# 6

# EVERYDAY ETHICS

## A personal journey in rural Ireland, 1980–2001

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### Introduction

There is today, among many parties and interest groups, a deep concern with the ethics and practice of research which involves human or animal subjects. To some extent, the issues being raised are similar to what anthropologists have long talked about, at least since my own graduate student days during the mid-1960s. However, the present concern has a different context. It is largely driven by an obsession with accountability and auditing which permeates contemporary institutional life in the public sector. Interestingly, this dovetails with, and is also now driven by, postmodern and post-colonial critiques of anthropology which emerged in the mid-1980s. The various practices which have been developed to address these concerns mean that, in theory, all parties can now be satisfied: the institutions through which we practice anthropology (government bureaucracies, universities, funding agencies), our more reflexive-style anthropological colleagues and the people among whom we do research and about whom we write.

In this chapter, however, I question what this recent ethical turn has accomplished for socio-cultural anthropologists. I do this by comparing monitoring procedures and reflexive moments with the materiality of daily life in the field and in writing ethnography. I do so by exploring my own experiences as a Canadian academic doing anthropological research in Ireland over the past 21 years.

### Defining and auditing ethics in the academy

At York University in Toronto, anxiety about ethical behaviour in research has given rise to a permanent 'Human Participants Review Sub-committee' consisting of a multi-disciplinary cross-section of academics. The sub-committee must formally approve any research project that involves human subjects before it can begin.[1] Its aim is to ensure ethical research, through



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'informed consent', that is, to ensure that no harm will come to subjects/participants and that all participants have been thoroughly informed about the research and have made an explicit decision to take part.

These aims are pursued by requiring university researchers to fill out a questionnaire which is vetted by the sub-committee. The questions include what informants will do (e.g. 'stimuli, ... tasks, ... tests, questionnaires, interviews, number of sessions and time required') and whether there are 'any foreseeable risks and benefits' for participants. It also asks how the researcher will inform people about the research so as to obtain their consent. There are three choices. The researcher can ask participants to sign a form which contains a description of the research. The researcher can send potential participants a letter outlining the research and their roles in it. Or the researcher can tell potential participants about the research and obtain their consent verbally. If verbal consent is to be used, the researcher must provide the sub-committee with 'a rationale on why the informed consent form is not being used' and 'provide a draft of the verbal statement'.

The York University questionnaire assumes a formalism in research design which posits highly structured interactions between researcher(s) and participant(s). No gesture is made to the main activity of anthropological research, namely, participant observation and its location in such everyday, on-going interaction as attending public meetings, buying meat from the butcher or chatting with people at the petrol pump. This omission is not accidental. That the questionnaire not only requires anthropologists to justify *not* using a formal consent form but also requires them to produce a formulaic speech, suggests that there is grave suspicion in the academy about how anthropologists produce their data, about those who do not conform to positivist stereotypes and about getting everything in writing. What does such practice have to do with ethics in anthropology?

## Permission granted!

Since 1980, my partner and colleague, Philip H. Gulliver, and I have carried out long-term, intensive research on a small town (population 1,450 in 1991) and rural hinterland (population 600) in south-eastern Ireland. Over the years, we have spent numerous long summers, an autumn and two sabbatical years in this locale (Thomastown, County Kilkenny). We have accumulated extensive archival materials, semi-formal interview data and field notes from participant observation. It was in 1995, as I was filling out the ethics questionnaire in order to gain access to a new research grant, that I realised I was under suspicion. I therefore made an especial effort to explain to the non-anthropologists on the York sub-committee what it is that we do most of the time. I was pleased with my efforts (Case 1); so was the sub-committee. I was not only given research permission but I was commended for my clear and thoughtful statement. Thus did I allay the fears of vigilant outsiders and



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define, through their idea of informed consent, the essence of ethical behaviour for anthropologists.

### *Case 1: 'Informed consent' in anthropology – a 1995 application*

Anthropological fieldwork is premised on a mutual rapport which must develop between those who are doing the research and those whose society and culture are being studied. Usually, all the participants – anthropologists and locals – are adults. The relationship between them is mediated by mutual trust: a breach of such trust, by either side, destroys the rapport which sustains the research and may destroy the entire project. Anthropologists therefore must tread carefully, and always with respect for local and interpersonal mores. For the behaviour of the anthropologist is subject always, and continuously, to community approval.

