or a problem to be solved.

More recently however, the role that the researcher's emotions and personal life play in research has been brought to the fore by existential and feminist researchers. This 'emotional turn' contends that emotions are not extraneous to research but are an unavoidable

and integral part of social research and rejects the possibility of emotionally objective ethnography (Young & Lee 1996). In particular, feminist scholars have challenged hierarchical dichotomies of reason/emotion, public/personal and valid and invalid knowledge (Oakley 1981; Widdowfield 2000). Here, emotional engagement is seen as necessary to collecting data which is reflexive, embedded and therefore genuine and authentic (Coffey 1999, p.159). As a result, fieldwork has been widely recognised as a form of emotion-work (Hochschild 1979).[2]

Importantly though, the 'emotional turn' has not replaced traditional suspicion about the supposedly contaminating effects of emotions in research. For example, Kane notes that: 'Introducing an observer's subjectivity challenges the objectivity claims made on the basis of more systematic dimensions of fieldwork. On some level, this weakens our scholarly authority' (1998, p.143; see also Hammersley & Atkinson 1995, p.115). Since an over-arching principle of detachment remains, discussing emotions alongside research reports remains risky. Coffey notes that: 'all too often research methods texts remains relatively silent on the ways in which fieldwork affects us and we affect the field… [i]ssues of identity, selfhood and emotionality are often referred to and thereby understood in tangential and semi-detached terms.' (1999:1).

This paper examines how my doctoral fieldwork affected me emotionally and personally to bring to light questions about doing and being an emotionally engaged ethnographer and doctoral student. I will describe what researching in prisons was like and examine key aspects in my fieldwork such as getting in, experiencing courtesy stigma, how working in a violent setting affected me and

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[2] See also Bondi 2005; Davidson, Bondi & Smith 2005; de Haan & Loader 2002; Hubbard, Backett-Milburn & Kemmer 2001; Kleinman & Copp 1993; Young & Lee 1996.

lastly how respondents' accounts of violence affected me. I will show how being a doctoral student shaped my fieldwork, in particular how working for prolonged periods of time far from home on a project I designed myself produced specific emotional capabilities, pressures and concerns which shaped the research in important ways.

Researching in prisons in Ecuador

My PhD research was on women in the international cocaine trade and in particular on drug mules.[3] I conducted fieldwork with imprisoned drug traffickers in prisons in Quito, the capital city of Ecuador. Ecuador is situated between Colombia and Peru where much of the world's cocaine is grown and plays an important role in the export of cocaine to North America and the rest of the world (Rivera 2005, p.233). As a result, prisons in Ecuador have a high concentration of inmates charged with drug trafficking (Nuñez & Gallardo 2006). Ecuador was therefore a suitable place to conduct research on the international cocaine trade.

Importantly though, my reasons for working in prisons in Ecuador were as personal as they were sociological. Fieldwork emerged from a chance encounter. I first visited prisons in Ecuador while I was studying Spanish in Quito. I heard that there were British nationals in prison who welcomed visitors. I naively arrived at the men's prison armed with cigarettes, chocolate and toilet paper which, after some limited deliberation, I decided were the essentials for surviving imprisonment abroad. Inmates welcomed me with generosity and encouraged me to return. We kept in touch through letters and phone calls and I negotiated their permission to return to conduct research for my undergraduate dissertation and later my

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[3] A person who knowingly physically carries drugs paid for by someone else across international borders. This includes different methods of carrying.

PhD. Since I had enjoyed living in Quito previously, I was excited at the prospect of making Quito my home for 15 months.

Fieldwork was fairly intense: I spent 15 months researching in women's and men's prisons using a mix of ethnographic observation and interviews. I was fortunate to receive an official research pass. This allowed me unsupervised access to most parts of both the men's and women's prisons four days a week.[4] Guards searched me on my way into prison in the morning, I then passed through a series of gates which were unlocked to let me through, then locked behind me. At four o clock, I knocked on the door to be let out. However, once I was inside the prison there were very few guards. Rather than being a physical presence inside the prison, they were most visible patrolling the roofs and walls with machineguns to ensure that prisoners did not escape. There was very little in terms of prison regime, education or training. As a result, prisoners had to support themselves and many ran small shops or businesses. On the inside, prisons functioned like small towns.

Given this unusual context, fieldwork was a bit like street ethnography. Rather than requesting interviews with prisoners through the prison staff, I instead hung out, observed and negotiated interviews where possible. Walking around in prison could be daunting, particularly at the beginning of fieldwork.[5] It is difficult to

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[4] Formal access to the prison for my PhD research was organised through the Ecuadorian Prison Directorate through a local university: The Latin American Faculty of Social Sciences (FLACSO). Anthropologists Jorge Nuñez and Paco García requested permission for an official pass to work in prison on week days as part of the project 'Prison in Ecuador: daily life, power relations and public policies' being conducted by the Urban studies program where I was based as an Associated Researcher. I remain extremely grateful to the department for all their practical assistance, support and encouragement.

[5] Working alone in prison precipitated a number of safety concerns that are too complex to go into here. Suffice to say negotiating the social landscape of prison and being aware of changes in climate were an important aspect of keeping safe in prison. I am grateful to a number of key respondents who 'watched my back',

be unemotional about prisons and imprisonment (de Haan & Loader 2002). Wacquant describes the carceral environment as 'an assault on the senses [...] an other-worldly place [...] it seems like a bad movie, a nightmare, the vision of another world that cannot actually exist' (2002, p.382). I would add that imprisonment is as banal as it is brutal and as boring as it is stressful. In addition, prisons in Ecuador are overcrowded and under-funded. Prisoners lived in physical conditions that breach any and all standards of minimum conditions of imprisonment set out by the United Nations (Knotzer, Ulbert & Wurth 1995).

Working in this environment was intimidating at first; nonetheless I soon became accustomed to the organised chaos which filled the hallways and patios where I spent most of my time. In spite of Wacquant's bleak portrayal and in spite of the inhumane conditions of prisons in Ecuador, spending time on the inside was not always stressful, boring and difficult. Although the focus of this paper is the turbulence and violence in prison, it is worth noting that most of my time was spent 'hanging out': I drank gallons of tea and coffee, smoked more cigarettes than is reasonable in the name of research, cooked, ate, watched TV, sunbathed, played cards, swapped stories, danced, attended birthday parties and even the annual 'Reina de la Cárcel' beauty pageant in the women's prison. At times it was tremendous, exhilarating work.

Getting in: acceptance and rapport

Working in a place that was hidden and physically separate from the rest of society shaped the process of 'getting in' and heightened good feelings about establishing rapport and acceptance.

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accompanied me when walking around the prison and otherwise kept their ear to the ground for news of upcoming conflict.

Prior to conducting fieldwork for my PhD, I had spent a month in prison conducting research for my undergraduate dissertation. I had already established rapport with several key respondents. Furthermore, my longstanding presence in both prisons did much to assure respondents of my dedication and trustworthiness. Nonetheless, establishing a role for myself, building rapport and acceptance was time-consuming and demanded that I do various forms of emotion-work. As an outsider I was a neutral person to talk to and a shoulder to cry on (Bosworth 1999). I also provided a much needed distraction and connection to the 'outside' world (Denton 2001, p.9). Furthermore, due to my commitment to principles of reciprocity I was quickly adopted as a source of news and cigarettes. When I returned to commence fieldwork, my respondents and I were excited about my PhD project. I had secured funding from the Economic and Social Research Council. Respondents who had participated in my undergraduate dissertation were impressed that my government considered their experiences worthy and important. They reminded me of the importance of making their stories public and in particular 'telling the world' about the inhumane conditions of their imprisonment. As a result, my return and regular presence in both men's and women's prisons was warmly met by inmates. Getting in, establishing rapport and becoming accepted by respondents was not the difficult process that others had led me to believe it would be (similarly see Taylor 1993).

Becoming accepted and 'part of the furniture' (Wilson 2006, p.6) was a source of professional pride. This feeling of achievement was heightened by the fact that only a handful of ethnographers had ever successfully done ethnographic research with drug traffickers (Adler 1993; Zaitch 2002). In hindsight, I can see I was somewhat

'romanced' by the approval I felt from respondents and the easy rapport that we quickly re-established.

Establishing rapport had a special draw for me as a novice researcher. Being accepted by those I hoped to study was the first sign that the project I had designed, written and talked about might actually work. The ethnographers I admired (and which were the most lauded) were those who had established entrée and rapport (Bourgois 2003; Crewe 2006; Maher 1997; Nuñez 2007). It was also evidence that I might actually be a good researcher and did much to quell my fears about not being mature or clever enough to complete the task I had set myself. Furthermore, the 'how-to' texts I read reiterated that emotional engagement is useful, professional and good:

> Emotional connectedness to process and practices of fieldwork is normal and appropriate. It should not be denied or stifled. It should be acknowledged, reflected upon and seen as a fundamental feature of well executed research. Having no connection to the research endeavour, setting or people is indicative of a poorly executed project. (Coffey 1999, p.159).

Following this initial phase of acceptance, I reflected critically on relationships with respondents. Aside from being hustled by inmates for cigarettes and occasional favours (which I accepted as an aspect of reciprocity in research) I was satisfied that I had achieved my goal of being an engaged researcher and acknowledged the emotional dynamic of this. It fitted with feminist researchers' concerns about the balance of power: the emotional bond established between respondents and myself allowed them space to negotiate how we would do research together in a way that could potentially avoid exploitation and crucially help me access the kind of good data that was not easily accessible to the outsider. Nonetheless, good feelings

about becoming emotionally engaged with respondents were accompanied by uncomfortable feelings too. Respondents found imprisonment tough: they suffered depression, physical illness (temporary and terminal), were beaten up and spiralled into drug addictions. Witnessing this was difficult: at times I felt utterly helpless and deeply sad.

During my Christmas holiday (after 4 months of fieldwork) I heard that the four year old son of one respondent was killed in a road accident. Angela was a single mother who had made several trips internationally as a mule. By this time, I had known her for about three years. Although I was far from prison, this incident troubled me:

> I walked home angry and frustrated feeling like I should phone her right away and tell her how sorry I am. I was angry that she had received such a long sentence and was so far from home. […] Would we imprison someone who stole a chicken to feed their family? What about if it was kilograms of something that kills other people's children? It's morally ambiguous but I know that she'll be mourning right now: yet another Christmas in prison. I am deeply sad for her.

At this time I was thousands of miles away from the field. My tape recorder, notebook, pen and file of research questions were happily abandoned over the holidays. However I did not and could not simply disengage from the personal and emotional aspects of fieldwork. Unsurprisingly, fieldwork relationships, worries and emotions spilled over into my personal life outside prison.

Courtesy stigma

Goffman notes that those associated with stigmatised groups are subject to 'courtesy stigma' as a result of their contact with a

stigmatised group (1968) (see also Kane 1998, p.146). Several ethnographers have written about experiencing courtesy stigma (sometimes called a contagion of stigma) whilst researching stigmatised groups such as sex phone line workers (Mattley 1998), prisons (Liebling 1999, 2001) and drug traffickers (Adler 1993).

Due to the illegal nature of the drug trade, traffickers are a hidden and stigmatised group understood largely through stereotype:

> One of the linguistic legacies of the 1980's was the transformation of the 'drug trafficker' into an ideological cue, a shorthand reference encompassing the menace, evil, greed, depravity and corruption (moral financial and political) required to ease the passage of repressive anti-drug legislation and policies (Green 1998, p.78).

Outside prison the subject of my research (and the fact that I worked in men's prisons) was often met with surprise. Being a foreigner, a woman and being in my early twenties no doubt added to people's surprise. Recent books and films about drug traffickers and prisons in Latin America (Marks 1997; Young & McFadden 2003) precipitated curiosity about me and my research. Furthermore, the assumption that 'you are what you research' (Kleinman & Copp 1993, p.6) led people to assume that my interest in drugs and imprisonment was driven by a personal drug habit, or perhaps imprisonment of a family member. Some people asked me for drugs, others asked to come and visit prison with me. I found these requests and assumptions awkward. Publicly displaying sympathy or emotional attachment to my respondents was usually met with awkwardness. Eventually I avoided talking about my research with anyone except my flatmates.

I became particularly aware of the problem of stigma when Nicky, a respondent, came to stay with me after she was released from prison. She had been detained for seven months before she was

released without charge. By this time her flight ticket had expired and she had no money. She stayed at my flat for several weeks while she waited to get her passport back and raised money to buy her ticket home.

> Nicky was unwilling to tell people about what she was doing in Ecuador and I participated in the conspiracy. My flatmates knew she was just released from prison, but no one else does. It was easy to tell only the parts of the truth that we wanted to. When we meet friends they typically assume that she is here as a tourist and often ask typical questions like 'how long is she staying for? We would usually reply 'Until she gets her passport'. Since tourists were often pick-pocketed in Quito friends would usually assume that this had happened to her too.

It became clear that Nicky was very probably under surveillance (by police or Interpol[6]) while she was staying with me and that I probably also was as a result. I discussed this with my flatmates who were not particularly worried and probably considered me to be a bit paranoid. Nicky and I talked about it. We considered the possibility that my phone might have been tapped and even once thought we were being followed. Feeling paranoid and stigmatised was worsened by the fact I was a novice researcher and was working far from home. Aside from Nicky, there was no-one at hand that I could safely discuss my concerns with. Although it worried me that Nicky was possibly under surveillance, I enjoyed having her around. She could understand both the worlds which I occupied: prison and my life on the 'outside'. In many ways I found an emotional resonance with her that I could not with my contemporaries who had legitimate occupations and public lives.

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[6] Interpol is an international police agency.

Institutionalised violence and emotions

The prison environment was turbulent. Although nothing seemed to be happening the majority of the time, the peace was punctuated with violent events. Prison was often referred to as a 'time bomb' by inmates. In addition to the stress of being deprived of one's liberty, prisoners were constantly struggling to gain access to limited resources due to overcrowding. Petty disputes frequently broke out over meagre resources such as public telephones, communal areas and even food. Power and water strikes were common. In addition, drug trafficking networks extended into both prisons. Anyone suspected of informing on other mules or inmates was subject to violent retributions.

Violence was a culturally meaningful and institutionalised practice in both the men's and women's prisons in which I worked. One inmate in the women's prison was brutally beaten up by guards, guards had sex with female inmates in exchange for food, a guard was assassinated as she left the men's prison and feuds in the men's prison resulted in fights and murders.

I had little experience to inform me about how to interpret these experiences. As fieldnotes show, I increasingly adopted inmate's (self-protective) attitude regarding violence and became increasingly 'ambivalent' about the violence that surrounded me. After one inmate was shot a few cells away from where I had been, I wrote:

> There's so much violence in the men's prison that I've become kind of ambivalent. Or perhaps just adopting the attitudes of the prisoners. i.e. 'it's not me. I'm alive. I don't know him. He got killed for a reason so it won't be me next. A kind of logical distancing perhaps. I don't know. Also perhaps I also feel like taking on someone else's trauma […] is self-indulgent. It's not going to help anyone. Harsh but there you go. Today it wasn't my drama [...].

