The main point I want to make in this methodological and reflexive chapter is the extent to which the research and interaction carried out for this book was experienced as emotional labour. In addition, I want to highlight the tension in presenting conflict, however prosaic it may seem to an outsider, between informants and the problems this presents for the ethnographer’s commitment to protecting privacy. The chapter also discusses the role of the researcher in more general detail and some disciplinary concerns.

The preface for this book began by highlighting the long-term engagement of the researcher with the field before any intention of ‘research’: I visited a village\(^1\) near Izluchino in the summertime for a number of years before starting fieldwork. I had no intention of doing ethnographic work there. It was there that I met some of the initial informants who feature in this book as summer neighbours. The influence of that semi-rural experience is reflected where I discuss the impact of the town on the rural hinterland, and vice versa. However, for most of the book, I have tried to keep a ‘low profile’ and try to foreground as much as possible the voices,

\(^1\)I should clarify that this was not originally a dacha settlement, but a ‘real’ rural settlement which had progressively been taken over by urban dwellers for country cottages.
as well as the interpretations, of the people in the research. Of course this is a neat and well-debunked conceit in anthropology. The whole text, including the choice of informants’ talk is mediated through the researcher. Nonetheless, because class identity is still an underresearched topic, particularly in contemporary Russia, I have been at some pains to step back as much as possible and concede space to the stories of the people in this research.

To readers there are two obvious points where my attempt to stay in the shadows breaks down—in the ‘car’ and garage talk, and in the chapter about Lyova’s drinking. Both these field contexts are a good starting point to discuss the researcher as entwined in the lives of the researched.

In discussing Nikita’s entry to the underground workshop and his building up prestige with older workers, I talked about his interpretation of the benefit of having a noteworthy foreign guest come along with him to the garage, even if my knowledge of car mechanics is risible. Adams’ (1999) findings about playing the ‘mascot researcher role’ are familiar feelings to researchers working in Russia, and my relations with Nikita in particular were no exception. Something of the tension in terms of control over identities is clear in his eagerness to have me with him in different contexts. I certainly gained from this initially in the second fieldwork (2010), when Nikita ‘adopted’ me and through him I gained access to a significant number of younger men and women. On the other hand, the extent to which I was able to break out of the ‘mascot’ role, where the researcher is constrained by the key informant, is also clear and relates to my confidence in my linguistic abilities—discussed at the end of this chapter—as well as the relatively marginalized position of Nikita himself within the social dynamics of his milieu (unmarried, living with parents, remaining in lower-paid work).

Nikita was in some respects keen to keep me ‘his’, and show me off to his own benefit. Nonetheless, ‘stepping out’ of the role (Adams 1999) and accessing other research contexts was made easier by personal attributes that might be seen in other contexts as inhibiting access to social groups of young working-class men: age, class and marital status. Having family, including small children, with me at various times allowed me to deflect some of Nikita’s demands on me as a mascot, at the same time as helping access to other groups. Performing roles of responsibility (professor and
father) was also important: ‘Nikita, I really have to go to talk to Petr now about his kid’s medicine/about his work’; ‘Nikita, no, I really can’t drink anymore, I have to go to sleep now!’ These formulae were useful ways of showing what a stick-in-the-mud I was, but also facilitated frank talk about the personal lives of informants themselves who often reflected on differences in age, marital status and so on. This is clear in the previous two chapters, for example, when Petr certainly aligned his more ‘grown-up’ attitude to going to work for the TNC in Kaluga with my professional status. At the same time, I experienced the emotional impact of this on our mutual friendship as something of a betrayal of Nikita by me. While I did share with him translations of earlier publications based on our talk, isn’t writing about the critical attitudes of some of Nikita’s confrères towards him a double betrayal? My feelings of guilt are an indication, not only of my ethical dilemma, but perhaps show that I did not break the power of the initial mascot role ascribed to me.