In such a research context, formalising consent to a piece of paper or a formulaic recitation destroys what it is intended to protect. It does this, first, by breaching interpersonal etiquette because it questions the trust that must underlie the interpersonal relations on which the research is based. In other words, it removes the right of local people to consent to the research in the way in which they believe their consent ought to be given: by their intimacy and their participation with the anthropologist's work. Second, given that anthropological fieldwork is long-term and on-going, over months and even years, formal consent – whether written or verbal – at one point in time removes people's right to withdraw consent at a later time, and to deny their past involvement, if they later wish to do so. In effect, it disempowers people and leaves the way open for the abuse which the form was designed to prevent.

Thus, it is unwritten and non-formalised verbal consent which best typifies participant observation in anthropology. It means that people's consent must be renewed each day – through their continuing interaction with the researcher and the project, through their help, co-operation and assent. This is the accepted style and nature of ethical anthropological research. Phrased another way, anthropological research can only take place in the light of informed consent – given continuously, openly and graciously because we are behaving, and have behaved, properly.

## Informed verbal consent: Crafting identities and rapport (unconsciously)

The sub-committee had agreed with me that 'continuity' and 'mutuality' underlay ethical anthropological research in terms of rapport, trust, respect, etiquette and rights. The lengthy questionnaire however, and the sub-



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committee's happy acquiescence, started me thinking about the implications of what I had written. Could I really behave so well? Had I? Was everyday life so transparent?[2]

Clearly not. My response, like the sub-committee's questionnaire, had been concerned with establishing parameters and practical rules. Despite being time-consuming, this exercise had been fairly easy. Life in the field, however, has been far more complicated: it has never been simply about codes and canons but about better or worse choices. What standards, criteria and norms did I in fact use when in the field? Why?

## Identity formation as ethics and field technique

The recent anthropological concern with authority and representation has tended to ignore what one of my graduate students said (and what I have long felt) about fieldwork: 'When you're dependent on people for information which you desperately need to write a thesis, it's hard to believe that you're the one with the power. I have never felt so powerless in my life.' In Thomastown, County Kilkenny, even after two decades of 'being there', my sense of being dependent has never left me. In fact, it has become more complicated through time.

Initially, when Philip Gulliver and I first settled into Thomastown in mid-1980, local people[3] were concerned to discover 'who we were' – especially, as we were later told, after the tourist season had ended and we were still around. We explained to anyone who was interested or curious, or anyone we wanted to speak to, that we were Canadian university professors writing a history and description of the parish. This was understood and acceptable.[4] However, it only answered the question of *what* we were doing. It did not address the more fundamental and important question of *who* we were.

Thomastown people had long had experiences with 'outsiders': in-marrying spouses, in-coming migrants and notables, tourists, visitors, and so on. The result was that this category had little import in the locality. Instead, so-called outsiders were always assigned more nuanced identities. In Philip's case, he spoke with an English accent which several people, as a result of sojourns in England, had been able to trace to the Midlands. He clearly and explicitly wasn't a Catholic. Was Philip then a Protestant? Not really, as it gradually became obvious that he never attended services, that he did not enter the local Protestant network and that he professed, openly, to be an agnostic. Nor did he have any Irish 'connections' (kin). Philip, therefore, could be labelled a 'blow-in' – one of the many English who, over the years, had wandered into the area, stayed a while and then left, and whose political attitudes were suspect. Philip's 'outsideness' was known and knowable.

In contrast, I was Jewish. In 1980, I was the first Jew that the vast majority of people there had ever encountered. As one woman noted: 'I



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thought you were different. You're dark.' Seldom, if ever, did anyone take this further and express an interest in Jewish belief, practice or history or in my own ancestors' migratory past.[5] I was, simply put, exotic and strange – an 'other'. This was bolstered by my anomalous gender: I was in my mid-thirties and childless; I was a professional (confirmed by the post office delivering letters to Dr Silverman, not Mrs Gulliver); and not only did I lack crucial homemaking skills but I was never at home anyway. Even my one familiar feature, that I spoke with an 'American accent', was vociferously belied by my repetitious claim that Canadians were different.