Adopting prisoners' way of understanding the pervasive violence in prison allowed me to roll with the punches and continue with research. Although I embarked on fieldwork with a goal of being an emotionally engaged researcher, it was clear that prison was not always a safe place in which to try and be engaged in this way. Nonetheless, as a researcher I constantly absorbed my surroundings and information about them. Although I tried to do this solely at an intellectual level, this inevitably affected me personally and emotionally. Even if I had not set out to be an emotionally engaged researcher, it is difficult to remain unaffected by an assassination or murder in one's place of work. Wacquant contends that researching in prisons demands that: 'you've got to anaesthetise yourself to pretend nothing's the matter and keep going' (2002, p. 378).

Problematically though, this numbness took some time to thaw after leaving prison. After one particularly tough day in prison, I wrote:

> I came home feeling kind of shaky. I had a shower and tried hard to think and process all that happened to make sense of it all. Instead I ate crisps and played computer games until it got dark and cold and the adrenaline started to wear off.

Outside of the field of prison, my emotional response no longer fitted my surroundings. Dealing with these events (as well not knowing what would come next) resulted in emotional paralysis. It became evident that I could not do more than collect data and try and cope with daily changes and challenges. There was little opportunity (and it was not emotionally possible) to process the majority of experiences and knowledge I was absorbing in prison on a daily basis.

This 'numbness' was paradoxical: on one hand Wacquant's 'anaesthetic' approach looks a lot like objectivity. My experience however, shows that it was not a lack of emotion but an overload of emotion. Lastly, my context outside of prison offered me little through which to make sense of the overwhelming level of violence.

Respondents' violence

Towards the end of fieldwork respondents' confessions of their experiences of being violent pushed my ability to engage emotionally with them.

Researchers have commented that violence is central to the functioning of the drugs trade (Hobbs and Pearson 2001). Respondents rarely brought up the topic of their own violence with me. When the topic came up it was mentioned in passing and usually between men rather than in response to my questions. As a researcher (and a woman) I was not expected to take part in these discussions and was rarely party to them. However, one day I was party to a frank confession of one respondent's violence:

> Paul told Ryan about killing a guy […] who owed him for half a kilo (I presume of cocaine). He said that he'd killed him with a baseball bat to the back of the head. I continued to eat my pancake.

Several hours later when I wrote my fieldnotes for the day I was still unable to express any emotion in response to this confession or name what I felt. The shortness of account here is uncharacteristic and reflects how little I had managed to interpret this confession. At the time, I did not know what to think so I continued to eat a pancake Paul had just cooked for me. I quickly became aware that I should not show any 'negative' reactions since this might be important data

which I did not want to chance missing. At worst this could be seen as a form of car-crash voyeurism, at best morally questionable.

Towards the end of fieldwork, I decided that I should try and broach the topic of violence in the drug trade in interviews. Although I had anticipated this being emotionally difficult, it was nonetheless hard to deal with, especially since I was emotionally engaged with respondents by this time:

> Hearing Ryan talk about violence shocked me. I think I was more affected by the interview than he was. I remember his disclosure of violence [a euphemism. Among other things he talked about putting an electric drill through someone's knee cap] and all the time him sitting there laughing [about it].

> I wonder what I looked like. I just took my notes. I came home and cried. I'm not sure why: perhaps from relief that it's nearly all over. Perhaps also because of the sadness of the situation, Frank joked about how he'll be leaving the prison in a box [Frank had a heart condition, the box he refers to is a coffin: he later died in prison]….

> I'm just too tired of the emotional strain, of having to pretend that Frank will be OK when I don't know if I really believe he will be. The tension between trying to balance up how I understand that guys that I like and have respect and are good to me are murderers and have a capacity for cruelty and violence that I have no understanding of.

These events forced me to face up to these contradictions between respondents as people that I liked and wanted to understand and the fact that they had committed violent crimes. This point in fieldwork marked the absolute limit of my engagement with respondents. Whilst I achieved 'cognitive empathy' (Kleinman & Copp 1999, p.38) and I could understand the logic of their actions, I could no

longer emotionally engage with respondents in the same way after this.

Reflections on dealing with fieldwork emotions

The emotional challenges that emerge from the particular alchemy of people, place and subject that compose each research project are individual and specific. Furthermore, fieldwork can be unpredictable. As a result the emotions that the researcher may experience can rarely be simply be planned for and coolly managed. The question of how to deal with fieldwork emotions productively is not within the scope of my experience or expertise. However in hindsight there are some things which I could have done better in the field. Looking at these briefly may be useful for post-graduate researchers about to enter or working in the field.

The first thing that may have helped was to recognise that the emotions I experienced in the field were a product of, and were governed by the 'feeling rules' of the field (Hochschild 1979). Hochschild notes that 'feeling rules' are conventions of feeling which, like social rules, are a product of, and belong to their context. Furthermore, like social rules, 'feeling rules' influence how we try to feel and designate which emotions are deigned appropriate or inappropriate in given contexts. Thus, using the intellectual tool of 'feeling rules' may be a useful way to 'map out' the anatomy of one's emotions in the field.

After leaving the field I found it helpful to reflect on how the 'feeling rules' in prison shaped what I felt and how I could express it. I found this useful since it enabled me to unpack *what* I felt and importantly to understand how my emotions shaped the data I collected as well as how I felt about the data I had collected. This

was a useful tool in gaining sufficient distance to reflect on what prison and fieldwork were like.

Secondly, making space for the 'self' away from the 'feeling rules' of prison may have been helpful. Although I tried to do this in writing fieldnotes, this was perhaps unsuccessful since fieldnotes lie in the domain of academia's 'feeling rules' (see Young and Lee 1996 for a concise analysis of academia's 'feeling rules'). One possibility might be to create and maintain a separate file for personal and emotional aspects of fieldwork. However this may be difficult since the personal and professional are intimately intertwined. Better still would be the opportunity to 'debrief' during or after fieldwork with someone unconnected to one's department or academic life. I met with a counsellor to debrief after I returned from fieldwork and found it helpful.

Conclusion: criticisms of emotional ethnography

In this paper I have attempted to offer an 'anatomy' of emotions in fieldwork. Reflecting on my experiences as an emotionally engaged researcher throughout fieldwork has unpacked some of the practicalities and problems of emotional engagement in the field.

At the start of fieldwork I found it easy to engage with respondents. As subjects of penal power and pawns in international anti-drug politics and policies they were the 'underdogs' (Becker 1967). As I became more involved in respondents and their lives it became increasingly difficult to disengage myself from the research. Engaging with respondents and prison was demanding – not only professionally but also personally.

Being emotionally engaged with people and place for me entailed embedding myself in the 'feeling rules' of prison as a means to emotional survival in the field. I had little experience previously to

inform me about how to make sense of the 'time bomb' of violence, deprivation and stress that composed the prison environment.[7] This was problematic though since it did not help me to process the emotional impact of working in a turbulent and sometimes violent place once I left prison each afternoon. This numbness was compounded by stigma, paranoia and isolation resulting from courtesy stigma and long periods of time in the field. Lastly, as a doctoral researcher, I had a number of fieldwork worries to contend with. I constantly worried that I did not have enough data for my thesis, or that the data I did have was not good enough. I had designed my research project and had fought hard for funding and ethical permission to do it. It was easy to think that I had backed myself into a corner through sheer stubbornness and stupidity. When I finished fieldwork I was burned out. In retrospect it is easy to see that I could have and probably should have done things differently. Nonetheless, that I chose to be an emotionally engaged researcher was a product of my personality, experiences and the aspects of social life that I privilege.

Now ostensibly out of the field for some time, I have had time to reflect on the emotion-work I was doing and the 'feeling rules' of prison and academia. I remain critical about the role of engagement in fieldwork and am unsure of what the value of such emotional engagement might be:

> I am left though, with some doubt as to whether I am entitled to have felt the emotions and anxieties I have described. Have I just been indulging in a vicarious type of suffering to which I have no legitimate claim? (Stanko 1997, p.84).

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[7] Of course inmates had to deal with this situation (and the prospect of much of the same for many years to come). Next to their experiences, mine seem like trivial self indulgence. After all, unlike them I had chosen to be there.

In contrast to academic dictates which contend that engagement is good, genuine and produces valid data, becoming empathetic with serious professional criminals might not always be possible, personally desirable or even ethically justifiable. Furthermore, working in a violent place tested the limits of my ability to engage, as well as the academic appropriateness of such engagement.

The 'emotional' turn rejects objectivity and distance in favour of emotional engagement. However, my experiences researching in prison demonstrate the 'edge' of emotional engagement. Whilst distancing oneself from the subject of study has fallen out of favour in feminist and ethnographic research on crime (Ferrell & Hamm 1998), my experiences suggest that the principle (rather than the reality) of distance should not be lost. This may be particularly the case when research engages with hidden or stigmatised groups. This is not to say that we should avoid engaging with or conducting research on stigmatised groups, but rather that establishing distance from those we study may be an important tool.

Doing the emotional labour entailed in fieldwork is unavoidable and essential for the researcher in the field. As Whyte eloquently put it:

> I […] had to learn that the fieldworker cannot afford to think only of learning to live with others in the field. He has to continue living with himself (1955, p.317).

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Engaging Anthropology in South West Uganda

Christopher Kidd (Independent Researcher)

Introduction

There is often discussion amongst experienced researchers and those starting out in research about whether their position in ethnographic investigation should be modelled on the detached scientist or the engaged human being (see for example Bennett 1996; Bryman 2004).[1]   This paper suggests that the perceived dichotomies of detached/engaged, objective/subjective and reason/emotion are not mutually exclusive terms and should not be imposed on the researcher or the social sciences. Using examples drawn from my 16 month PhD fieldwork I will argue that the question of objectivity is largely irrelevant as researchers are inherently bound within social relations that demand their involvement as engaged human beings whether they choose to be or not. I suggest it would be much more useful for researchers in the social sciences to acknowledge our social engagement and find productive ways to understand the effects our relationships have on our research and the wider world we are part of.

Since 2001, I have been involved in supporting the heavily discriminated and marginalised Batwa of South West Uganda in roles varying from undergraduate and postgraduate researcher to consultant anthropologist and human rights worker. Despite being

one of the indigenous people of Uganda, the Batwa are rarely mentioned in any histories of the country and current state policies either fail to include them or actively exclude them. The Batwa have been represented by others in local, national and global contexts in ways divergent from their own perceptions of their identities and situations. These external representations place the Batwa as almost mythical beings, either as 'Pygmies' who represent a past the rest of the world has left behind or as romanticised hunter–gatherers who possess a hidden wisdom on how humanity should interact with its environment. As such the Batwa are not only hidden through their exclusion from the political landscape but hidden from wider settings through multiple layers of imposed and often misplaced identities.

Through trying to understand the agendas and discourses of 'Western' conservation and development initiatives on the lives of these communities, I have become intimately engaged in those same communities' struggle for survival. I have found it difficult to objectively and rationally observe my work at a distance and deny my engagement in the lives of the people I work with. I have found it equally difficult to maintain the analytical distance I have needed to produce robust analysis. In response to the context I work in I have become an active agent in the Batwa's struggle for self–determination. This article attempts to discuss my own effort to negotiate these issues and the effects they have had on myself and those I work amongst during my periods of research and work.

Locating Anthropology

> At the very time when my studies were most successful, there cut across this plan which I had as a scientist, a red ray which could not be ignored. I remember when it first, as it were, startled me to my feet [...] Sam Hose had

been lynched, and they said that his knuckles were on exhibition at a grocery store […]I began to turn aside from my work […] one could not be a calm, cool, and detached scientist while Negroes were lynched, murdered, and starved […] (Du Bois 1984, p.67).

In 2002 I undertook my first fieldwork period amongst the Batwa as an undergraduate investigating their situation since their evictions from their ancestral lands. During my four months of research I can remember being torn between my interest in the Batwa's relationship with the forest and their contemporary situation outside the forest. The following narrative brings together three important strands of my research experience: the methodology, the context, and the position I took within this context, as a researcher, a friend and a colleague to the people I was living amongst.

In the Batwa organisation's office, I interviewed two brothers who lived in the Kisoro area. They had formerly hunted inside Mgahinga Gorilla National Park (MGNP) but had been evicted from this their ancestral home when it became a national park in 1991. They talked at length about the history of their people and after half an hour I asked what I thought at the time was an innocuous question: 'Do you want to go back into the forest and if so, for what reason?' I expected a response which would illuminate their relationship to their forest. Instead I was offered a response which illuminated their relationships outside their forest. One of the brothers explained that he was scared of going back to the forest because only months earlier his son had tried, been caught and was beaten to death by local non-Batwa villagers. It was said that his neighbours were angry at his continued extraction of firewood from inside the national park.

Looking back today at the interview transcript, it is clear I simply continued with my questions as if this death had never been mentioned. I remember being shocked and, not knowing how to engage with this complex topic, I continued with my original line of questioning. I remember feeling helpless. There were no tools in my 'anthropological toolbox' that could help this man in his loss and I felt unable to begin to grasp such a complex situation. I was wholly out of my depth. Later, in an attempt to take some action, I went to the police with representatives of the Batwa's organisation to enquire about the investigation into the death. The police officers insinuated that, since an investigation is not carried out for the death of a dog, why should resources be used to investigate the murder of this Mutwa?[2] Despite this attitude, and due to the persistence of the Batwa organisation, suspects were later identified by the relatives of the deceased and taken into custody. Unfortunately, this led to death threats being directed at these relatives and all charges were later dropped. The suspects were released back into the community in which the Batwa lived.

In 2005, a few months into my PhD fieldwork period, the father of this murdered man, the man I remember interviewing so vividly, was himself murdered. Back in 2002 this man had narrated his son's death and the denial of basic human rights to the Batwa. In 2005 his wife came to the office and told us of his murder, asking us to help her. I remember again feeling incensed by this brutality, and again feeling powerless – my helplessness venting itself in anger and rage. I also felt diminished by my failure to answer the wife as she begged to know why we had not done anything to prevent this death. The police again seemed unconcerned with this death, blaming it on internal fighting among the Batwa. In contrast, I

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[2] Singular of Batwa

believed that – like his son before him – this father had been murdered by his neighbours within a wider context of violent discrimination.   To date, neither death has been successfully investigated nor have any suspects been brought to trial. The death of this man – who was one of the first Batwa I had interviewed – marked a transition in my research after which I was no longer able to stand aside and understand these deaths as happening to my informants. These events happened to my friends.

My response to the situation is telling, as I found it unethical to carry out my research without supporting the Batwa.   My methodology became firmly rooted in advocating one position in a complex scenario.   Through supporting the Batwa, their struggle became my struggle and in being seen by other non-Batwa as a supporter of the Batwa, the animosity the Batwa were subjected to by their neighbours also became directed towards me. Despite this, my analysis has had to remain objective in order to provide robust material which could be used by the Batwa in their struggle.