Nonetheless, the ethical values of friendship and loyalty among most, if not all the people in my research, are robust despite everything. Hopefully, despite the frankness of this book, people like Nikita and Lyova will not experience my informant ‘promiscuity’ as a betrayal. ‘Uncle’ Lyova, ten years older than me, is trickier. In the initial period of fieldwork (2009) I lived in his flat. At one point our relationship broke down and this was because I transgressed the boundaries, not of my mascot status, which did not exist for Lyova, but because, for him, being his guest and doing research were incompatible. This is linked to the feelings of shame and anger generally, but also to durable moral values linked to the working-class household in Russia (and elsewhere) of propriety, circumspection and suspicion. For Lyova ‘doing academic work’ was writing at a desk, not talking to all and sundry about god-knows-what! At one point he warned me, only half-jokingly, ‘you know there is rampant syphilis in this town?’ Traditional views on gender relations often involved the projection of fears and fantasies of informants on to the researcher.  Even in twenty-first century Russia, a lone female taking a taxi ride can result in

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2Nancy Ries (1997: 77) presents analogous stories from a woman’s perspective about the sexual dangers projected of consorting with male informants that were impressed on her during fieldwork.
devastating gossip. Similarly, I encountered incredulity (more often from married men than women) at unchaperoned interaction with young women. ‘You are lucky to have such a trusting wife’ was one of the milder comments.

Lyova was ‘Uncle’ to me because of our prior relationship. This I acknowledge in retaining his honorific title throughout, as I often do in conversation with him. But after our falling out, in my first period of fieldwork, this relationship was under threat. Good relations were only restored long after I had left his house, and after I had made a number of efforts to ‘pay my respects’ to him in a suitably contrite manner. Lyova’s power was connected to our prior relationship, and his seniority as an ‘uncle’ figure, as much as our relationship, facilitated research. It shows the difficulty and importance of managing informal ‘authorities’ in the field as much as formal ones (Adams 1999; Enguix 2014). Lyova had an avuncular attitude to me. At the same time as acknowledging conflict and disappointment (my research material would have been richer with more of Lyova’s involvement) I must actively choose to acknowledge the empathy and friendship between us as inseparable from describing his experience. Reflecting and writing about zapoi is nearly as much emotional labour as being there and seeing it. Empathy not only facilitates ‘data’ collection (Enguix 2014: 83), it has to be generative of meaning and meaningful ethnographic encounters.

As with Lyova, my relations with the owner of Steelpipe, Saraev, required the acknowledgement of both status-nearness (professionalism) and distance (his superiority as entrepreneur, manager and older man). While I understood the need to concede power, I retained more professional prestige in middle-class informants’ eyes by virtue of their acknowledgement of my institutional and ethnographic authority (Enguix 2014: 82), the latter underlined by linguistic competence. This certainly facilitated ‘passing’ for a trustworthy interlocutor on numerous occasions in the suspicious and stressful atmosphere of Putin’s Russia. In addition, in the case of interviewing the Moscow sales director of Cement, I concealed my first-hand knowledge of the actual workings of the plant and my interactions with workers, more so as not to influence his talk, than to protect informants. I have to admit I also gained some satisfaction from this too.
Insider and Outsider in the Field: Foreignness, Gender and Class

In their examination of the researcher’s positionality in fieldwork in both Turkey and Azerbaijan, Ergun and Erdemir (2010) discuss how foreignness and cultural familiarity interact with research contexts. They summarize well some of the problems with insider status that are particularly relevant to the Russian context: an ‘insider, for example, may be perceived as being untrustworthy because of his or her knowledge of and connections to the community under study’ (17). Outsider status allowed a degree of greater access (as well as distrust and disbelief by others) in Izluchino.³ Did the lime kiln technicians genuinely believe that our conversations might get back to the director (see following text and the next chapter)? Russian reality suggests that their fears are reasonable. While cross-cultural issues constitute the ‘elephant in the room’ for foreign area studies researchers working on Russia, outsider status can help not only to mitigate but also to reverse the researcher–researched relationship, particularly when it is understood in terms of cultural exchange (Walker 2011: 216, 224). This is no less true as Russia moves further away from its closed past (if anything since the Ukraine conflict, a sense of cultural difference has been emphasized by the state itself and people are more inquisitive than ever about ‘representatives of Europe’). By the same token, my foreignness allowed me to witness, both first-hand and in stories, significant illegality—particularly in the informal economy, but also in terms of stealing from work, and so on. What possible risk would there be from a foreigner—the status of whom in Russia is always viewed as contingent, powerless and temporary? At the same time, what Ergun and Erdemir call ‘cultural proximity’, evidenced by linguistic competency and lived experience, allows a researcher to cross over temporarily into partial ‘insider’ status which can build rapport, trustworthiness and openness (2010: 18). For these reasons, in the research conducted for this book, the insider/outsider distinction was therefore ‘frequently situational, depending on the prevailing social, political, and cultural values

³ Compare Mah’s reflection on ethical issues related to outsider status in three deindustrializing contexts of her research (2012).
of a given social context’ (Kusow 2003: 592, as quoted in Ergun and Erdemir 2010).