In such ways, our outsider status became fixed as 'blow-in' and 'other'. Interestingly, these definitions gradually became part of our own sensibility as to who ourselves were in Thomastown – as individuals and a couple and as anthropologists. On the one hand, it affected how we comported ourselves. Tales of the marital squabbles of other blow-ins from the past led us never to exchange a cross word with each other in public. Consciousness of Ireland's colonial past vis-à-vis England made us ultra-Canadian. So, when we brought forward in conversation our own experiences as analogies (e.g. about buying cars, weather, farming, road traffic and so on), we never used English examples, only Canadian ones. We colluded in being the butt of mild humour: 'Here come the Canadians, better turn on the heat.' I listened, always silently and sometimes painfully, to tales of privation and poverty, but was never asked about the pogroms and violence in my own mother's past. Awareness, too, of my incomplete role as a woman led me into several homes to learn how to bake amid unspoken amazement at my faulty education.

On the other hand, and at the same time, Philip and I slowly began, albeit only in part consciously, to use our identities as blow-in and other as a means for cutting through the formalities and privacies of much of local life and for establishing rapport, eliciting information and building the social life which would ease participant observation.[6] For example, we began inviting people to dinner in the evening – a very unusual way of socialising at the time. And we invited couples, in a world where the social activities of men and women were largely separate. We took long walks over farmers' fields in a rural space devoid of walking paths, hikers and country strollers. We met several (surprised) farmers that way and were rewarded with lengthy conversations. Most important, perhaps, in our visiting, talking on the street or in shops, and in simply being visible, we cut across the status–class and class barriers that comprised the socio-cultural map of the locality: we were seen by farmers as we spoke with farm labourers, by workers as we spoke with capitalists, by shopkeepers as we spoke with the elderly poor, by professionals as we talked with the unskilled. Our identities, as blow-in and other, gave us the freedom to bend conventions and to craft novel situations and relationships. We used this more and more consciously and unashamedly as time went on.



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## Reciprocity and confidentiality

Essential features which underlie social and cultural life in Thomastown are reciprocity and equivalence. These permeate all extra-household life, from the most accidental encounter to the most structured. From casual greetings on the street and comments about the weather, through to casual and thence intimate gossip, from pub behaviour to extending favours, gift-giving and proffering invitations, it is imperative for people to maintain a semblance of balanced exchange and both moral and material symmetry in all relationships and interactions. All local residents – natives, blow-ins and others alike – are invariably implicated in this behavioural code.

One of the most important exchange items which moved along local networks was gossip, shared in varying degrees with others according to a person's knowledge.[7] Most important was the fact that gossip was communicated according to an expectation of reciprocity. As anthropologists, we were seen entering and leaving homes, driving in a particular direction, talking to people, attending public functions, joining local clubs and associations. Who we became acquainted with, as well as what we were learning about the locality and its people, invariably became part of the gossip which circulated. Conversely, people approached us to learn what we might know. We had no professional or personal problem with being the objects of gossip. However, how far could we actively trade in this currency? How far could we afford not to?

To elicit information required that we provide information. How could this be done without breaching the promise of confidentiality which we continually emphasised? How could we even put information about ourselves into circulation by chatting with some people since this suggested a favouritism which might limit our access to others? After several months in the field, we found ourselves unable even to articulate to local people who else we had met. We had become closed-mouthed, unsociable and inaccessible.

We gradually came to realise, however, that in so doing, we had put another item into circulation which could be used in exchange for information. This was our promise of absolute confidentiality combined with our growing knowledge of the past. Through our archival work, we were collecting information which, we decided, could and should be shared with the people whose ancestors it concerned. We thus began to exchange genealogical knowledge and gossip about the past for information about the present as well as about the past. We became known as experts in local history and tracing 'connections' (genealogy) and, for most people, as unwilling to share any information except with those whose pasts and ancestors it concerned. This solution to the ethical dilemma had the unintended effect of adding to the pressures which were pushing us more and more into an exploration of Thomastown's past – as we mined for information to exchange,



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as we happily saw a way out of an ethical conundrum and, more importantly as time passed, as we ourselves became increasingly fascinated with that past.