Applied Anthropology

My initial doctoral research plan aimed to situate myself within a Batwa community and embed myself in their everyday activities. The purpose of this research was to try to understand how knowledge was transferred within a community and analyse patterns of conflict resolution among this community. In order for this plan to succeed, I needed to create a role for myself amongst the Batwa and their neighbours and purchase or rent agricultural land in order to participate in subsistence agricultural activities.

However, when I reached Kisoro my initial plans immediately became unfeasible. In addition to my earlier undergraduate research in 2002, I had worked in Kisoro for an indigenous rights group in

2003, fostering links between the Batwa's own organisation and larger national and international organisations. When I reached Kisoro in 2005 many people – Batwa and non-Batwa alike – still associated me with this former role. The Batwa assumed I was there to continue supporting their organisation, and the non-Batwa immediately singled me out as a friend and supporter of the Batwa. This had dramatic consequences and I was left questioning whether it was ethical for me to deny the Batwa their request and insist that my research be constituted as a value neutral academic process, or to become engaged in their rights struggle.

I would argue however, as do many feminists, that the concept of a value neutral knowledge and/or methodology is questionable[3]. Myers and Tronto state that 'Value neutrality is often equated with the absence of political partisanship or a "passionate detachment" that then lets the facts/arguments speak for themselves' (1998, p.808). Oakley discarded the value neutral position in her own work,

> [One] reason for departing from [value neutral] interviewing ethics was that I regarded sociological research as an essential way of giving the subjective situation of women greater visibility […] What *was* important was not taken-for-granted sociological assumptions about the role of the interviewer but a new awareness of the interviewer as an instrument for promoting a sociology for women (emphasis in original, 1981, p.48).

Additionally, Oakley acknowledges the effects her research had on the lives of those she researched. In questionnaires, three quarters of her respondents felt her research had affected their own experiences of becoming a mother. Her evidence rejects the belief that the researcher and research can become integrated in the lives of

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[3] See Burton 2001; Gray 1968; Oakley 1981; Myers & Tronto 1998.

respondents but yet still remain 'objective' and she concludes that 'all research is political, from the micropolitics of interpersonal relationships, through the politics of research units, institutions and universities' (Oakley 1981, p.54).

The way in which I carried out my research and the role I took during my fieldwork in 2005 was not one in which I consciously positioned myself. On the one hand, that role was already formed through people's association of me with my previous work in 2003. But on the other hand, my role was decided by the relationships I had formed with the Batwa previously and by the relationships I would go on to form over the duration of my fieldwork period. I was a friend to many of my informants; I became a member of their family and a colleague in their organisation. As Hastrup and Elsass write,

> [...] in particular cases advocacy is no option but an implicit requirement of the social relationship established between the anthropologist and the local people (1990, p.301).

To form these bonds and gain their trust but then fail to respond to their needs as people would, in my mind, have been wholly unethical and would have removed my capacity to form these bonds in the first place. My relationships would not have been reciprocal and meaningful, so my 'decision' to support them was one born out of a necessity to engage with people in a way that was meaningful to both them and to me.

As a result of this, and the Batwa's desire for me to continue to support their own organisation, I quickly abandoned my original research aims and focused on two more pressing issues. Firstly, I wanted to investigate the contexts which had located the Batwa in

the discriminated and marginalised position they inhabited in Ugandan society. Secondly, I wanted to investigate why the development interventions directed at the Batwa were failing to help them escape their situation and in many cases were serving only to entrench the processes of discrimination they were caught in. In hindsight, these were the very questions I failed to ask three years earlier, when I had first interviewed the father whose son had been murdered and who would later be murdered himself.

Laura Thompson wrote in the 1970s that,

> [...] an applied anthropologist may help a client group as a consultant by defining the group's practical options in local, regional, national, and global contexts" but that the "choice of a preferred alternative and its enactment, however, should remain the prerogative and responsibility of the client" (Thompson 1976, cited in Bennett 1996, p.S38).

Notwithstanding the appropriateness of Thompson's advice, my research and work did more than just present options to the Batwa. In order to present options I was often involved in using my own skills to open up political, economic and social spaces into which the Batwa themselves could step. According to Rappaport, the defining features of an empowerment research model is,

> [...] identifying, facilitating or creating contexts in which heretofore silent and isolated people, those who are 'outsiders' in various settings, organizations and communities, gain understanding, voice, and influence over decisions that affect their lives (Rappaport 1987, cited in Small 1995, p.945).

In this sense I advocated and negotiated for the Batwa to be included in project designs and implementations, whether by pressuring

existing Development projects to include Batwa members within their management, or facilitating the Batwa to enact their own development and advocacy projects independently.

Acknowledging my position as an applied anthropologist meant accepting more than just the practical and ethical considerations of working with a disadvantaged group like the Batwa. It also meant accepting the influence my position had on the communities I worked with as well as the influence they had on me. As Goodman notes,

> It is not simply that, if they so desire, anthropologists can find ways around the inevitable effects that they have on the societies they study and those which the societies reciprocally have on them, but that such interactions are the very stuff of the anthropological project, providing as they do important information on the way that societies (both that of the anthropologist and the society they study) operate, think about themselves and change. We should embrace, incorporate and 'translate' the effect of these interactions rather than try to avoid them (2000, p.152).

Taking up a voluntary role for the Batwa's organisation, whilst conducting my research, completely changed my intended research focus. This focus moved from the internal dynamics of a Batwa community towards the relationships that such a community might have with external actors and groups. I quickly realised my repositioning now placed me central to a new topic which was crucial to the Batwa and their struggle. My position was part of what Bodley, in a response to an article by Bennett, suggested was a new movement in applied anthropology in which,

> […] its practitioners are working for hundreds of small non-profit organizations around the world that are

dedicated to social justice and sustainable development. They use anthropology's holistic method and deal with cultural systems. I propose that 'action anthropology' shifts its research focus upward to what Bennett calls "the other in the background who call the shots or hold the ultimate power," "the basic power structure," or the "Big Boys who run the show" (Bodley 1996, cited in Bennett 1996, p.S42).

Indeed, through my research I was able to offer valuable information back to my informants by taking on a role within their organisation which they would have otherwise been unable to pay for. Additionally, my new role allowed me to investigate the interaction between the Batwa organisation and the 'Big Boys' of the Development world. Due to my education and identity as a white European, I was able to enter into a world in which the Batwa were unwelcome and for whom the door was often closed. It was this access which enabled me to open up spaces whilst at the same time documenting the dynamics between the Development world and Batwa communities. I gained immediate access, through friendship and trust, with Batwa communities who saw me as someone working alongside them and therefore with them. And through this role within the Batwa organisation, I also gained access to those organisations working with the Batwa. As a result I inhabited a place never fully positioned on either side of the Developmental binary that differentiates the 'developed' from the 'undeveloped'. I was educated and white, yet I chose to live in a village and support a marginalised people, and this caused most Developmentalists to never fully trust me. On the other hand, although I chose to locate myself amongst the Batwa, as a foreign university graduate I was never able to fully inhabit the social complexity of the situations the Batwa found themselves in.

A Step Too Far?

In taking on such an engaged position during my fieldwork I was forced to question my intellectual location as a researcher within my research process. One of the alleged risks which have permeated ethnographic fieldwork since its inception is the risk of 'going native'. According to Bryman this process happens,

> [...] when [researchers] lose their sense of being a researcher and become wrapped up in the world view of the people they are studying (2004, p.302).

If we accept this definition for the basis of my argument, then I question whether the act of 'going native' is inherently dangerous to the anthropological endeavour. As Tedlock points out,

> What seems to lie behind the belief that 'going native' poses a serious danger to the fieldworker is the logical construction of the relationship between objectivity and subjectivity, between scientist and native, between Self and Other, as an unbridgeable opposition. The implication is that a subject's way of knowing is incompatible with the scientist's way of knowing and that the domain of objectivity is the sole property of the outsider (1991, p.71).

To think like the 'native' does not inherently suggest the throwing away of rationality. Instead,

> [t]o rule out the possibility of belief in another's reality is to encapsulate that reality and, thus, to impose implicitly the hegemony of one's own view of the world (Ewing 1994, p.572).

'Going native' suggests that the researcher has attempted, as best s/he can, to begin to think about the structures of a society and culture in terms other than the ones s/he has been given by their own society. To fail to 'go native' suggests the researcher has maintained an essentialised notion of both their informants and their own society as separate and unyielding categories which cannot be 'stepped out of'. If culture is created and defined through interactions between those who enact that culture, then it can only be understood by participating with it and not by observing it from afar. 'Going native' was a process where I attempted as best I could to fully embody the relationships I found myself in, to try and understand the world in ways which were different to my own. This did not mean that it became my task to become a Mutwa and nowhere do I try to represent the experiences of the Batwa as my own. In my own conception, 'going native' was an attitude I had which denied an objective separation between myself as a researcher and the Batwa as my subjects and which acknowledged, for the duration of my research, that we both inhabited the same world of relationships.

With this period of doctoral research behind me, I look back on my time in Uganda and can see no other way to have interacted with the Batwa other than 'going native' in the sense outlined above. My fieldwork experience and the process of engaging with the context of my work not only allowed better access to another's worldview or more embedded data, but it was also a response to human interaction. Indeed,

> [...] it is impossible for ethnographers not to become a part of the society in which they spend a significant part of their lives. Ethnographers are drawn, often involuntarily, into the nets of significance cast by the people among whom they conduct research and are

thrust into their discourse and debates (Ewing 1994, p.578).

In order to respond to the Batwa in a meaningful way and be part of their lives, I could only become engaged in their world, invest emotions in that world, and try to understand the position they saw themselves in within that world. My fieldwork was not simply a route to an academic union card, but rather the centre of my intellectual and emotional life at that point in time (Tedlock 1991, p.82). To have done otherwise would have been to separate myself from the people I lived amongst, objectify my endeavour and fail to enter into meaningful relationships.

Understanding Emotions

Rosaldo notes that the general rule of anthropological studies,

> [...] seems to be that one should tidy things up as much as possible by wiping away the tears and ignoring the tantrums (1984, p.189).

To remove such emotions would however, 'distort their descriptions and remove potentially key variables from their explanations' (Rosaldo 1984, p.188). This next section will recount some of the emotions and conflicts present in my research, not as a way to validate or invalidate my research but to show that as a key component of my lived experience, my emotions and responses are variables in my data and need to be acknowledged.

In becoming entangled in the struggles of the Batwa, events I saw or experienced became relevant to me. This made me more able to understand the complex problems the Batwa faced but it also opened me up to the emotional anguish of being witness to their

situation. I became so emotionally involved with the Batwa that I often responded to situations outside of the implied objectivity a researcher is assumed to have. In most situations I knew no other way to respond. When a policeman told me that he did not have the time to investigate the murder of a friend, I knew no other reaction than to become enraged. These kinds of incidents moved beyond the abstract field of social investigation and tumbled into personal relationships and the lives of the people I knew. Despite my commitment to these relationships and the responses these relationships exposed, I often felt guilty as I perceived I was failing as a researcher. It was one thing to feel sad at the loss of a friend whilst in the field, but it was another thing entirely when I became angered by the response of the policeman. I felt I had abandoned my ability to see the structural forces which shape the actions of individuals. I felt I had lost, what I thought at the time was, the very essence of my anthropological identity.

For the duration of my PhD fieldwork I also felt anger, resentment or dislike for many of the local and international NGO staff who were supposed to be my informants[4]. In taking on the role of support staff with the Batwa, I gained immediate trust and friendship with the many Batwa spread throughout the region. However, this research focus – the interaction between the Batwa

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[4] These NGOs have backgrounds in development, conservation, evangelism, or human rights approaches. From within a paradigm of progress these groups almost universally view the Batwa as 'undeveloped' and blame their hunting and gathering culture for their inability to develop like others. Most projects therefore focus on helping the Batwa develop sustainable livelihoods outside the forest and increase their living standards through the provision of health care, land, homes, education etc. At the management level, most NGO staff were based in the capital and tended to be of European or North American background. At the field level, NGO staff were almost universally from the neighbouring local ethnic groups. As a member of support staff to the Batwa's own NGO, I facilitated dialogue between State and civil society actors and the Batwa communities, and supported the Batwa to participate in the design and management of projects implemented by NGOs.

and external Development agencies – meant that the majority of my time was spent with non-Batwa informants. These were the very people I was witnessing discriminate against the Batwa. Sometimes this discrimination was explicit in actions and comments that were directed towards the Batwa. Other times this discrimination and marginalisation was more passive and found within the design and implementation of projects where the Batwa were reserved to positions of passive recipients of external aid, devoid of capacity to transform their own lives.

I found it difficult to socialise with some informants despite knowing that this interaction was vital to my research. I often came home from a meeting ready to explode after listening to how the Batwa were to blame for their situation and how everyone would be better off if they just slowly died out. Ultimately, this resentment hampered my ability to form bonds with certain individuals and reduced my field of research as I avoided speaking to some individuals who may have had important information.

This situation is noted by Lee-Treweek who discusses her feelings towards auxiliary staff in the care home where she was carrying out her research. She explains that, 'it would be fair to say that dislike was the predominant feeling I had towards the auxiliary care staff' (2000, p.177). She interpreted these emotions,

> [...] as a sign of personal inadequacy. After all, I had not
> read many accounts where dislike and attempts to create
> distance were key components of a researcher's response
> to their participants (Lee-Treweek 2000, p.122).

It was not until she began the writing-up process that she realised that these emotions, rather than being at best a section of her research she should keep unspoken, were actually crucial to her

understanding of the context of which she was an integral part. They mirrored many of the emotions the care staff felt towards their patients and helped her understand why they responded to their patients in the way they did. My own emotions brought me closer to the Batwa by understanding their situation much more acutely, and as I learned to use these emotions as a tool to analyse my interactions, I was better able to analyse the actions of those who discriminated against the Batwa.

Unlike the illusionary fieldwork experience I thought I was entering into, where the researcher is loved and loves in equal measure the people s/he is researching, there were times in my own fieldwork where I was disliked as much as I disliked the people I was with. In walking into the role of a support worker I immediately stepped into a social setting I was unprepared for. I remember initially, full of excitement at the months ahead, explaining to people that I was there to work with the Batwa and support their struggle. I equally remember what felt like a physical blow when those same people turned to me to laugh and ridicule my endeavour. I had entered into this period confident about my work and proud of my role in supporting a marginalised people. I had not given any thought to how non-Batwa would perceive me, and if I did, I naively assumed it would not matter to me as long as my work was supported by the Batwa.

I was of course wrong. The local population was composed of 99% non-Batwa, most of whom discriminated against the Batwa, and I spent some very dark months feeling ostracised and alienated. I awoke feeling distress about the day ahead and had to force myself out of the house to face the people who disliked both me and the work that I was doing. In the end however, my situation is best

described by Berreman who, in writing of his own research within a heavily segregated village in India, explains,

> Although I remained an alien and was never made to feel that my presence in the village was actively desired by most of its members, I was thereafter tolerated with considerable indulgence. I became established as a resident of Sirkanda, albeit a peculiar one, and no one tried to get me to leave (2007, p.143).