In terms of relevance for the conduct of research in this book, perhaps more important than nationality is the intersection of masculinity and class. As recounted in some of the material about cars and garages, it was disconcerting and amusing to sometimes be credited with undeserved ‘status’ for basic knowledge about mechanics. Getting my hands dirty under the bonnets of cars was significant, even if most of the time it was more symbolic than effective. The episode where Nikita credited my presence with at least partially smoothing his way into informal employment shows the potential knock-on effects of small acts by the researcher in getting involved in the lives of the researched on their own terms (to which I will return later). At the same time it shows how simple acts of openness to the world of informants are essential in carrying out research in such contexts. Perhaps more important than verbal and textual empathy (which can be both presumptive and reductive, and of which there is a long critique in anthropology—see Metcalf 2002: 52), showing one’s willingness to engage in practices important to those in the field can be practically effective (which sounds rather instrumentalist unless approached sincerely) but is also a political act. What if the researched are potentially violent racists (as in Pilkington’s recent Russian research, see 2010a)? Fortunately for me, such involvement was mainly restricted to drinking alcohol in the morning and fiddling with carburettors, but nonetheless also shows the significance of the classed performance of researchers.

The lived experience of socio-economic class is at the heart of this book, but by no stretch of the imagination can I lay claim to a working-class identity. In 2014 a fellow academic participant at a conference questioned me about my research, saying, ‘You must be of working-class background to do such research.’ How to answer this, apart from: ‘No, but …’? In many cases the choice of research topic is related to the political views of the researcher and my case is no exception. But the danger in over-identification towards the other is romanticizing of the other’s life world. This is not the same as empathy and is something both Pilkington (2010b: 233) and I are at pains to avoid. But the reader will be the judge of that. Certainly the class background of the researcher is rarely even a
matter of note these days in ethnographic accounts. Part of my reflection on class is to remark how difficult it would have been to gain trust among blue-collar workers in my own country and culture to do similar work. Therefore once again I would point to the significance of ‘foreignness’ in contributing to a mixed and mobile insider/outsider status (Walker makes a similar point: 2011: 224). Certainly the research was helped by the extent to which my non-working-class background was obscured or rendered less relevant by foreignness alongside my displaying empathy and sincere engagement with everyday practices.

But perhaps this too is disingenuous—too neat and patronizing. I am reminded of bell hook’s words ‘no need to hear your voice when I can talk about you better’ (1990: 241). In so far as the research for this book has been successful, it is where (if?) that process is both empathetic and based on shared experience with the people who feature in it. This is not the same as co-production, participatory action research, or even collaborative research (in the sense proposed by Pink 2007), but it does try to meet the requirement in co-production of mutual respect and emotional engagement, as well as tensions and conflicting interpretations, both between the researcher and the people researched and between the people of the research themselves. This is the real sense of participant observation that I have tried to carry out in Izluchino. And it is in that sense of participant observer that I wish to contrast the observation of participation—proposed as part of the reflexive turn some decades ago (Tedlock 1991), and which remains a source of debate as the term ‘ethnography’ gains ever more traction in disciplines outside anthropology (Ingold 2014).

How can I make these immodest claims, considering that the researcher, in deciding on the final written word of the research, has much more power than the researched? This idea of ‘giving voice’ to the ‘natives’ is sometimes a tired cliché in anthropology and can lead to psychological reductionism (Metcalf 2002) or more often serves to obscure power relations rather than reveal them. Nonetheless, ethnography as a method retains its political potential for emancipation, and this extends to postsocialist polities and peoples (Pickles and Smith 2007). Stenning and Hörschelmann (2008) draw on emancipatory theories of postcolonialism to argue for the imperative of postsocialist scholars to understand
the multiplicity of the present (as globalized, as revealed in the everyday, the long durée), and the presence, within that present, of the past. Verdery provides the insight that postcolonial studies’ focus on the practices of domination in contrast to area studies’ preoccupation with the political is needed (Hann et al. 2002: 17). Others point to the need for researchers to continue questioning the suitability of the applications of normative categories like ‘social trust’ and ‘civil society’, and exhorting a renewed examination of how everyday moral communities of socialism have changed (Hann et al. 2002: 10). I argue that none of these imperatives can be fulfilled in postsocialist studies without better engagement with the lifeworlds of the researched. A participatory and intimate ethnography is needed.