During our early years of fieldwork, this exchange worked well. This does not mean that everyone spoke with us, that those who did shared more than the most superficial information, or that everyone came to trust our promise of privacy. It did mean, however, that we spent more and more time in archives and that our lives in the locality came to conform more and more closely to the segmentary model which an informant had once described for her own social world: 'There are those people I know to see', she said. 'Then there are those I chat with about the weather. There are then those I gossip with and, finally, there are a few who will tell me almost anything.' Invariably, and perhaps predictably, as we kept coming back to Thomastown over two decades, the most intimate zones began to yield up new moral contradictions. What happened, in other words, when informants became friends?

## Escalating ethical choices

In mining for historical information about the Thomastown locality, we began to move from seeing data in terms of their exchange value to seeing the material as objects for detailed examinations of the processes of socio-cultural change, the political economy of domination and exploitation, the patterns of continuity and discontinuity over the *longue durée*. We quickly found that other local people, long attuned to the emphasis on history through various educative institutions, were also interested in Thomastown's past, usually as a matter of excavating facts or as antiquarian preservation. Over time, as we ourselves became more immersed in the past, many of the people whom we came to know best were those who also had a concern with history. Enter another ethical problem: how far should we share archival information with local historians? These historians had themselves often collected materials about the distant past (the nineteenth century or earlier) and sometimes had had access to sources no longer available. We thus began to share archival data, such as records of deeds and property conveyances, through multiple photocopies and carbon paper notes. This quickly became the norm. More recent materials – such as parochial records and civil registers of births and marriages in the latter half of the twentieth century, or probated wills – proved difficult.

However, anthropologists know that friendship ties can bear a good deal of imbalance. As Sahlins famously said: 'gifts make friends but friends make gifts'. Friendship, in other words, does not always depend on balanced exchange but can move towards a relation based on more generalised reciprocity. Thus, the longer we have remained in the field, and the more often we have come back to Thomastown, the more has the emphasis on balance slowly given way to more generalised exchanges with numerous



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people. We have become increasingly able to withhold what we deemed were sensitive archival data without ever having to explain explicitly that we were doing so. Local historians, our friends, came to know that we freely shared most materials but, when not sharing, we were not only respecting the privacy of others but also demonstrating that, equally, we would protect the privacy of our friends. Friendship and trust, alongside reciprocity, had thus allowed for boundaries to be drawn and expectations to be defined and respected in relation to archival materials.

In relation to gossip about contemporary people and events, our solution became easier as time went on. This was because, as we became more connected with local networks, we were more privy to common, public information. It was public because it always began with: 'Did you hear about ... ?' Thus, we happily exchanged this information, giving out what most people already knew. Conversely, we never spoke about those whom we had interviewed; we never passed on personal/private information; and we never even mentioned the names of people whom we knew or had interviewed or to whom we had spoken.

Most recently, however, such moral certainty has been undermined. The canons of long-term, intimate friendship (mutual visits across the ocean; wedding invitations; financial help) increasingly and invariably demand informality and the ever-more-loose exchange of information. This conflicts, many times a day when in Thomastown, with the need to retain the confidentiality of far less intimate others. Where does my professional self end/begin and my personal self begin/end? Can anthropologists really maintain intimate friends in the field site?

## Informed verbal consent in everyday life in the field

Within the general context of the decisions which we made about the presentation of our public selves, inserting ourselves into local networks, and reciprocity and confidentiality, moral choices embedded in the minutiae of everyday life became relevant. First were the daily decisions which had to be confronted on the spur of the moment, and quickly, when we bumped into people casually. The second took place in more formal meetings which we arranged to visit people in their homes to talk to them about their family histories and enterprises (farm, shop, business) or to discuss their particular areas of expertise (e.g. the community council, workfare, trade unionism). Moral dilemmas in this setting could often be anticipated and choices made beforehand. This did not mean, however, that we always got it right.