I spent the first twelve months feeling alone and alienated. I was not visiting the Batwa communities because my research required that most of my contact and relationships were with non–Batwa people. I had friends I could have visited in the capital and there were plenty of tourists intermittently travelling through the area that I could have sought out for friendship, but I felt determined not to rely on people external to the situation. I believe this tactic eventually worked and after the first year I felt a noticeable difference in the way I was regarded by the wider community. Munck (1998) describes three stages of 'hanging out' when on fieldwork. The first is the *stranger* stage where the researcher tries to become familiar with the community (or group) and they with you. The *acquaintance* stage comes as the researcher and participants begin to see each other as individuals, which then leads on to the *intimate* stage where the researcher and participants share a mutual history and range of experiences (Munck 1998, pp.41–42).

I was never able to reach the intimate stage with all the people I lived with in Kisoro. Many relationships did not get past the stranger stage as some people failed to see me as anyone more than a supporter of the Batwa, an identity which bore many similarities to the way 'kaffir lovers' were viewed in apartheid South Africa. Yet others knew me by the end of my sixteen months as an

acquaintance, saw beyond this stigma and regarded me as an individual. As more NGOs understood my work they began to ask for my support and I became a valuable tool for the Batwa through my ability to work within external organisations as an 'expert', helping to shape project designs and management to favour the goals of the Batwa. It was the Batwa and a few non-Batwa who allowed me to finally enter into intimate stages of 'hanging out', where our lives became one of respect and acknowledgement. However, as my informants were largely non-Batwa these intimate relationships were always in the minority.

Negotiating Ethics

It is also important to discuss some ethical considerations I have grappled with during my involvement with the Batwa. Despite ethics being defined by the contexts in which they arise, I will consider some general issues which reoccurred throughout my periods of research. A fundamental issue has been: to what extent can the researcher always refrain from active intervention in the lives of the people s/he has to live with? I decided to actively intervene in the lives of the Batwa through my day-to-day work, supporting their organisation and through the advocacy work I continue to this day. From the sections above it is clear I was faced with an extreme situation where my interventions had immediate and often life changing effects on the Batwa.

As a result I also had to negotiate the safety of my participants in my interventions in their lives. In 2002 on a routine community visit with other colleagues from the Batwa organisation, halfway through the visit we were confronted by an angry man who claimed that we had no right to talk to his Batwa. He was the local chairperson of the village and allowed the Batwa to live on his land

as long as they worked the land for him. Looking back I realise that he must have been scared we were in the process of 'liberating' the Batwa he considered to be his own property; in many ways we were. He responded to his fear by lashing out violently – physically as well as verbally – and for a brief moment I asked my colleagues if I should defend not only us but the Batwa he was abusing. We agreed that this would only make things worse for the Batwa and we left. I was only then aware of the danger we had brought to this community by our visit and felt responsible for the abuse they suffered. I felt troubled and wanted to get away from this scene of guilt as quickly as possible. However, after fleeing their 'patron' the Batwa came running after us and demanded that we continue talking on some communal land ahead where their 'patron' had no authority over them. The Batwa wanted to be 'liberated'; they wanted to discuss their human rights situation with us, so we continued the meeting despite the dangers.

My intention in telling this anecdote is to suggest that despite the mistake we made in visiting the community, in this situation the ultimate decision as to whether the meeting should continue rested with the Batwa. Since this experience I have attempted to allow the Batwa to negotiate many of the ethical dilemmas we have faced as our work together has progressed. In many of these issues they are more aware of the risks than I am and I have learned to trust their decisions and accept them.

In a much more recent experience the lines between detached/engaged, objective/subjective, and reason/emotion dissolved into a situation where no amount of reason or emotion could provide satisfactory comprehension. Today I work for the same indigenous rights group I worked for back in 2003. As part of my work I was a member of a team who visited one Batwa

community to discuss their situation. Throughout the dialogue one woman sat at a distance from the rest of the community. I asked about her and the community responded that she recently had a miscarriage and had tried to end her life. She was sitting away from the community, partly because she was still bleeding heavily from her miscarriage, and partly because she had been ostracised by the community. This had not been her first miscarriage and doctors had told her she was losing her children because of complications from the sexual transmitted diseases she carried. Despite her own attempts to seek treatment her husband refused and continued to re-infect her.

We spoke to the husband and urged him to visit the hospital for treatment with his wife but he again refused. He instead blamed his wife for being insane and possessed, despite both states being created by the loss of her unborn children. As we left, unsure of how we could intervene, the husband retreated to his house only to return with a machete to harm his wife. In this instance we could only intervene to prevent the harm to the wife whilst we were present, but it was unimaginable that we could keep her from harm indefinitely. The husband demanded she leave, but she was part of a context that she didn't want to be removed from. Her family and community were close by and she chose to remain. We talked to the community and pleaded with them to intervene. We also went to the local district officials and the hospital asking that they also show an interest in the life of this woman.

On this day I entered into the lives of the Batwa hoping to elicit information from the community that I could use to support their land rights struggle but was instead absorbed into their personal and social milieux. I was thrown into a situation I was unprepared for and unable to act upon. Despite not choosing to become

engaged in the lives of this husband and wife prior to our meeting, I was uncontrollably involved in this situation by my presence on that day. In spite of doing all I could to help this woman, I am today left troubled by my personal inabilities to resolve this situation, regardless of whether such a resolution was ever possible, and uncomfortable by my exit from her life to my own life of relative security. As individuals present in the lives of others, researchers are often uncontrollably entangled in those lives, often in ways we have not foreseen. It is therefore important to acknowledge the effect we have on others and the emotional effects those lives can have on us.

Conclusion

Although the events in this article are extreme in comparison to the majority of my experiences with the Batwa, they do serve to highlight social science researchers' inability to disengage from a world we are intimately part of. I have resolved to understand my role as an engaged anthropologist intimately bound within the contexts I work in. This does not suggest I see myself involved in a subdivision of anthropology, a new approach, or a remodelling of an old technique. It is not to suggest that anthropologists should lay down their methods and adopt engaged anthropology as a new technique. Instead my suggestion is to change nothing in our practice other than to acknowledge and value the social engagement that social science researchers have always been part of. Engaged anthropology is the only approach we can make as individuals in an interconnected world. It is important to acknowledge the spaces we step into in our research and to acknowledge the transformation those spaces have on us and those we share those spaces with. The only decision to be made is whether to accept the contexts we are part of and engage with them, or to deny them and hopelessly

attempt to disengage from a world of social relations we are intimately bound to.

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Russian prisons: Bringing a riddle out of hiding

Laura Piacentini (University of Strathclyde)

Introduction

In this paper I discuss doing research in Russian prisons, which remained hidden from the international research community for nearly 100 years.[1] Over the last 13 years, I have visited and conducted research in more than 20 prison establishments in Russia and interviewed over 300 prison personnel and prisoners.[2] Whether I

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[1] I wish to thank the two Reviewers of the paper for their highly constructive comments. I wish also to extend a big thank you to the Hidden Communities team for their input and advice on the paper.

[2] See Piacentini (2004) and (2007) for a fuller account of the range and types of establishments visited. All of the research I carry out can be loosely described as a socio-political analysis of post-Soviet imprisonment. For ten years, I carried out research in men's prison establishments. I have also carried out empirical work in different regime categories (for example, long-term establishments, open prisons, young offenders' institutions). Since 1997, I have conducted 6 periods of fieldwork most of which I have conducted on my own without interpreters. Over the years, fieldwork methodologies have ranged from intense ethnography in the earlier periods where I lived inside prison regimes, to more traditional/classic quantitative and qualitative techniques, such as in-depth interviewing, focus groups and questionnaires in the later periods. Methodological choices have been driven by research aims as well as wider and often extremely turbulent economic and social climates that influenced access to research sites as well as the duration of the fieldwork. The dates are: 1997 (6 weeks: Moscow, Ryazan and Western Russia), 1998 (1 week: Moscow), 1999 (5 months: Smolensk, Western Siberia, Ryazan and Moscow), 2003 (5 weeks: two Siberian regions), 2006 (10 days: Ryazan) and 2007 (12 days: Mordovia). My current research, with the Universities of Oxford and Birmingham, is a major ESRC project on the relationship between distance and punishment in women's prisons in Russia. This is the first research of its kind in the   world.   See http://www.geog.ox.ac.uk/research/transformations/projects/russia/ for more information. Theoretically and conceptually, this project draws from the disciplines of human geography, the social history of Russia and prison sociology and seeks to build theoretical insights into modes of penal punishment and the experiences of being 'distanced' from home for this cohort of offenders.

travel to Russia on my own to carry out a research project, or travel with research partners (which has been the case since 2006) all of the criminology research I do continues to be conducted during extraordinary political, economic and cultural change, the effects of which are still being felt today. The paper reflects on past and present experiences of researching Russia's vast penal territories between 1997 and the present, and covers three areas, beginning with a discussion of the tension between *hiddenness* and *visibility* in prisons generally. This is followed by a brief history of Russia's penal system. The paper's final section describes some of the methodological concerns I faced in researching this hidden penal community. My aim is to say a little bit more than how to gather data from Russian prisons. I aim to look beyond penal politics and towards history and culture to examine whether the tension between hiddenness and visibility, that characterises Western penal systems, occurs in Russia and the effects of this.

Prisons: hidden and visible

All prisons are, essentially, hidden. The institution through *exclusion* functions to punish, control and supervise those committed to it. Prisons are mostly unseen and almost always unfelt spaces. Few in the public enter but of those held captive, many return. The prison is a bounded community and captives and custodians must negotiate this physical isolation. Generally speaking, prison life provokes extreme views because with the exception of those who work and live in prisons, nobody really knows what happens inside. The persistent stereotype is that prisons are either disturbingly violent or frustratingly lenient. A further institutional stereotype has more than a ring of truth to it – prisoners live behind high walls, there is a mistrust of prisoners, and nobody cares about staff or prisoners

(Crawley & Crawley 2008). The prison is also the persistent material and metaphorical measure of state power and is, therefore, heavily symbolic. First, what happens in prisons cannot be separated from the publics' view that crime harms communities and that victims' justice is often perceived as unmet. Thus, prisoners are different from 'us' and moral responsibility must be taken for true rehabilitation to occur. Second, there is tacit agreement among academic prison sociologists that penal policy, legislation and media-fuelled anxieties over 'what works' in prison have rendered prisons an abiding feature of contemporary Western societies (Jewkes 2007).

Prisons are also *not* hidden insofar as prison workers and officials engage constantly with the outside world; with social workers, bureaucrats, businesses, housing organisations and a range of services allied to education and mental health, to enable the process of re-entry into society and to ensure that captives do not return. As a visible community prisoners and guards share a common emotional, geographical and physical isolation from the outside world. So while there are clear lines of status between workers and prisoners, there are also inextricable linkages and proximities because order, accommodation and compromise must be negotiated. Also common to both prisoners and prison staff is being held captive in social isolation and subject to movement control and regulated interaction. This common experience can break down these clear lines of difference to create shared social solidarity and shared identities that can sustain life on an everyday level until the prisoner is released or until the guard returns home. Yet, while prison personnel work 'for' society, in the profession of prison guarding there is also, at its core, an occupational culture which is highly visible and undervalued by managers and the general public (Crawley & Crawley 2008).

The tension between the hiddenness and visibility of imprisonment is captured well in the description of penal systems as subject to a political 'carnival ride' (Jewkes 2007, p.24). As prison populations soar at an almost breathtaking speed across the world (Piacentini 2004), there are two conflicting messages: while there is the hope of rehabilitation, there must also be severity. If you add to this that the majority of prisoners return to jail within two years of release in the UK, [3] then the prison can be understood as a normalised, visible presence in the life course of imprisoned criminals. It is, therefore, undoubtedly the case that imprisonment is a laboratory of humanity where a change process is expected to occur (change is hidden and out of sight) but where the demands of the public are paramount (visible accountability).

If prisons are both hidden and visible communities, then they could be described also as having a porous periphery and this makes them peculiar locations for social research. In these well-bounded spaces, enormous bureaucracies and conflicts can render officials suspicious of the research and the researcher and prisoners weary and afraid. Yet penetrating the unique physical barrier of the prison educes burdensome non-physical barriers in terms of gathering information (by building social rapport while suspending moral judgements); coping with an intricate ethnography (by establishing position and purpose in an unfamiliar hidden place); and navigating the complex relationships between captives and custodians (by recognising the myriad power relations operating within this unique scenery). I discuss these issues in relation to Russia further on but it is worth noting Denzin here where he argues that as social researchers, we are integral to the social world we study: '…the Other's presence

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[3] See Scottish Government (2008).

is directly connected to the writer's self-presence in the text' (1994, p.503). Knowledge and understanding of social phenomena, therefore, are contextually grounded. Clearly for the prison researcher, the method and the data analysis are not separate entities but are instead reflexively interdependent. Yet because of the tensions outlined above – between severity and rehabilitation in punishment, and between hiddenness and visibility – prison research demands methods that cannot be neutral or mechanical and instead carry with them the epistemological, ontological and theoretical assumptions of the researcher who develops them (Mauthner & Doucet 2003). In terms of the theme of this Special Issue, there is the hiddenness of the social group that each paper explores. For prison scholars, when penetrating the porous periphery of prisons, it comes down to the issue of taking sides (Becker 1967; Liebling 2001).

Criminologists tend to agree that prisons produce misery and that researching prison institutions demands a special research stance. There is a strong history of prison sociologists being self-consciously sympathetic towards 'deviants' or subordinates. Liebling (2001), discussing an article by Howard Becker published in the journal *Social Problems* in 1967, argues that it is technically and morally impossible to be neutral when doing social research because 'personal and political sympathies contaminate' (Liebling 2001, p.472). These sympathies might include a concern for the human and legal rights of prisoners, miscarriages of justice or even deaths in custody. Drawing sympathy from one side of the research is a particular moral conundrum for prison researchers deriving from a long-held 'deep sympathy' (Becker 1967, p.240 cited in Liebling 2001, p.472) towards deviancy; a sympathy that is embedded in the intellectual ascendancy of prison scholarship from the 1960s onwards. If imprisonment is painful then one cannot view the prisoner as an

object of neutral analysis in terms of whether prisoners suffer 'pain' in the way that they experience imprisonment.

In summary, somewhat surprisingly, prison scholarship continues to direct the sociological gaze on how the tension between the hiddenness and visibility in the prison arises from the coming together of politics, crime and punishment. Less well documented is how *alternative methodologies* can be employed to explore for evidence of the tension between hiddenness and visibility in penal jurisdictions in Western prisons; while there remains very little prison scholarship beyond Western jurisdictions more generally.

In the following section I outline what makes Russia's hidden penal community exceptional and distinctive and say something about the politics of imprisonment in that country, which has been likened to the break-up of an ideological fiction and an immense piece of punishment machinery.

Russian prisons

Nowhere is the penal landscape subject to such tension between hiddenness and visibility than in Russia. Russia has the second highest prison population in the world with over 889,948 persons held in prisons and pre-trial prisons (The World Prison Brief 2009). This is an increase of 18,255 since 2007.