‘Participation’ and Intimate Ethnography

Is the sense of participant observation I suggest possible without a commitment to ‘intimate ethnography’? Barbara Rylko-Bauer proposes the latter, where the personal and the emotional inform the meaning of research throughout (2005: 12). Numerous works on ethnographic methods call for an ethical commitment that goes beyond avoidance of harm and a respect for informants and which includes the imperative to return and share with informants the research to which their involvement contributed. This is a necessary corollary of ‘confronting [one’s own] subject position as researcher and challeng[ing] their own field of power’ (Pilkington 2010a: 213). However, in practice many researchers find this impractical. More likely, for many it is inconvenient or risky to both the validity of their research and their further access to the field. As discussed earlier, in the politics of engagement, empathy and commitment to the people of the field are proposed as prerequisites (Enguix 2014: 91; Katz 1994). However, emotional openness in the feminist mode proposed by Pilkington and her colleagues (Pilkington et al. 2010) is only part of the answer. Given that the majority of my time was spent among men, respect and ‘participation’ can only be generated in ‘doing’—the attempts at production and ingenuity so highly prized and constitutive of working-class men’s lives. In Sasha’s talk he exhorts me (patiently and assuredly) to ‘try to do it; someone will...
help you’. Why wouldn’t I be able to build a fish tank from scratch if I had around me an assemblage of respecting confrères? The expectations are that without the ability to do manual work for oneself, one cannot be considered a normal ‘bloke’. At the same time, note how different from work-on-the-self this is: entrepreneurial yet situated in work-for-the-self.

While prudence, circumspection, propriety, and sense of justice are often put forward as relatively stable working-class values (Sayer 2005; Skeggs 1997), the ability to try to ‘do’, and not to shy away from useful and decorative practices of domestic production is key. It would be foolish to claim that I was always successful. While Nikita and I learned together how to replace his car’s thermostat, Sasha was always highly critical, if hypocritical: ‘You pay someone to fix your car?’ This was to ignore his own well-documented car problems that he himself could not solve. The intimacy of this ethnography relates clearly to participation in practices, labour and ‘shared experience’, over and above empathy through commitment. Without participation, the lived experience cannot be communicated adequately, but without empathy participation can only reveal half-truths. A good example is the ethnographic portrait of factory piece-work in socialist Hungary. Haraszti intimately participates in shop-floor life, describing the repetitive work as ‘mak[ing] love without loving […] I do not feel it’ (1977: 113). His overall picture, however, reproduces an intellectual’s view of manual labour. In the end, despite being a very emotional, personal portrait of factory life, it remains an unconvincing reduction of the workshop to a Marxian depiction of total alienation of labour from production. Apart from an acknowledgement of the passion of workers for gleaned material and DIY ‘homers’, it lacks empathy for the more nuanced meanings and affordances of working-class life. The ideal of participative ethnography can mean different things in different circumstances. I wasn’t always able to follow informants into their working lives, but, as with the example of Denis ‘oiling up the chain’ and drinking early in the day, merely subjecting oneself to rhythmic situations is useful. Writing recently on immersive ethnography in therapeutic communities, Michael Seltzer refers back to Goffman’s imperatives for us to ‘subject ourselves, our bodies and our personalities to “… the set of contingencies that play upon a set of individuals …”’ (Seltzer forthcoming: 1; Goffman 1989:125).
Dilemmas of Privacy, Harm and Conflict

Along with participation and emotional labour comes the dilemma of privacy and presenting conflict between informants. One of the points of this chapter is the importance of avoiding ‘hidden ethnography’ (Garifzianova 2010: 208), where emotional effects of fieldwork are concealed or brushed over. Drawing on Lutz and White (1986), Garifzianova discusses her fieldwork with skinheads in Russia, pointing to the significance of the emotional labour of the field and its contribution to the production of affective and corporally experienced knowledge (2010: 209). In that my research involved emotionally charged friendships (Nikita, Sasha), as well as conflict (with Lyova, Sasha), it was experienced as emotional labour. A second element of emotional labour is the way that the researcher can become a conduit for affective forces in the field—Katya’s colleagues ‘crying into her waistcoat’ described the reliance of all those around her on her stoic and uncomplaining qualities of leadership and management. Nonetheless, the emotional conflict between her and her brother and husband are the formative backdrop of our conversation about being a ‘strong’ woman at work. Indeed, so severe was the effect of the failure of her marriage on those around her that I have largely omitted discussion of it. The researcher as the recipient of the emotional energies of researchers brings with it the dilemma of respecting privacy. Just as significant is a third kind of emotional work that comes with dealing with those materials that make it into the ethnography. Earlier, I have briefly described feeling that I betray Nikita for a second time by writing about his conflict with others. Similarly, I feel uneasy in writing about Lyova and his drinking. I begin that chapter with a remark that tenderness between us remains—does this not strike me in retrospect as part of an elaborate self-justification for betraying his privacy and in detail describing his alcohol use? This is the other side of ‘intimate ethnography’.