### *Cases 2–5: Moral choices in everyday fieldwork*

(2) Thomastown had a business enterprise which had evolved from an artisanal shop in the later nineteenth century into a factory and multiple



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retail outlets after the Second World War. The current owner was a sociable man but, like all of Thomastown's shopkeepers, he 'kept himself to himself' and maintained firm control over the information which circulated about his business. We wanted to talk to him – about the history of his family's enterprise, his entrepreneurial strategies and whether and how he saw himself as a town notable. We already 'knew him' well enough to talk about the weather and casually gossip. Now, we wanted more. Our first few suggestions for a semi-formal meeting were deflected. How far should we pester him?

(3) While interviewing a farmer and his wife one evening, we were going through our usual array of topics – family-farm history, kinship relations and contemporary farming systems – when, arriving at a point in his family tree which required him to explain what had happened to his father's sister, he suddenly said: 'Don't write this down!' We ostentatiously put down our pens and he proceeded to explain how she had married a labourer 50 years before and had emigrated, never to be heard from since. Should we write down this information after we left him?

(4) While talking to another farmer and his family, our genealogical records indicated that his father's sister had had an illegitimate child 30 years before. Given the stigma which surrounded such births at the time, we decided that we would leave her out of the list of kin about whom we asked. As soon as we had finished our list, the farmer looked at us and said: 'You left out my aunt Mary. She lives in Waterford and has a shop.' We apologised for the oversight under the unblinking gaze of the entire family.

(5) One of the archives in which we worked was the Deeds Registry in Dublin. Minutes of all registered property conveyances, from 1830 on, are to be found there. Our use of this source became known after we published our first book (Silverman and Gulliver 1986). On a subsequent visit, we were approached by a middle-aged Thomastown woman who asked if we had found a record of her cottage being registered. We asked why. The answer was quickly forthcoming. She had lived with and taken care of her widowed mother for many years; her brother had emigrated 30 years before, leaving the two women to cope as best they could. Her mother had died recently without leaving a will. Her brother had now returned and claimed half the cottage. She could not afford a lawyer to check in the Deeds Registry but she needed to know if her mother had ever transferred the cottage to her, as she had once promised to do. Should we give her access to our records?

In effacing aunt Mary from the records according to one moral code (stigma), we had offended another value (family privacy). In continuing to pursue the businessman and finally catching him, we obtained some superb insights into Thomastown's commercial sector and into the transformation of an artisanal enterprise. In later writing down what we had been requested not



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to record (but red-circling it to remember for ever how we came by the information), we were able to fill in our own understanding of status–class endogamy in the locality. And by providing a woman with information which lay clearly in the public domain but which required an outlay of cash, we made the apparatus of the state less inaccessible.

All this, however, should not be seen as a recipe for contemporary research in Ireland or even in Thomastown today. Moralities, values and codes change continually; and they certainly have done so during the two decades during which we have been doing research in and about the locality. For example, the town's businessmen have become increasingly unapproachable, inaccessible and uninterested as, more and more, they have come from well beyond the locality and region. Status–class endogamy has been reproduced but, in so doing, the categories have been transformed as have the boundaries, beliefs and interactions which are maintained through them. Illegitimacy and a history of tuberculosis no longer deface a family's reputation. The Deeds Registry is now open to anyone who wants information about his/her own property.

These changing materialities and moralities mean that anthropologists who have been there for over two decades are still having to negotiate their ethics, every day.

## Informed verbal consent: The ethics of (re)presentation

Researching among a highly literate, English-speaking population seemingly resolves one ethical issue by removing the boundary between writer (anthropologist) and reader (local people). Yet, this erasure – and the fact that Thomastown people can read anything that we write – brings to the fore, more than ever, the moral problems which surround presentation and representation, accessibility and inscription.

During our first and most intensive periods of fieldwork (sabbatical 1980–1; summer 1983), we had commonly told people that we were writing a book about Thomastown. Since the time-consuming work of writing is largely invisible and unknown to most non-writers, we were always being asked: 'When is the book coming out?'. In 1983, we had to decide whether to analyse our materials and write them up for an academic audience or to give precedence to the local one. Which audience had the stronger claim?