I am not the first Westerner to have entered a Russian prison. I am, however, the first Western criminologist to have conducted theoretical and empirical research on Russia's contemporary penal system having stepped into a Russian prison for the first time in 1998. Operationally and culturally, Russia has a unique penal system, which for almost an entire century remained hidden from the international community. Aside from work published by political dissidents and reports from Non-Governmental Organisations, few

knew what went on inside Russia's hidden prison system (Piacentini 2004). Indeed there continues to be an acute paucity of criminological research in this area.

What is now known is that the penal system operated way beyond crime control in the typical sense insofar as it became an industrial monolith that was economically and politically central to the advancement of the Soviet regime. Yet prisons were built (primarily) in the frontiers and at the peripheries, so geographically there was considerable institutional distance from the central power of the regime. Prisoners worked on ambitious industrial projects that included agriculture, forestry, road-building and mining. The social function of imprisonment was political correction: prisoners were 'rehabilitated' as builders of the communist utopia. Unlike other prison systems around the world, a Soviet prisoner's crime was judged according to the extent that it wrecked Soviet harmony. In this sense, all crime was that which was politically and culturally harmful to Marxist/Leninist ideology. This is a departure from conventional criminal justice discourse, which fuses social engineering arguments with social-psychological approaches aiming to redeem and reform offenders. What is also distinctive about Russia's hidden penal community is that jailed criminals took their place alongside teachers, doctors, mothers and fathers to commit to labour that would create the long cherished dream: a 'kingdom of heaven on earth'. In a sense, the prison population was highly visible due to the fact that the boundary between 'prisoner' and 'worker' was blurred and presented as thus in propaganda and media discourse. In 1934, the Russian Gulag, an over-populated and hidden slave labour system, was created:

> We were from the powerful tribe of zeks (camp prisoner), unique on the face of the earth. And the Kolyma was the greatest island, the pole of ferocity of that amazing country of Gulag, which though scattered in an Archipelago geographically, was in the psychological sense, fused into a continent – an almost hidden, almost imperceptible country inhabited by the zek people (Solzhenitsyn 1973, pp.xvi–xvii).

Prisoners were transported all over Russia and the penal system grew in excess of 12 million prisoners in the late 1940s (Figes 2008). For the entire Soviet period, prisoners were displaced, compressed and distanciated from their previous selves. Once lifted from home, they were compelled to live under a new set of social relations. Essentially, the Soviet system exiled its mass prison population to far-off lands.

The environment that I stepped into, when I set out to research the system, was in a scandalous state, out of reach and out of sight for decades. Human rights abuses saturated the system. Massive overcrowding, disease and torture were commonplace. TB was rife and prisoners died of overcrowding and malnutrition. Victims of AIDS had joined the prison population. The scale of human rights violations was horrifying with conditions described by the Special Rapporteur to the United Nations as 'repulsive and torturous' (Piacentini 2004). The current penal system is a direct legacy of the repression of 25 million Russians between 1928 and 1953 (on average 1 in every 1.5 families in USSR in 1941 was 'repressed'; that is, shot or deported or sent to the Gulag) (Figes 2008). Lives have been damaged in disturbing ways with profound social consequences still felt today. The 1990s marked a sustained campaign to rupture the system through a reduction of the population and reform of the legislative and organisational structure of law and criminal justice.

Yet it remains a visible penal system in that it is frequently the subject of highly critical human rights reports and sustained political pressure to reduce the population and improve human rights (Piacentini 2007).[4]

Invariably, when confronted with the reality of Russian prisons (a population with acute addiction problems, often severe overcrowding and human rights abuses), I was faced with the balancing act of giving an intellectual account of 'what is happening' while suspending 'what ought to be', particularly when the overwhelming knowledge base on Russian jails came from human rights reports and not academic research. Indeed a good example of the complexities of suspending, or not suspending, moral beliefs about what ought to be done to improve prisons is the powerful international penal reform movement, which time and again reminds us that terrible inhumanities are often committed in places of confinement. Given what has previously been documented in Russian jails, in a penal landscape scarred by oppressive human rights abuses, the position of neutrality is vulnerable to pressure to appeal to the position of the prisoner. Time and again, the demanding transition from free citizen to prisoner is described in graphic and at times harrowing detail in ex-prisoner literary accounts, campaigning web-sites and human rights activism.[5]

As a prison sociologist, with an interest in Russian social history, a primary consideration of mine in understanding the conditions in Russian jails is adopting a more enlarged perspective that embeds the research findings within a cultural and historical analysis of crime and punishment in Russia. The enlarged standpoint

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[4] In 2005 there was a mass self-harm protest of prisoners recorded across the international news media.
[5] See www.robertamsterdam.com for detailed and regular news and academic articles on Russian politics, criminal justice and law.

position is outside of, and different from, the social world of the guard or the prisoners. It is a question of style and approach as to how that standpoint is reached, and the techniques used to avoid settling on either 'side' can be achieved by interviewing both the superordinates and the subordinates. That is, having all those who shape the standpoint present while being intellectually attentive to history and culture, the complexity of hierarchy, and the nature of agency and power. When it is again considered that in a country such as Russia, where there was a blurred boundary between prisoner and guard, such an enlarged perspective has growing importance.

In the following section I aim to make a contribution to prison scholarship by exploring how the strands of penal punishment in Russia connect to culture, thus raising new and interesting questions on long-established debates about hiddenness and visibility.

Entering a hidden penal community

In this section, I discuss some of the challenges to conducting research in Russian jails and how I responded to these challenges in the field.

My journey through Russia's penal terrain has been epic and turbulent, combining acute fear of the unknown with carrying the weight of a hitherto unknown area of prison scholarship. It was once said to me: 'Nobody knows and nobody wants to know what happens here' (female prison officer, L'Govo Penal Colony for girls, 2006).

There was clearly a story to tell and a variety of experiences to be had in reaching this hidden prison community that, in the 1990s, began the process of engagement with the outside world and becoming visible. It began in the first year of my Doctorate when I

was sent to Siberia by a senior prison official to 'get me out of the way', compelled to live with prisoners and watch as they navigated severe over-crowding and survival. I lived inside a decrepit and dilapidated system where conditions were scandalous and nutritious food scarce. I have been buried inside jails, unaware of the turmoil outside. I heard nothing about the 300 people murdered in apartment bombings in Moscow in 1999, or about the Russian economy collapsing in 1998. Even the then little known oligarch Roman Abramovich buying Chelsea football team escaped my attention. I have been chased by a soldier with a rifle as I strolled around the vast perimeter of an open jail in Siberia. He thought I was a spy. Yet in these very places, I chatted to Siberian prisoner cowboys who ploughed the land and rode their horses with pride. As one prisoner remarked to me, '[w]ith towns some 300 kilometres away and with all this open space, escaping would be pointless'.

Such turbulence has not, thankfully, continued but in my current research, I have been concerned to see women prisoners perform beauty pageants for senior staff; an activity presented as a form of 'rehabilitation'.[6] It thus appears that the riddle of post–Soviet penality continues to be controversial, misshapen and subject to the ebbs and flows of Western ideas coming into the country at different points and at different moments.

The Russian prison research site is still going through a form of transition. Ten years ago, the economy was turbulent and brought to a total collapse. Nowadays, the economy is more stable but the political structures continue to ignite frustration and concern (and often humour). For example, over the years, senior public officials

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would tell me that they must turn up to work even if they do not get paid because that is the Russian duty. In the prisons, over vodka, pickled herring and poetry, I have mourned the loss of a distinctive 'Russian identity' – because Russia's transition has been chaotic, volatile and tragic. As Russian society engages with the world community, the once over-powerful penal system is trying to shake off the Gulag legacy and move towards openness, the rule of law and democracy. Yet to understand as fully as possible how the temporal shift from the autocratic Soviet society to a modern democracy impacted on the prison system, it was essential to embed myself deep inside Russian culture to establish some form of concrete knowledge of post-Soviet penal culture. When it is considered that for much of the twentieth century, a punishment 'fantasy' endured – that through forced labour and political correction, prisoners would be builders of the Communist Utopia – the scope for employing approaches which engaged with the political and cultural past surfaced increasingly throughout the research fieldwork.

In all of my work I operate 'inside' Russia: I first enter the field, I position myself inside or as close as I can to a prison, I share in relations and I engage in culture (speaking in Russian, giving English lessons and participating in cultural events). The assumption (from a research methods text-book point of view) is that I then 'disengage' from the achieved insiderness and from the deep ethnography. In Russia, the dimensions of culture were expressed acutely in the penal world. Culture was a collective entity that operated under the political doctrine of Marxism/Leninism and as a 'site of ritual performance and cultural production' (Garland 2006, pp.420-421). If the culture was changing into something new and unrecognisable from the past, then the status of my insiderness would be subject to some change and shift (as a woman, as a Russian

speaker and as a Westerner). In a community of such chronic tensions and change, the epistemological approach I adopted (and continue to adopt) was based on the motivations for, and management of, my own transition from my home communities to a researcher of a vast hidden community. For example, I engage in two processes when exploring the hiddenness and visibility of Russian penality.

First, as preparation I establish historical and political sensitivity by examining the relevant historical texts on Russian history (social and political). I strive to evade the trap of thinking that present-day versions of Russia's trajectory are unproblematic and not subject to revision. This involved gathering documents from the Central Lenin Library in Moscow and conducting a discourse analysis of various Russian language, penal policy and criminal law materials. Interestingly, I found an abundance of literature on Soviet penality, which revealed something distinctive about tension between hiddenness and visibility. Looking beyond the sentence, it was evident that the verbose language of punishment in Soviet times was coherent and naturally occurring when considered alongside the political doctrine of Marxism. Prisoners are discussed not as criminals in need of punishment but as fallen comrades in need of correction. If we compare this to UK penal policy changes in recent times, the difference could not be starker. Knee-jerk responses, contradictory presentations from politicians and emotional and frenzied approaches to imprisonment have become the principal feature of UK penal trends since the mid-1990s (Bottoms 1995).

There are managerial and political interests driving prison population rates so it was important, as a criminologist, to grasp penal politics in Russia by querying official definitions of problems and issues as they arose. I had to maximise both my criminological

perspective and my Russian Area Studies perspective. For example, as a criminologist, Russian penality presents a fascinating challenge as for almost 70 years culture was deployed in incarceration in the form of 'penal fantasy'. Such audacious myth-making allowed the penal system to operate way beyond crime control in the normal sense and the penal system grew and grew to excessive and inhumane proportions. As a Russian Area Studies specialist, I was entering a new territory – in every sense – and investigating a phenomenon not widely researched in contemporary social science but widely known and discussed among social historians. Being clear on these distinctive branches of scholarship and mastering the disciplinary nature of each proved useful in creating contextual sensitivity and safe-guarding against contamination of the research problem with my own values.

Second, as soon as I arrive in Russia, I employ a note-taking system that I revise daily, updating the ideas as I formulate them and developing confidence that there is no finality to the note-taking system. Fluctuations in the note-taking process assist me hugely in how to bring Russian prisons out of hiding. For example, in the past there has been regular scrutiny and judgment of my motivations (both personal and professional). This would take the form of small tests of my knowledge of Russian history, literature and culture, or stating publicly before interviews how I would organise my work and what ideas were informing the decisions to ask specific questions. On several occasions in the prisons, I have been asked to recite poetry and engage in vodka-drinking to demonstrate my veracity and 'Russian soul'. This has been a wearying and contentious form of scrutiny but it was also revealing of how hierarchies of power and order operated, and of how this hidden community itself changed and saw me first as a curious novice, and then as an expert. In the

following section, I describe and explain some of the methods I have employed in Russian prisons.

Employing cultural approaches

As mentioned previously there has been a great deal of academic interest in the process of doing research in the contested environment of the prison; an environment that is both hidden and visible and which operates under acute contradictions (Jewkes 2007). I've learned a lot from discussing prison research with other prison sociologists all of whom work in the field with integrity, professionalism and negotiation by establishing their own 'standpoint'. Yet these accounts tend to exclude the interplay between cultural ritual and doing research (ritual being an artefact of culture) (see Ferrell & Hamm 1998; Burawoy 2003; Garland 2006 for debates). Since conducting empirical work in Russian jails, the questions that have perplexed me over the last 13 years are thus: is Russia subject to the same or similar tensions between hiddenness and visibility in the penal realm? If so, what forms does this take and how can it be explained? The first answer could be found outside of the prison. The problems, pitfalls and experiences I face are not Russian-*prison*-specific, per se, but rather Russian-*culture*-specific because the cultural context and temporal shifts deeply influence the research process, as well as the ontological and epistemological assumptions and outcomes.

The emotional response of feeling as if one is an outsider is a constant burden for researchers who operate in completely unfamiliar cultural realms and where points of physical and emotional exit from the researched world seem blurred, oscillating and fleeting. Related to this is location. While I have acknowledged that political events can shape and define the contours and direction of incarceration,

often overlooked is how *culture mediates researcher identity* and the research process. My use of the term culture here relates to how a network of beliefs and attitudes underpin and codify practices that embody ideology. Rarer still do researchers recall how the chaotic bigger picture then necessitates radically altering one's language, lifestyle, dress and diet. The point I wish to make here is that Western research accounts expose a taken-for-grantedness possibly because there is a degree of material, economic and political stability in Western societies. Such a state of affairs, argue Ferrell & Hamm (1998), creates a form of Western scholarship that is conventional in its criminological complexion. Sibley takes this even further arguing that conventional methods produce *dangerous knowledge*:

> The defence of social space has its counterpart in the defence of regions of knowledge. This means that what constitutes knowledge, that is those ideas which gain currency through books and periodicals, is conditioned by power relations which determine the boundaries of 'knowledge' and exclude dangerous or threatening ideas and authors. It follows that any prescriptions for a better integrated and more egalitarian society must also include proposals for change in the way academic knowledge is produced (Sibley 1995, p.xvi).

Indeed in all prison research, a researcher can be turned away because the researcher possesses a stigma and a different-ness (free/law-abiding/'clean'). Yet in pursuing signs of hiddenness and tension, I was presented with the challenge of unravelling the symbiosis of the carceral and the cultural because the cultural in Soviet penology produced an artificial manufactured notion of deviancy.

Hence an approach close to cultural anthropology was adopted. Russian history provides many clues to penality's cultural

sensibilities. For example, in the USSR, like the penal environment, it came down to one of two sides: alienation from the USSR against a need to find a place in it. How do you trust a government you fear in such a culture? And what of today's government and the forms that criminal justice and criminal law takes, now that they are no longer used to mobilise the masses into a slave labour system? For example, as I am interested in mapping out structural changes in imprisonment (such as changes in penal practice and penal philosophy) part of that includes an examination of how deeply ingrained criminal justice attitudes, values and assumptions, which once projected an extraordinary and indeed remote (hidden?) ideology, have come to be transformed. How do penal actors attribute meaning to their actions in post-Soviet society and how truthful is this meaning in a fleeting, moving transition?