The stories of Lyova and Katya are more stand-alone cases, but the exploration of Nikita’s life is returned to repeatedly and in ever more intimate detail throughout the book. I discuss the disparaging attitudes
of not only the young women in his circle, but the conflict with his sister and close friend Petr. How can this be squared with a commitment to privacy and non-harm to informants? After all, as I mentioned in the previous chapter, Nikita was keen to read what I had written about him and surely reading about what others have said would constitute ‘harm’. The dilemma is clear, but without foregrounding Nikita’s and Petr’s contrasting attitudes and conflict about the worth of work and worker, the assimilation and rejection of neoliberalizing imperatives within the lived experience of people in Izluchino would not be possible. Here again, intimacy reveals the deep analytical content of practices, actions and talk. As a researcher I can only learn of this through experience of conflict; revealing that learning betrays privacy. As is evident from the lengthy informant quotations throughout this book, the problem of privacy is compounded by many of the people not believing that what I was doing constituted research; even those who were initially guarded let down their guard relatively quickly. This was not for want of trying to make them informed and consenting participants. Especially with people like Nikita, the ethical issues around friendship or ‘over-rapport’ (Hammersley and Atkinson 1995; 111–2) appear not only unsolvable but also, at least partly, misplaced. If one is to sincerely engage with the world of the researched, how is ‘over-rapport’ not inevitable or even welcome? As long as we retain a commitment to reflexivity, as Coffey argues, most of us never truly become part of the cultural setting we study and can maintain an intellectual balance between familiarity and strangeness (1999: 31–3, 37). As the extreme pole of total participant in the lives of the observed, the only compromise solution that I can propose is the masking of concrete informant identities by using composite portraits. This allows the reporting of informant talk in its entirety, but removes in part the identification of speech with specific real individuals. Thus, while elements of the ‘real’ Lyova are retained, the ‘harm’ to his privacy through point-to-point identification of the ethnographic Lyova and the real is mitigated by building into his story elements of other informant lives. Once again, the reader and informants will be the ultimate judge of this.
Finally I wish to return to language competence and prior engagement with the field. Having lived in Russia for some time in the 1990s, I came to the experience of fieldwork with arguably better language skills than some fellow researchers working in and on Russia. At the same time as presenting the prior immersion in the field and language skills as advantages, they were also experienced as anxieties for me throughout the period of research and writing of this book. These two points are related.

In the Anglophone academic world there is fortunately still intellectual space for researchers who do in-depth, hands on ethnography of postsocialist spaces, including in area studies departments like my own, or in sociology and anthropology. But the model for research on the former Soviet Union, at any rate, remains policy- and politics-orientated in the social sciences and mostly desk-based (cf. Caldwell 2004: 14 on the methodological ‘distancing’ of researchers and research objects in Soviet studies). In addition, the old guard of generalists are retiring. These are colleagues who combine high-level language skills with expertise in both social science and significant cultural knowledge. While some researchers at the forefront of grounded social science research in the region have rightly called for more engagement with the local, with the everyday, and implicitly, for more ethnographically based research (Flynn and Oldfield 2006), the wind is blowing in the other direction. Another point is that studying Russia remains highly politicized, and this extends to the type of social research carried out. Particularly during the most recent Putin term, the voices of researchers who seek to highlight the everyday experience of Russians have been subject to marginalization at best, as the political perspective dominates—as if the everyday were not itself political!

This may seem strange to those working in sociology or anthropology research environments, but in a geographic area of study dominated by political science and international relations, ethnographic research on Russia is always going to provoke some unusual responses. For example, the sincere and incredulous question from colleagues: ‘Why do you visit Russia every year? Why for so long?’ A myriad of problematic issues in
area studies and its cognates lie behind this question. ‘Isn’t your research a bit like journalism?’ ‘Surely a couple of weeks in the field are enough for interviews?’ ‘Aren’t you taking a translator with you?’ ‘Why are you transcribing your own interviews?’ ‘Why are you hiding the identity of your respondents? Surely that invalidates your research?’ ‘Basically, ethnography is just making stuff up, isn’t it?’ The last comment was said jokingly, but was all the more revealing as it came from a senior researcher with excellent language skills and many years in-country experience, but who had spent his career in a ‘policy-orientated’ research institute.