Our premise at the time was that we had two distinct audiences. We knew that what most local people hoped to read about was of little interest to anthropologists, and vice versa. Thomastown residents wanted to see the names of their parents and grandparents inscribed in a book, along with any (neutral) details (e.g. occupations, offices, accolades) that we had uncovered. They also wanted commonly known stories about local people wrought in ways that would capture their public personalities and quirks. What they did not want was academic jargon and anthropological theory. Nor did they want



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unbecoming details publicly inscribed. That Joe Reddy was 'a man of his times' – humorous and a story teller – should be recorded, but not the fact that, as everyone knew, it was his heavy drinking that underwrote his leisure time, humour and persona. That the 'shopkeeper Doyles were so mean that they'd chase a crow for a crust of bread' was equally well-known but not for publication. Seen analytically, what Thomastown people wanted were primary data – facts – about respectable people and respectable facts about all people. Thus, even before it was written, our book had been appropriated by many locals as part of their own public representation of their selves.

We eventually decided to write our first book primarily for a Thomastown audience, largely to fulfil our promise as quickly as possible and to return what had been given to us. It also seemed a good way of easing ourselves into our data.[8] In making this choice, we also had to make two compromises. First, the book (1986) had little that was theoretically explicit and, therefore, little that would interest anthropologists outside Ireland.[9] Second, to avoid offending anyone, we wrote largely about the more distant past, barely venturing beyond the Second World War. What did we gain? A lot of goodwill and legitimacy. But we were also academics, concerned with anthropological theory and debate. We thus published other pieces (Gulliver 1989, 1992; Silverman 1989b, 1993, 1995, 2000; Silverman and Gulliver 1996, 1997) in locations not easily knowable or available to Thomastown people, except for a few close friends to whom we gave copies of some of our work. So far, so good: two audiences (local vs. anthropological), two genres (detailed empiricism vs. analytical ethnography), and two locations (local vs. limited availability).

However, a local history and articles are, relatively speaking, small pieces of work. What were we to do when we wrote lengthy, labour-intensive, analytical anthropological books? Could or should Thomastown people be excluded from accessing such a major endeavour about themselves because of its academic language, limited availability or content? Clearly not. What did this mean? First, we thought about the common tactic of changing all names; but we knew that local people would quickly de-code this. In any case, they wanted to know about people, both past and present. Second, we thought that perhaps we could be cautious in what we included and avoid certain topics. But could we really? How does one write about inheritance patterns without mentioning disputes? The code of respectability without stigma (illegitimacy, alcoholism, disease)? Capitalist enterprises without exploitation? Class relations without antagonism? Gender without violence? Social relations without ostracism? Religious belief without hegemony? Third, how could we theorise about, for example, the petty bourgeoisie, class and world systems (Gulliver and Silverman 1995), and about hegemony and power (Silverman 2001a), without alienating local readers? Finally, perhaps if we stuck to the more distant past, potentially unpalatable names and events could be avoided? Yet, the whole point of writing in the genre of historical



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anthropology was to explain how the past led into the present (Silverman and Gulliver 1992a and 1992b). Thus, as we contemplated writing academic books, the dilemmas inherent in presentation and representation emerged dramatically, especially as we also tried to factor out paternalistic, over-protective sentiments from a genuine fear of doing harm or of breaching the tenets of confidentiality which, we believed, covered data obtained from historical sources as well as from participant observation and interviews.[10]

Our solutions were as follows. We decided that all topics and theories which had anthropological relevance should be included. However, we also began to develop a 'textual strategy', initially in *Merchants and Shopkeepers* (1995) and then in *An Irish Working Class* (2001a). This strategy allowed parallel readings of the text. Taking the 2001 volume as an example, I put all primary data (extracts of documents, anecdotes, and so on), unanalysed, inside demarcated 'cases'.[11] Enough to constitute a local history on their own, the 167 cases in the 2001 volume can be, and are being, read as such by Thomastown people.[12] We also decided to use real names unless the data or the tales were compromising. What, though, was compromising? Some decisions were clear: the general lack of charitable impulse among shopkeepers or the stigma of alcoholism could not be illustrated using the name of Doyle or Reddy. However, most cases were not as clear; and we found no simple rules. We came, frankly, to depend on our instincts which, in turn, were informed by many years of 'being there'. Even so, as the examples in the next set of case studies illustrate, we can never hope to get it right all the time.