Journey over, getting closer

All of the papers presented in this Special Issue will invariably be connected by one common theme – that research into 'hidden communities' demands getting 'up close', with the researcher situating her/himself socially and emotionally proximate to respondents. I have already described the journey into the penal territory and the preparation I undertake. Once in the field, the research interview would often be prefaced with a form of social activity such as a stroll in the park, generous Russian hospitality with other prison officers or a visit to a museum at the weekend followed by more socialising with families and friends of officers. It felt like everyone who was connected to the prison through employment fell directly under my radar because of my distinctive status as a Western woman conducting research in men's jails. Conversations away from the formal research site of the prison focused on feelings from staff

about 'belonging' to an international profession of 'prison employment'. Staff felt neglected and misunderstood. Roles were rarely clarified by managers because the long-standing cultural norm of being a custodian of Soviet values, with its confident morale boosting mantras, had collapsed and given way to confusion, fear and isolation. Some of the dynamic features that embodied this confused penological environment can be summarised as deriving from a complex penal history inextricably linked to an autocratic political ideology; a penal culture that was audacious in its myth-making. This environment was a 'storied place' whose landscape was intended to enable the Soviet Utopia through a penal expansion that criss-crossed the country like a gigantic patchwork. Now no longer hidden in any sense, this penal landscape was scarred. Indeed, from a criminological perspective, this remains an *exceptionally visible* penal environment marked by confusion and inertia over what kind of system it can and should become. With some participants over the years weeping when probed about what it felt like to live and work amid such penal turbulence, culturally, for me at least, my research revealed a wounded society scarred by penality.

I have written about the effects of developing a cultural anthropological approach in this hidden community where the power between the researcher and the researched can shift radically. The conceptual framework I developed was based on the notion of the socially constructed self (Goffman 1963). I argue that deep immersion in prison worlds and their associated hidden landscapes creates 'productive turmoil' where the researcher, in an (often desperate) attempt to constantly feel accepted, is reduced to the role of gratifier. These methods include:

- speaking in Russian;

- deep immersion in conversations about the direction that Russia is heading;

- attending important events while living in the prison communities, to embed myself and become recognisable, stable and familiar: 'being part of the place';

- affectively sharing to create an empathy dynamic.

There were problems and pitfalls of cultural immersion as a methodological approach (over and above the standard social science qualitative and quantitative research methods that I use such as questionnaires, focus groups, semi-structured interviews and participant observation). These are discussed in Piacentini (2007). Briefly here, my concern (obsession?) to craft and communicate my empathy and commitment to the scholarship and commitment to all my respondents in a serious manner was rarely questioned in Russia and at home. The goal of becoming the expert and not solely the eye-witness was fundamental to bringing the prison world fully out of hiding. However, in Russia, this was off-set with constant scrutiny of how much I knew about Russia, surveillance, and interrogatory questions on my political views. Shame and blame sensibilities all became the stuff of the research interview. It quickly became apparent that in being sensitive to the constructed nature of hiddenness in Russian prisons, I become exploited by the environment's ideological constructedness, and my role and position as a researcher became subject to a peculiar mode of control by actors in a culture that was struggling to come to terms with itself after the collapse of the Soviet Union.

In summary, cultural differences, transition and change were all surfacing in this hidden penal community and my methods had to

give each recorded shift or change a degree of structure and specificity.[7]

Conclusion

The variation in Russian culture in the 1990s and early 2000s when I was living there from time to time produced a distinctive methodology that utilised cultural rituals to assess whether there was tension between hiddenness and visibility in Russia's penal system. Organising my experiences of doing research in Russia around affect first and cognition second (that is, around how actors felt about the changing carceral and cultural landscapes) created rich textures of meaning and understanding of penal change. I was witnessing first-hand how Soviet values came to lose their grip and fall away from traditional ideological frameworks. This led me to consider what replaced them and how bodies of norms permeate states in transition. One of my findings was that Western mandates such as human rights were imported into Russia but that the mechanistic, technicist and bureaucratic manner of their delivery transformed human rights into bureaucratic boxes to be ticked. This prompted me to think about how my work can contribute to the body of prison scholarship that views prisons as distinctively hidden and visible in specific ways (penal expansion, the language of risk, media–propelled fear of crime and the restoration of state authority). Outside of North America, Western European and Westernised societies, research into hidden penal communities raises a separate set of issues about the sources of knowledge that enter penal territories to make them more visible. The trajectory of penal change is different.

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[7] I refer to these shifts as 'Occasions of Penal Identification' (Piacentini 2004). The occasions are not unproblematic but they did provide me with a point of reference for researching Russia's hidden and changing penal community.

In the introduction, I referred to one abiding feature of Western prisons being the 'what works' agenda and the creation of a politics of imprisonment where offenders are viewed as threatening. In Russia, the abiding feature of *hidden versus visible* has been how the movement to modernise the penal system, to bring it closer to international visibility and therefore accountability, has been uni–directional. One aspect of this appears to be the wider political culture in the Council of Europe, which Russia joined in 1996. The membership accordingly brings with it a tacit assumption that Russia will reform its penal law, legislatures and criminal justice system to meet European standards. As an ethnographer, of keen interest was the discovery that Russia's hiddenness was countered by a visibility process constructed around bringing rights violations into the public domain. As I tacked backwards and forwards between pasts told to me and the presents I inhabited, the penal system and all those who lived and worked in it were absorbing norms brought into the country and written into its legislative frameworks for the first time in Russian penal political history. I learned that many in the prisons felt that the penal system was failing, not in the Western sense of failing to reform prisoners or failing to satisfy the national body politic, but failing to meet international 'standards' of human rights and modernisation. Through internationalisation and penal reform, Russia's penal system would be brought out of hiding and this is what makes Russian prisons distinctive as hidden communities.

In conclusion, bringing Russian prisons out of hiding, while exploring the features of its visibility, involves continual 'behind the scenes' excavation of the cultural milieu. Looked at this way, I now understand the hiddenness and visibility of the prison as a cultural question. According to Burawoy:

> […] the divergent orbits of ethnography and anthropology reflected the histories of the disciplines, but they are also responses to the era in which we live. (2003, p.674)

The Russia I inhabit is more stable to a degree now but it is also one that remains the subject of much scrutiny and turbulence. One concern is that in the penal sphere a post-disciplinary penality is surfacing; one marked not by human rights, judicial reform and the rule of law but one marked by legislation designed to curtail NGO activity and ongoing human rights abuses. Under these conditions, what does it mean now to continue work in this hidden community? The prison now exists in a country with volatile connections to other countries. In the 1990s the hidden penal community had the appearance of visible progress, gaining Western societies' trust by directing itself towards a mode of governance that was 'Western'. Today in Russia life is being re-composed back towards national interests, needs, desires and anxieties. Whether the clock is being turned backwards in the penal realm is almost guesswork. But if the past is resurrected in cultural attitudes or administrative organisation and management, is this sustainable? In the large body of prison scholarship, criminologists remain focused on researching how penal attitudes endure by exploring the tensions between hiddenness and visibility. In the case of Russia, it is certainly the case that away from the controversies, debates and scrutiny, it is those who live and work in the penal system – many of whom were absorbed into it during the Soviet period – that should not be overlooked.

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Overcoming Mixed Feelings about Mixed Methodologies: Complex Strategies for Research among Hidden Populations

Jeffrey S. Murer (University of St. Andrews)

Introduction

Choosing a methodological approach for any research problematique can be difficult; choosing such an approach for complex social research agendas, especially those exploring issues or behaviours among hard to reach or difficult to study populations, also known as hidden populations, can pose real challenges. This article presents the challenges encountered and approaches engaged in two studies among hidden populations in Central Europe. For any study the approaches taken must suit the questions asked as well as the frame of understanding sought. For both of the studies presented here creating a greater understanding was the goal of each project; it is in this goal that one may see most clearly the human aspect of social research. Research methods are forms of practice, socially embedded, linguistically constituted and socially constructed. They are modes of praxis for inquiry, contributing to social understanding, as opposed to the oft-presented language of object-like 'tools' in a research 'tool kit'. By approaching modes of inquiry as practice, the subjectivity of both the inquirer and the inquired can be mutually enhanced. In such an intersubjective exchange the social quality of research and the goal of human understanding can be best realised. Too often, questions of methodology are treated in purely instrumental or technical terms; by shifting the emphasis away from the researcher and his concerns and moving it towards the respondent and her

subjectivity, the question of methodology changes to praxis. This is especially important when conducting research among hidden populations, which often avoid participating in social research for a number of reasons, the least of which is being objectified or reduced to a singular behavioural practice or trait. It is this very act of objectification that destroys and denies subjectivity; creating research strategies that preserves or even enhances the subjectivity of the inquired is social research at its best.

This article explores two projects presenting the research goals and analysing the approaches taken to best ask the questions grounding the research. My overall research concern is with the preservation of the subjectivities of respondents. That is, I am concerned with methods to preserve the voices of respondents, to present their world-views and self-understandings in their own words, and elicit the articulation of such subject positions with the least intervention on the part of the researcher. In the presentation of the first project, I explore my research experiences while listening to and engaging with populations that hold political viewpoints and social attitudes very far from my own. In this, the article explores engaging with the "respondent/other." In the presentation of the second project, I describe some of the techniques I have employed in a larger research project to preserve and promote the subjectivity of the respondents while also using quantitative instruments. Concretely, the first project was connected with my doctoral research into the resurgence of Antisemitism in Hungary after the collapse of Realized Socialism. Here I present the considerations of working among ardent nationalists, many of whom expressed anti-Semitic sentiments, and the challenges of creating an environment in which these highly politically mobilized groups would talk about their own feelings and their thoughts on their political and social

articulations. The second project, which is currently being conducted, explores similar self-understandings and expressions of highly mobilised populations involved in 'radical' social and political action. This study, in which interviews are being conducted in four countries across six field sites, presents additional problems in terms of language access and translation. The challenge is to create a research approach which preserves the subjectivity of the interviewed while also allowing for cross-cultural comparisons. This challenge is redoubled considering that all of the populations involved can be considered 'hidden'.

Defining the Hidden

Populations are considered hidden when public acknowledgement of membership can be seen as threatening or stigmatizing, driving group members to hide or deny membership or to otherwise evade group identification or affiliation. Social researchers who want to engage and study such communities face enormous challenges, not least of which are finding such communities, gaining access, and building trust with group members. For researchers far more fundamental are the problems of simply designing the scope and frame of a study: the very definition of a population can be difficult as members of a given hidden population may see themselves very differently compared with the terms of a research proposal. A more technical manner of defining hidden populations is that they are communities without a sampling frame. This poses methodological challenges in addition to the conceptual ones. Even when the population is delimited by an act, i.e. using drugs intravenously or engaging in sex with partners of the same gender, questions of degree or frequency arise: for example, should men with only one male sexual encounter per year be included in a study of gay men? This proves even more difficult

when the community is marked by sentiment or opinion. Where does a radicalised political community begin and where does it end? How racist, xenophobic, or nationalist does one need to be to be considered among ardent supporters of racist, xenophobic or nationalist politics?

Such questions pose obvious problems for quantitative methodological purists, particularly those associated with philosophical positivism; how can racism or xenophobia be constructed in 'time- and context-free' generalizable terms (Johnson & Onwuegbuzie 2004; C.f. Nagel 1986). On the other hand, while such questions appear to demand the approaches suggested by qualitative purists such as Egon G. Guba, premised on the assertion that the 'knower and the known cannot be separated because the subjective knower is the only source of reality' (1990, p.18), traditional qualitative methods can make cross-cultural comparison difficult because of linguistic and cultural specificity, although these concerns vary across the disciplines within the social sciences. Anthropology for example, has used qualitative methods to conduct cross-cultural comparisons since its very conception. In Political Science on the other hand there has been a sharp divide between researchers employing qualitative or quantitative methods, especially among those engaged in the sub-discipline of comparative politics. Out of this divide there has been a growing movement over the past twenty years in the English-speaking social research communities for a mixed methods research paradigm (Patton 1990; Tashakkori & Teddlie 2003; Johnson & Onwuegbuzie 2004), however much of the work has centred on questions of sequencing between qualitative and quantitative techniques (cf. Cameron 2009). The mixed methods approaches presented here will focus on attempts to extend qualitative approaches to larger population segments, usually thought

of as only being achievable through quantitative analytic techniques. Similarly the paper will explore how mixed methods within qualitative approaches yield important avenues of insight for working within hidden communities. To illustrate my points, I will draw on my own work from two empirical studies dealing with highly politically mobilized communities. Although I was trained as a comparativist and theorist within North American Political Science, my research methods are largely derived from Cultural Anthropology, which I use to explore the psychological aspects of social and political identity working within Social Psychology.

Engaging the Respondent-Other

Often social researchers engaged in qualitative methods and working among hidden populations are motivated to present a more humanistic portrait of a given social group (Johnson & Onwuegbuzie 2004), one which through what Clifford Geertz called 'thick [empathic] description', will present the multiple perspectives or opinions or beliefs. These are sometimes presented as the alternate 'realities' of that group (Phillips & Burbules 2000). This interest in bearing witness to that which goes unseen, or to present the genealogies of knowledge that are often excluded from more widely accepted or popular truth narratives, is a powerful motivating engine of social research to bring forth to social consciousness that which often is ignored or marginalised. Such motivations are at the heart of action research and qualitative methods designed explicitly to counter the hegemonic assumptions that often underpin positivistic approaches (Chilcote 1994, p.39). This struggle has been at the heart of the qualitative/quantitative/ mixed method debate. Commenting on changes of the mode of academic production, Keith Griffin noted that increasingly, American-style positivist formal modelling,

particularly that associated with economics or game theory within political science, has become a dominant form of the social sciences, crowding out other approaches especially within Political Science, Psychology, and certain schools of Sociology. He wrote,

> […] research in the social sciences has become globalised [with] the effect […] of screening out eccentrics, radicals and critics of the establishment. The shift has further strengthened the mainstream and helped it silence heretical voices (Griffin 1991, p.6).

Much of qualitative social research works to counter such tendencies and has been the mainstay of resistance to this privileging of qualitative methods. Indeed, qualitative methods retain the 'social' as an important aspect of academic research, and it is the very basis of action research; as Michael Polanyi put it, 'knowledge is always gained through action and for action' (1962, p.47). This is an extremely important academic goal: maintaining the social in social research.