But this discussion is perhaps a dead end. It serves merely as an illustration of the research and disciplinary context of this particular researcher. If justifying ethnography to some colleagues is an exercise in overcoming suspicion, it has long been the case that fieldwork itself has been compared to espionage: ‘a shifty business carried out by individuals regarded by the general community with suspicion’ (Hendry and Watson 2000: 1). And this is particularly true of Russia, where even the most ordinary people are highly—one might say genetically—attuned to the dangers of expressing an opinion to strangers. Ethnographic work on socialism and postsocialism has always been interpreted politically (Hann 2009: 135). This remains true today and perhaps even more so than many realize. Even the choice of using translators and transcribers is not without pitfalls (Turbine 2007: 52–3), and can also be seen as political (such as when translators ‘correct’ substandard speech). This is where the language skills and prior engagement come back into frame.

For example, take speaking Russian (well enough): ‘You dress like a CIA officer!’ said the marketing director of the Cement plant. We had been sitting in his plush office in Moscow for two hours talking about workers in Izluchino and their tendency to steal diesel fuel from the tankers. Relieved at my willingness to conduct the interview in Russian, the executive had, at least to my mind, unburdened himself fully, revealing all kinds of juicy details. Suddenly towards the end of the interview, on my revealing that I had travelled not by car, but, horror of horrors, by metro, he burst out with the comment on my choice of dress: blue jeans with a cream blazer and purple tie. I had to think for a few seconds—he was referring to the jeans and jacket combi, but also had only just fully taken in that we’d been speaking in Russian for the last two hours. At
the other end of the spectrum, the charming lime kiln operators Alina and Dasha had point blank refused to speak to me, despite their supervisor and a main gatekeeper telling them who I was and reassuring them: ‘Whatever we say will get back to Markov [the Director], your Russian is too good for you to be only interested in workers.’ This was not the first time that relatively fluent Russian aroused suspicion, both of my person and of my intentions. Perhaps, as in other branches of area studies, too close an engagement with the country of study is seen as ‘professionally suspect’ in the neatly packaged neoliberal university, just as ethnographically focussed, low-cost fieldwork is seen as ‘anomalous’ (Duffield 2015: 2, 14).

On the other hand, my first conversation with Filipp and Nikita, in the *DeKa* where they worked, sticks out and relates both to language and prior engagement with the field. Nikita (having opened a beer) was telling me about the town and the prisoner labour used to build it. He then went on to talk about the inherent laziness of Russians and of only wanting to work for money for the next drinking session. Then Filipp burst out: ‘What are you saying! Just look at him. Can’t you see he has the measure of us already? He knows us inside out.’ Both linguistic competence and time in the field marked me out as something else entirely. Flattering, of course, but disconcerting nonetheless. Occasionally, with people I got to know better, I experienced the classic ethnographic situation of having to pretend I didn’t understand context, or even language. This was sometimes incongruous.

Finally, there is the problem and blessing of ‘disbelief’—and this relates to both language and field engagement. Language first: linguistic ‘disbelief’ in terms of an advantage was clear in some field encounters, somewhat in contrast to the points made here earlier: there were clearly occasions when informants did not believe I understood as much as I did, in terms of either cultural context or language. But then this returns us to another issue: potential harm to informants and invasion of privacy. This works the other way as well. Sometimes I genuinely did not understand everything, and this was also a source of disbelief. Disbelief relating to current research endeavour was also sometimes a field advantage: having been used to me for so long as part of the summer life of the village, there was a measure of blurring of my partial insider status
and researcher which resulted in greater candour. At the same time it increased the expectations on me and raised again ethical dilemmas when informants revealed particularly intimate details and did not believe that they could feature as written research.

But again, pursuing further the linguistic and personal history aspect of the researcher may also be a dead end and risk the ‘fetishization of individual reflections on fieldwork’ (Pilkington 2010a: 211). While the other issues of this chapter on emotional labour, positionality and ethics are no doubt more interesting they cannot be divorced from language use and nationality. Indeed, discussion of linguistic and cultural competence remains an uncomfortable subject in anthropological debate (see, for example, Metcalf 2002: 1; Bryant 2004).

References