### *Cases 6–8: Ethical questions of (re)presentation*

(6) In *An Irish Working Class* (2001a), I traced the history of radical politics among workers during the 1930s and 1940s. Among the activists were several who were communists. At the time, the stigmatisation was intense. The adults were ostracised and threatened, their children were taunted, the families were denounced from the pulpit. In the book, I changed the names of the families. I knew that elderly local people would know who they were but I thought that young people and, especially, outsiders should be kept in ignorance. The response: members of the present generation of the families were divided. Those who saw the past as heroic wanted their parents named; those who saw the past as compromising current respectability did not. Who should decide?

(7) In *Merchants and Shopkeepers* (1995), we presented, in Chapter 12, detailed descriptions of three shop premises in the town in order to explore the dynamics of retailing through the nineteenth and twentieth centuries. Our data came both from archival sources and from interviews. Together, the cases touched on marriages of convenience, elopements, disappearing dowries,



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familial disputes, bankruptcies, alcoholism, improvidence, religious conversion (Catholic to Protestant), marital squabbles, stepfather–stepson altercations. Such features could not be censored, for they, among others, were crucial to patterns of enterprise continuity (inheritance)–discontinuity (sale). The names of families could not be changed; everyone knew who they were. Was there any room for ethical choice here?[13]

(8) Since publishing *In the Valley of the Nore* (1986), we have felt morally obliged to continue writing articles for local and regional historical journals (Gulliver and Silverman 1993; Silverman 1989a, 1990, 1991, 1992a and 1992b, 1994, 1998, 2001b). Thomastown people, such as students writing papers, shopkeepers preparing a brochure, local journalists seeking background information, have mined these works for facts about the locality. So, too, have local historians who have then published our material, often verbatim, in their articles. Our publications are never cited as the sources in these cases. In other words, our work has been appropriated. Should we say something?

## Conclusions

When teaching students, whether graduate or undergraduate, over the past 30 years, I have always been struck by the fact that they find the subject of ethics so fascinating. Nothing will get seminar discussion moving more quickly, and encourage more participation, than putting forward a so-called ethical dilemma, such as the ones in Cases 1 to 8 above. I have long pondered this. Is it because anthropology students are particularly sensitised? Is it because moral choices are intellectually or emotionally jarring? What is clear to me, however, is that the kind of auditing which is being done by institutions such as York University has little to do with anthropological practice.

In this article, I have tried to illustrate the profound differences between institutional audits and discourse, phrased at York as 'informed consent', and the experiences of being in the field and of writing up. The former is about rules, standards and formalities which will neatly fit all research projects and keep liability at bay. Yet even when approached through anthropological discourse, such as mutuality, rapport, trust, respect, etiquette and rights, the audit fails to come near my own experiences of everyday moral choices which underlie my anthropological practice. Nor are ethics in anthropology about the stark choices which must be made at dramatic moments and which are beloved of students: do you tell the police if you know that murder will be committed that night?

Instead, everyday ethics is about crafting a persona and identity that will mutually engage both the researcher and the people, without doing damage to either. Then, it is about the continual need for choices, each day. It is about



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ambiguity, conflicting interests, fine lines, judgement calls and, therefore, about awkward decisions. This means that every research site is different, as is the personal style which every anthropologist brings to the field.

I do not believe that the new culture of accountability or the recent reflexive and post-colonial turns have altered ethical anthropological practice in any real way; at least they haven't done so for me. I cannot recall behaving differently in my earlier field projects in rural Guyana (1969–73) or coastal Ecuador (1978–9). I also do not recall that we, as graduate students in the late 1960s, spoke about ethics in ways that were very different from those spoken of today. What has happened is that the language has changed and some important issues have been lost, such as questions about 'clean' funding for research and how our research helps the material conditions of people among whom we work. Thus, within the strictures carved in stone – 'do no harm and do not cheat' – our discourse spoke of honesty and openness in our explanations of who we were, what we were doing and from where our research funds were coming. We were to 'respect differences' and 'withhold judgements'. This older discourse also included the need to make our 'findings' available to all and, ideally but importantly, to make these findings 'relevant' to informants' lives. We were to be givers as well as takers. Now that we 'produce data' rather than 'collect' them, 'represent' rather than 'present our findings', and 'appropriate' rather than 'give', have our everyday moral lives in the field really changed? Have our choices become simpler?[14]