However, it is a frequent tendency for researchers when selecting a research subject or a community of respondents to choose a group for whom they have sympathy, or with whom they share a political social outlook. The sympathetic researcher promotes the experiences or perceptions of the subject community as part of the overall research project, and often suggests that by understanding the experiences of the subject community, the dominant or mainstream community may likewise come to engage the subject community with sympathy and compassion. There are, on the other hand, opportunities to encounter hidden populations with the same intention to promote understanding, and to perhaps ameliorate conditions that give rise to conflict or violence, and yet the research may not engage in promoting or advocating the experiences or

perceptions of the subject group. It is in these instances when a social researcher must engage a community with great empathy, but not necessarily sympathy. It is an engagement defined by the sort of respectful listening associated with the best intersubjective relationships, but it is not necessarily one of agreement. It is this aim, to represent the subjectivity of respondents, which is at the heart of qualitative research. There are many different techniques that can be employed under the rubric of qualitative research and each has it own repercussions on research with hidden populations. Being aware of how these techniques can be employed in conjunction with one another, each to complement the next and to compensate and balance for deficiencies, will vastly improve the overall quality of the research and the presentation of the population studied. This is of particular concern when the community of study can be considered a hidden population; one that either is so small in size as to have no statistical significance in any type of population sample, or one that is engaged in behaviours or holds social or political views that are perceived to be so divergent with social norms that they must be concealed from the public gaze. In the first instance, the fact that a group may have very few members does not mitigate the reality of their experience, nor does it render it socially, culturally or politically insignificant. A small number of terrorists does not render the problem of terrorism socially and politically insignificant, just as the remaining handful of fluent speakers of a language on the verge of extinction does not render their experience unworthy of note and record. On the contrary, research of and among hidden populations often documents a social phenomenon, which is ignored or maligned because it is misunderstood or was not even recognised in the first place. It is the very work of researchers within and among hidden

populations that creates the knowledge of which Michael Polanyi so passionately wrote.

Subjectivities and Social Research

The greatest challenge in social research is accounting for the very impact the act of engaging in a study has on the subject of that study. This issue has two sides: one side is the potential to objectify the participant such that there is an erasure of the subject by the researcher; the other and related side is the researcher not being aware of his impact on the research respondent, ignoring how their interaction – researcher and subject – creates its own environment. The first situation is typically produced in one of two ways. In the first instance the research respondent is treated in such a mechanistic fashion that she is transformed into an object by the researcher. Her existence is reduced to automated or highly regulated responses. Research design itself often drives a supposition of causality. The reductionist tendency often derives from the pursuit of ensuring that research designs are replicable as stipulated in keeping with the fundamentals of Western 'scientific method.' The very engagement in qualitative research methods can constitute a move away from fetishising replicability in favour of promoting the understanding of specific subjectivities. As Peter Banister *et al* have written '[t]he aim of qualitative research is not [so much] replicability as it is specificity' (2003, p.11). Specificity is further eroded when researchers actually fetishise themselves. That is in an effort to minimize the impact of any personal bias of the researcher on the research the researcher tries to eliminate herself from the study. In extreme cases the effort to 'eliminate' the 'experimenter's effect', through the incorporation of

randomised double-blind trials,[1] the researcher will never actually meet the subjects she is studying; the result is the destruction of the subjectivities of both the researcher and the respondents. The personal interests, motivations, insights, experiences, and intuitions of the researchers are designed out of the model, just as the human quality of the respondents as people to meet and engage are discounted if not eliminated. Such efforts fetishise the notion of replicability. Concerns of replicability can be balanced through efforts regarding questions of validity and particularly ecological validity, whereby researchers ensure that methods, materials and setting of the study approximate or are appropriate to the real–life situations under investigation.

Peter Banister *et al* (2003) suggest that replicability concerns in qualitative research can be offset by efforts to explore consistency across research among similar populations by other researchers, by conducting research in similar but different locations, or among similar populations at different times. Do similar populations with similar self-understandings and self-presentations manifest the same phenomena or produce the same responses (Banister *et al* 2003, p.11)? Toward this end, Ronald Chilcote suggests that far from trying to eliminate or minimise a researcher's personal bias, a researcher should be open and frank about her own research orientation, motives, and relationship to the subject of study (1994, p.29). By placing the researcher within her own research, it becomes possible to see more clearly the position of the respondents. This insertion of the subject position of the researcher into the research design allows one to overcome the second problem of not taking

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[1] In double-blind trials neither the subjects nor the researchers actually know who is in the control group and who is in the experimental group. The goal is to eliminate both the subjective inferences of the researchers, but to also minimize attempt on the part of subjects to perform the "expected" outcome.

into account the intersubjective quality of social research: the respondent altering his response for the researcher.

The simplest scenario of the respondent altering his behaviour because of the presence of the researcher is that the respondent becomes quite self-conscious about being observed. This self-awareness leads to monitoring and self-censorship, which in turn alters the actions in which the respondent would normally engage. Anthropology and sociology in particular have been concerned with this phenomenon. Ignoring such concerns can create conditions related to what is known as the Hawthorne Effect. The Hawthorne Works outside of Chicago, USA, commissioned a series of experiments between 1924 and 1932 in order to evaluate what changes in the work environment might lead to increases in productivity. Initially the factory managers were interested in whether increased or decreased light conditions would lead to better production outcomes; the researchers found significant but short-lived increases in productivity when they both increased and decreased the light levels on the factory floor. Similarly cleaning workstations, opening space on the factory floor and even relocating workstations all resulted in increased production levels (Landsberger 1958). While there has been a great deal of controversy regarding the meaning and implications of the Hawthorne Effect (c.f Adair 1984; Gillespie 1991; Mayo 1949), what is clear is that the workers were influenced by what they perceived to be the sought after response by the researchers. The research subjects were cognizant that they were engaged in a relationship with the researchers. In the case of the Hawthorne Effect, it was the researchers who did not recognize this engagement: the significance was only found twenty-five years later by Landsberger.

This can occur similarly in the case of strong normative conventions. Most members of contemporary Western society understand there is a strong norm against public utterances that reflect racism, yet racism persists. In such an environment it may be difficult to evaluate racist sentiments frontally. In such circumstances it may be necessary to employ multiple types of engagements through multiple iterations under different conditions so as to obscure the direct object of study, in this case racism, among a particular subjective population. Such research-based engagements must provide respondents the space to perform acts or make utterances they may not be inclined to do in public environments (Dovidio *et al* 2009). In this case it may be the very willingness or inclination to engage in such behaviours or make such utterances that make a given cohort members of a population hidden in the first place.

The recognition of this relationship is reflected in the Reflexive Sociology of Anthony Giddens (1991) and that of Pierre Bourdieu & Loïc J.D. Wacquant (1992). In this consideration of the relationship between researcher and respondent, it is important that the researcher is cognizant of the modes of inquiry and that the categories theorised and implemented in the research may well be her own. It is vital for the researcher to not mistake them for categories that the respondents may necessarily make sense of or use for themselves. In this way the sociologist should take stock of the cultural conventions, rituals and discursive patterns employed in the study of cultural conventions, rituals and discursive patterns used by the hidden population being engaged: a sociology of sociology. Further, just as the researcher is capable of reflection on the process of inquiry so too are respondents; respondents are capable of learning, adaptation, self-criticism, and self-reflection: reflexivity.

Ideally researchers should document and track the dynamic processes of adaptation both among researchers and respondents (Bourdieu & Wacquant 1992, p.213). These moments of reflection are themselves opportunities to understand the dynamic processes of social change and social recognition that may define hidden populations. In this, as Peter Banister *et al* wrote, 'subjectivity is a resource not a problem' (2003, p.13). To capture the different aspects of respondent subjectivity it may be necessary to engage in a multiplicity of research techniques associated with qualitative methods.

Empathic Engagements as Intersubjective Encounters

Quantitative methods and approaches to social problems most certainly have their place within social inquiry. However it must always be remembered that quantitative methodologies require quantification; that is only that which can be quantified – that which can be counted or measured – can be studied. This process of creating an objectified reality is itself a theoretical construction derived through abstraction and formalization. This process of moving toward idealised, abstract forms which can be verified, moves away from the subtleties and nuances of the everyday life-world. Yet, meaningful and significant cross-cultural comparisons can be made through qualitative methods. Moreover, qualitative considerations can inform quantitative methods in order to retain the subjectivity of those engaged and observed. In order to relate how multiple techniques associated with qualitative methods may be employed so as to capture the subjectivity of respondents and contribute to both the validity and the specificity of a social research project, I present below two studies in which I was intimately engaged and responsible for designing. Across the two research designs I employed a full range of qualitative techniques associated

with ethnography: observation; informal and formal interviews and questioning of what I observed; interviewing as narrative research; discourse analysis; action research; focus group analysis; and visual anthropology. The first project is the research I conducted in the mid-1990s for my doctoral dissertation; the second is on-going and comprises a larger, international, multi-site collaborative research project exploring youth socio-political radicalisation and the potential for youth support for violence. The second study is similarly a mixed method research project that also employs quantitative methods. Thus I conclude with a discussion on applying qualitative sensibilities to quantitative methods.

In the autumn of 1996 I engaged in a year-long data collection project of exploring the resurgence of anti-Semitism in Hungary since the collapse of Realised Socialism in 1989. I believed the resurgence of very public expressions of anti-Semitic sentiments – such as graffiti, political pamphlets, public pronouncements by political figures, expressions on placards at rallies, and the circulation of jokes – was related to a sense of disappointment on the part of particular elements within the Hungarian polity. The end of socialism did not bring about what certain segments of Hungarian society anticipated and hoped for, at least not for them. Additionally the presentation of these sentiments coincided with a resurgence of public expressions related to losses resulting from the end of the First World War. The Treaty of Trianon became a by-word for the accumulated losses associated with the end of the First World War, ranging from the loss of the Emperor Franz Joszef in 1916, through the collapse of a short-lived 1919 Soviet Republic, to the 1920 annexation of nearly two-thirds of the territory of the Kingdom of Hungary to the newly created states of Czechoslovakia, Yugoslavia, the enlarged Kingdom of Romania, and the newly formed though

truncated Austrian Republic. This period culminated in a vicious White Terror pogrom. In the mid-1990s, following the collapse of socialism, public narratives reviving these events circulated throughout Hungary.[2]

To explore these phenomena and to assess whether they were related it was imperative that I be in the field, and that I meet people engaged in these activities. I undertook a great deal of observation, but I was also concerned to maintain a distinction between observation and participant observation. I would attend many rallies, meetings and marches of those expressing disappointment with the trajectory of the transition since the end of Realized Socialism, which frequently included people publicly demonstrating anti-Semitic sentiments. However, it was important for me to remind myself that I was merely an observer and not a participant. This was often difficult when attending a rally, or march or other demonstration, because my very presence could be interpreted as another body expressing support for the political and social views advocated by the organisers. Yet, it was important to me that I maintain an acute awareness of my own subject position, in that I did not in any way feel that I had to pretend to support their political or social positions in order to walk among them. As much as I did not want to participate in their activities, it was extremely important that I was among them; for in order to conduct a proper ethnography I had to ask questions to better understand what I was observing. This is the driving motivation of ethnography: building understanding (Hammersley & Atkinson 1983, p.17). Moreover, I used my own reactions and feelings as points of connection and even as information themselves. My emotional responses from being among these crowds formed the bases of my questions, and thus were a

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[2] Murer 1999a; Murer 1999b; Murer 2002; Murer 2009.

means of concretising concepts. I did not ask about hypothetical responses, I could use my own.

While present at these public demonstrations I collected numerous publications, flyers, pamphlets, and other sources of printed material to subject to discourse analysis. These textual sources also became a first corner for triangulating ecological validity. These published texts could be compared with the transcripts of spoken text produced in the form of interviews, especially formal interviews, as a second corner. Finally, press reports, articles, books and the personally related observations of other scholars, academicians, government officials and reporters would form the third corner of triangulation. In addition to using discourse analysis in order to imagine and engage the worldview of speakers in their own terms, I would also engage in narrative research, exploring how individuals wound the story of their own lives together with larger collective narratives marking both significant personal and collective events. Analysing how respondents assembled their life stories provided insight into which historical events they perceived as having a profound impact on both their individual and collective identity formation.

Often these various strands of research would coincide through the textual analysis of images. I found it extremely useful to present respondents with photographs and to ask them to explain to me the significance of the image. Even when the significance of an image appeared quite self-evident, I found it useful to ask a respondent to explain it in her/his own words. In this way I was not engaged in speculation, but asking a respondent directly to explain his or her subjective relation and understanding to a particular set of texts or images. Perhaps the best example of this is the photograph in illustration 1. I saw this placard while attending a rally in Budapest in

1997. Although more than 100,000 people attended this rally, the placard pictured below stuck out to me. The icon in the centre of the placard is of course a Star of David, a symbol associated with Judaism. The triangles composing the outer parts of the star are coloured red; the interior hexagon is coloured blue. The title of the placard states simply: 'The Current Coalition.' The caption at the bottom of the placard continues: 'A picture which needs no explanation.' I approached the young man in his twenties, who was holding the placard, and asked if he would explain it to me. After looking at me incredulously and pointing out that the placard made it clear that the picture 'needed no explanation,' he was happy to explain the exact meaning to me. He stated that the then current political coalition was an alliance of liberals – depicted by the colour blue – and the former Communists *cum* the Hungarian Socialist Party – depicted by the colour red. He stated that since everyone knew that liberals and communists are Jews (a favoured term at this rally was '*liberalbolshevik*' (Murer 1999a; Murer 1999b; Murer 2009)) the coalition was depicted with a Star of David. It was important that I did not merely speculate that this was what he meant. By asking him, I had his direct language and his direct interpretation of what was to be understood by reading the placard.

Illustration 1: Placard 'depicting' the composition of the Hungarian government at a Nationalist Political rally in 1997. Photograph by the author, Budapest, 15 March 1997.

Perhaps it was the most important interview I conducted. In both the formal and informal interviews I was very attentive to the relationships in which I was interested: how did the respondent feel about life since the collapse of socialism? How would the respondent characterise her political or social position before the collapse? What were the respondent's thoughts on liberalism as a political or social project, and what did she think of capitalism as an economic project? What forces does she believe are directing politics, domestically or internationally? What are the significant markers in both the respondent's personal life and those in Hungary's history? I found it important to give the respondent as much room as possible in which to navigate these tricky topics. I also found it important not to prompt any responses, as pre-determined categories would limit the

range of responses. Even more importantly discrete yes/no responses had to be avoided at all costs. The format needed to provide the greatest range of options and the most conceptual room for the respondent. I made great efforts to pose questions in a comparative framework, inviting the respondent to place herself within a continuum of her own description. Even when a respondent would make a direct response addressing the larger themes in which I was interested, I would press for further elaboration. I made a point of stressing that I wanted to be perfectly clear in my understanding, and I would ask similar questions repeatedly to triangulate the responses, and be assured that the interpretation was not merely my own. By repeatedly asking the respondent to relate to the themes and questions that were theoretically important to my study, I was engaged in Bourdieu's reflexive analysis of my own scholarship in my relationship with my respondent. I also conceptualised this relationship in terms of what is known within psychoanalysis as the therapeutic alliance.

At the heart of the therapeutic alliance is an intersubjective relationship based on trust. While the relationship is a real one, it is not a friendship, nor is it reciprocal, insofar as the analyst has a professional responsibility that is not reciprocated by the analysand (Meissner 1996). This is parallel to the professional responsibility that the researcher has to the respondent. However, the metaphoric parallel to the therapeutic alliance is all the more profound along this axis of responsibility, for in the clinical setting the analysand seeks out the analyst, where generally it is the researcher who seeks out the respondent. Yet just as the focus on the analysand's experience and interpretations requires the analyst to listen closely and carefully, and to be present and emotionally available, neither the analyst nor the researcher should share their personal perspective lest it overly

influence the analysand or the respondent. When, for example, I was asked by respondents whether I am Jewish, I would respond by asking how a response from me either way would make the respondent feel. I would use such moments as opportunities to explore hypothetical situations, but they were not of my construction, they were of the respondents'. Facilitating the expression of personal constructions is extremely beneficial to this kind of research.

Mapping the Self, Locating Others

Personal construct approaches are at the heart of the second project. This psychological theory, most often associated with the American psychologist George Kelly (1955), begins with the assumption that individuals organise thoughts of their own experiences and world–views in highly coherent ways. Kelly and others believed that there was much to be gained by listening to the articulations of subjects regarding their own interpretations of the world, and their experience, without direct interventions or directions by the psychologists or researchers. This theoretical and methodological approach is often associated with 'constructivism' whereby concepts or truths do not exist in their own right, but are constantly and socially negotiated between individuals and groups, each of whom have their own notions of what is being negotiated. It is the agreement on these concepts that creates and holds their form. This is a highly discursive process of social engagement. The challenge of this second research project, which aimed to incorporate this approach, was to design protocols and instruments that would best allow respondents to portray their experience, as they understand it, but would also as utilise quantitative tools to best allow for cross language comparisons.

The project – the European Study of Youth Mobilisation (or ESYM), sponsored by the British Council, directed by myself and a team of researchers at the University of St. Andrews, and executed by local academic research teams – seeks to compare the political and social experiences of young people who express themselves along a diverse set of political and social orientations and to see if they share motivations, self-understandings, senses of purpose, or senses of agency. It explores the concept of political and social radicalisation among young people aged 16 to 30 in eleven countries across Northern, Central and Baltic Europe, including Scotland. The study explores young people who are motivated by religious identification, whether Muslim, Christian, Hindi, or otherwise; by ethnic identification, for example Kurdish, Roma, or Dutch; by class politics, for example on the Left, anarchists, trade unionists, socialists, or Marxists; or community politics, for example on the Right integralists, nationalist, or skinheads; by environmental politics; or by something altogether different and defined in an exclusively local context. With so many different potential strands of motivation the St. Andrews team decided that the total number of people to be interviewed should exceed a minimum of 1000 and will ideally approach a level of 1500 individuals. The St. Andrews team also decided to limit the study to young people found in certain designated research cities. At present, the cities include Bratislava, Brno, Budapest, Dundee, Glasgow, Krakow, Prague, and Warsaw. In the future research will also be conducted in Copenhagen, Helsinki, and Tallinn. The interview instrument was designed to capture a great number of qualitative concerns, particularly those arrived at through personal construct approaches. By remembering Bourdieu & Wacquant's (1992) admonition noted earlier in this paper, we sought to create spaces for respondents' personal

constructions of meaningful categories. However, this created enormous logistical and methodological problems. In the present research sites alone there are five official languages, let alone numerous other languages spoken by immigrants and other communities. Translation was to pose an enormous logistical challenge. Moreover, it would be difficult to engage and assert the validity of discourse analysis if it was all presented in translation (or if the analysis was conducted in translation). For this reason the St. Andrews team decided that formal interviews would be conducted through a survey instrument in official country languages, but would be largely reported in quantitative terms.

Additionally the ESYM research is comparing different methodological approaches to mixed method enquiry. The ESYM research will compare three different approaches to respondent selection. In Bratislava, Brno, and Prague respondents were found through traditional chain-referral methods. To find the initial respondents – or 'seeds' – who would serve as the bases for the follow-on snowballing chains, local researchers engaged in ethnographic research, observing where young people would congregate and discuss politics. In the second instance, in Budapest, Krakow and Warsaw, the local research teams look to expand beyond the more traditional, non-probability methods of snow-balling or location sampling, and engage in a more sophisticated mode of respondent driven sampling where the biases associated with chain referrals are analysed in such detail as to suggest a known level of precision (Heckathorn 2002). To facilitate this, the Hungarian and Polish teams will engage in a very thick ethnography to find the necessary respondent 'seeds', and will be very interested in the social

network relations between respondents.[3]   This will be compared with the largely ethnographic-only study in Scotland where each respondent will be selected separately by the researcher without scrutinising the relationship between respondents. Thus the ESYM study will compare results derived from chain-referrals, respondent-driven sampling, and ethnographic interviewing.

In all three case studies ethnography is a key method to initiate the study. This appears similar to the most common relationship in a mixed method study: ethnographic research functions as a pilot study before the more traditional quantitative instruments are deployed. However in this case, the ethnography is not separate; it also directly facilitates the application of the survey instrument. The ethnographic component is essential to the overall design of the study, as it provides not only the contacts for the interviews, but also the content and the context. The concerns of local groups were incorporated in the interview structure both as a means of connecting with the respondent, but also as an indication that the researchers are taking these concerns into account, and are listening to the various groups. This was not done to parrot young people's concerns or articulations, but rather used to shape the interview in a way that better connected with the respondents. By making the interview process responsive to the data and observations collected through ethnographic engagement, the respondents understood that

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[3] Respondent Driven Sampling (RDS) was developed by Douglas Heckathorn in 1997 as a means of overcoming what had long been a problem in working with hidden populations or populations of a very small size. Chain referral sampling or snowballing offer insight into those engaged but offer little in the way of knowledge of prevalence, population size or the relationship of those engaged to the population as a whole. Heckathorn devised a mathematical balancing for the non-random fashion by which participants are recruited. In RDS respondents recruit their peers to participate, and the researchers track who recruited whom and their number of social contacts within the network. This is done through coding so respondents can remain anonymous but the relationships within the social network can be tracked and evaluated.

the researchers had not simply dropped out of the sky. It is this demonstration of responsive social engagement, as opposed to cold, clinical scientificism often associated with political research, that opened access to many groups and also, in my estimation, provided much better interviews in which many respondents felt they could be spontaneous and disarmed.

Moreover the instrument was designed to capture many qualitative aspects of social and political engagement. Much as grounded theory explores concepts and terms developed by respondents and engaged communities of study, the local expressions and terminology contained in the survey were determined through local focus groups with young people similar to the target population. Local researchers explored terms that best expressed the ideas that theoretically ground the research; by following the decisions made in the focus groups, the terms incorporated in the survey instruments are expressed in a fashion that would be recognised locally and by the target respondents. Thus, we used the words used by the young people, and in the ways that they would use them in everyday encounters with each other and on the street. Indeed one of the biggest challenges in designing the interview script was finding the proper wording for the most basic question: 'with whom do you discuss politics?' This may seem straightforward enough from an academic standpoint, but for many of the young people we hoped to interview they did not believe that they are involved in 'politics'. For most 'politics' suggests formal, parliamentary political parties and elections only. For many of these young people having opinions about immigration, the economy, or the role of religion in society is not considered 'political', but this is precisely what we as researchers were interested in knowing more: the politics of the everyday. In addition to the focus groups and

ethnographic observations to structure the interview scripts, the interviewers made reference to particular local events that were the most prominent of the day. So the interviewers would ask 'with whom to talk about local event X?' Even though the answers were being recorded in a numerical form, the respondent and the interviewer completed the question schedule together. Interviewers were briefed about the approach to the overall project and discussed with project managers ways to draw out interview subjects. Most importantly all field teams were briefed on the need to promote intersubjective engagement through all aspects of the research. By referring to specific local events, the interviewer prompted the respondent to discuss his or her activity in a more conversational and less abstract fashion. These interventions also built trust through the course of the interview, reinforcing the more conversational approach to the formal interview.

To compliment the information reported quantitatively, the survey includes sections in which the respondents are asked to express themselves with reference to presented maps, provided pictures, and self-constructed drawings. Thus in addition to being asked how they relate to a number of political and social situations by identifying their level of agreement or disagreement along a numerical scale, respondents are asked to place themselves graphically within a spatial representation of political attitudes. Further respondents are asked to map or draw their social network. They are asked to diagrammatically portray where they see themselves among their friends, colleagues and family. They are also asked to map their relationships to authority and to power. The graphic representations will help the research analysis teams interpret the respondents' numerical expressions. This also functions as a primer to encourage a more open discussion with the respondent regarding her social

network, and became the basis of the randomised selection of network members for the next waves of the respondent driven sampling.

Finally, respondents are asked to map their conceptions of the cities in which they live and are politically and socially active. As can be seen in Illustration Number 2, below, the respondents are presented with a map illustration of the research city. They are asked about their perceptions of the dynamics of the city in terms of where different economic classes reside, where crime occurs, where they feel safe and where they do not, and where they perceive change to be occurring. The respondents then draw on the page indicating their perceptions of the changing environment around them. Also on this page, respondents indicate along a single axis where they would position themselves with regards to binary terms: left/right; liberty/order; non-believer/religiously devout. Each of these graphic representations is then coded to correspond with a set of numerically represented scales or item blocks within the survey (Dixon & Durrheim 2003). A similar combination is deployed in the project's use of the life event calendar whereby respondents represent their life story and personal narrative by denoting particular events and their significance graphically within a time-line that relates both chronological (*chronos*) and social (*kairos*) time (Axinn *et al* 1999). In each of these cases the ecological validity of the study is sustained and enhanced as the research setting, both spatial and temporal, is explored with the respondents (Banister *et al* 2003, p.11). Drawing the respondent into the research in this way both recognises his subjectivity, engaging him as a knowing active subject, and enhances the strength of the study itself.
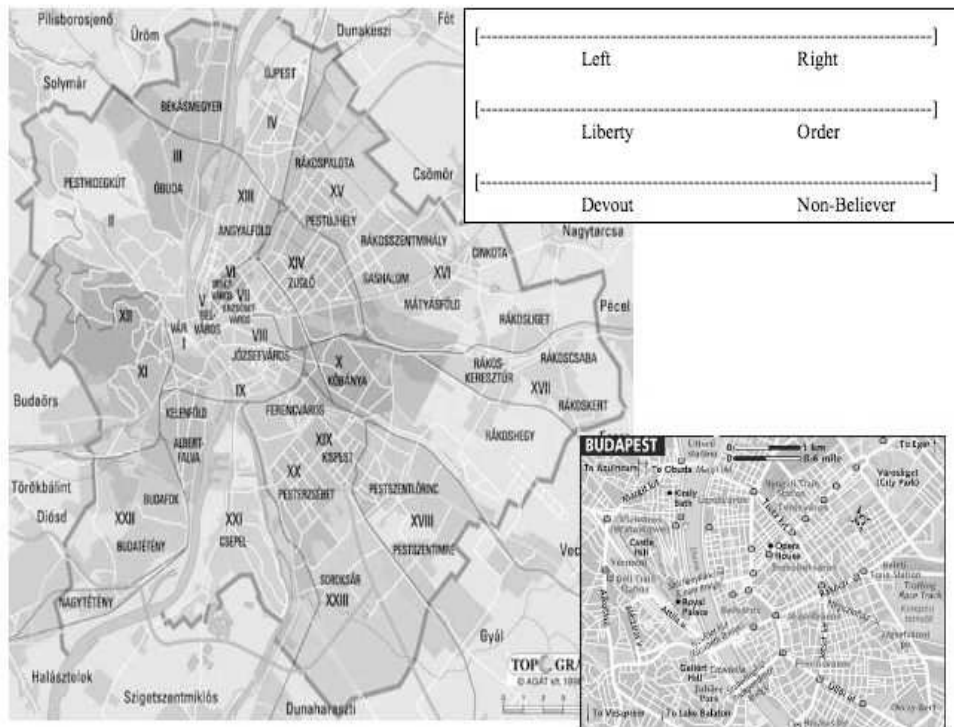
Illustration 2: Reproduction of Research City Map, here as Budapest. Respondents indicate on the map where they believe changes to be happening in the city.

One of the main considerations driving this research project was generating as many interviews as possible, with young people holding as many different political and social perspectives considered outside of the mainstream as possible. Much of the research regarding political involvement has been conducted on a very small scale (for example, see the work of Andrew Silke or John Horgan). Further, many of the recent studies (i.e. since 2001) concerning radicalisation in Europe focus on young Muslim men (see Bakker 2006, Tille & Slootman 2006) . The point of our study was to listen to as many young people in the field sites as we could and then compare their experiences both across political persuasions within a given field site as well as both across different and among similar positions in other

field sites. The greatest challenge was gaining access to the various political communities we were interested in engaging. It was the ethnographic portion of the research that provided access. Just as in my personal research in Hungary more than a decade ago, the key to access was trust, and trust was built through the act of listening. It was this principle that structured the formal interview. Through the use of ethnographic research before conducting the formal interviews, the interview script could include insights and bridges for connection engendered by previous interactions between the various groups and the researchers. In this way, the formal interview script and content were structured and refined by a range of methods: engaged observations through ethnography; discourse analysis of those engagements; and focus group discussion of these findings. This made the engagement between interviewer and respondent less distant and less cold. Each research component contributed to the next for a complex mixed methodological approach to the research question of exploring the self-understanding and motivations of young people involved in political and social mobilisation outside of mainstream politics.

Conclusions

One of the most challenging aspects of working with and among hidden populations involves avoiding the kind of reductionism whereby members of a group are identified with a single quality or a single set of qualities. Striving to recognise the subjectivity of respondents and to promote a greater human understanding should be one of the main goals when exploring hidden populations in any research study. Such research goals necessitate the creation of open spaces for respondents by not limiting the response ranges to pre-determined choices or prompting respondents with a nod toward a

normatively preferential choice. For example, by providing neutral contrasts or comparisons, or by providing distancing primers that suggest a large range of valid responses none of which are specifically aligned with a position (especially a normative position) within the given socio-political environment. These open spaces can be created through the idiom of engagement in the therapeutic alliance. In this space the researcher, like the analyst, listens without judgement; yet the researcher can also use her reaction to the respondents' presentations as a data point, just as the analyst must evaluate her own feelings of counter-transference against her perceptions of transference by the analysand. This type of engagement focuses on the respondent, and just as any interview violates the norms of polite social discourse, the therapeutic alliance idiom makes no pretence to being a regular conversation. It is a special kind of dialogue where the rules of polite discourse can be suspended, yet it is one made by profound empathy and careful and energetic listening of both participants' parts.

These attempts to maintain and ensure the recognition of the subjectivity of respondents can inform both qualitative and quantitative approaches. While these goals of social research may be more traditionally aligned with quantitative techniques, it remains important to explore the various modes of interface among a range of qualitative techniques, just as it is important to analyse how qualitative and quantitative methods can be mixed. This is a long way from the declarations of twenty years ago that 'the accommodation between paradigms is impossible' (Guba 1990, p.81), leading some academics to advocate an 'incompatibility thesis' which precludes the complimentary employment of qualitative and quantitative modes of inquiry (Howe 1988). Rather, researchers should look to mixed methods as a mode of inquiry that builds upon

complementary strengths and has the potential to reduce or compensate for methodological weaknesses. What should be chosen is that which presents research subjects with the greatest humanity and builds a greater sense of social understanding, regardless of the positions taken by the groups under study. The researcher, therefore, shapes the methodology of the study as a mode of engagement, as a mode of praxis. This may be the best goal for social research.

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