Like my graduate student who mentioned her abject sense of powerlessness in the field, I too think that the dependence of anthropologists on the people among whom we work needs highlighting here, for this too has moral implications. It is therefore useful, when exploring everyday ethics, also to reflect on how dependence has framed the 20-year trajectory of our work. Succinctly put, our concern not to offend, to do no harm in a field site in which English was the working language, has pushed us towards exploring more of the past and away from documenting the present. Equally, our concern to make our work accessible to a local audience has had a similar impact, given the very clear interests of Thomastown people in reading about their pasts as distinct from their present. However, as I now write up materials for which the above-described research permission was granted in 1995 – to explore socio-economic change in Thomastown since Ireland joined the the European Economic Community in 1973 – I have also decided that I will do this in a way which is relevant for anthropologists rather than for a local audience. To write about the present, I will hide informants and agents, be sparing with the empirical examples, build theory. Having done so much to make our work accessible so much of the time, am I ethically correct in not doing so this time?

For me, anthropology is, every day and with every decision, a 'moral discipline'.



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## Notes

1 Originating in the 1980s, the present manifestation of this sub-committee is now more formal and extensive in its coverage and more coercive in its demands.

2 It also got me thinking about the behaviour of others who had worked in Ireland. For example, I recall being horrified at the disingenuousness of Mart Bax's comments about how he had sometimes used a hidden tape-recorder during interviews and how this impacted on later rapport (Bax 1976: 5). The present chapter is premised, perhaps naively, on the assumption that all anthropologists follow fundamental ethical guidelines which, certainly in my own experience, have been part of graduate education since the late 1960s. These were simple edicts: do no harm and do not cheat. That even these stark rules could raise ethical dilemmas in some volatile or extreme contexts is further proof of the complexity of moral behaviour in the field.

3 I use the term 'Thomastown people' or 'local people' in a colloquial and indeterminate way. Residents of Thomastown – an unbounded area (of town and rural hinterland) – are neither homogeneous nor all acquainted with each other. These qualities of local life are discussed in Gulliver and Silverman 1990.

4 Note that we refrained from defining ourselves as 'anthropologists'. First, we already knew from our readings about the public and political importance of the Irish historiographic tradition and how it had long been disseminated through the educative organs of society (e.g. schools, voluntary associations, church). To be 'historians', to do history, was valued. Second, in addition to the fact that few people actually knew what anthropology was, to be anthropologists at this time in Ireland was a problem. We were there during an altercation in the national press between Scheper-Hughes, as an 'anthropologist', and Michael Viney, a regular *Irish Times* columnist, over whether Scheper-Hughes' (1979) book had 'betrayed' the people in her village because of its focus on schizophrenia and economic decline and because she failed to protect sufficiently the identities and feelings of her informants (Kane 1979; Viney 1980; Scheper-Hughes 1981; Komito 1982). The debate entered the anthropological record through RAIN (Kane 1982; Scheper-Hughes 1982; Nixon 1983). Because we were doing political economy – that is, how the past informs the present – we felt justified in speaking of ourselves as 'doing history and contemporary description'. The debate around anthropology in Ireland continued for some time and came to incorporate Messenger's ethnography (1969) as well as that of Viney (1983). This altercation and debate became another strand of experience propelling us to privilege the past in our research. However, we had decided, long before arriving in Ireland, that we did not want to study 'the West'. For a discussion of this see Silverman and Gulliver 1992b.

5 A lack of interest in, or a denial of, a past and a history is, of course, a key feature in defining an 'other'.

6 I think that it was the combined identities of 'blow-in' and 'other' which worked so well for us. To some extent, Philip could overcome the blemish of his Englishness in an Irish context by his association with me; I, in turn, could be rendered more familiar through my marriage to Philip.

7 As we described in Gulliver and Silverman (1990), items of gossip are neither universally nor equally distributed in all segments of local networks. Instead, knowledge depends on physical proximity, class, occupation, gender, age and so on. This is why gossip – defined here as the transmission of information, whether known or putative – is so important. It is a scarce, and unevenly distributed, commodity.

8 By 1983, we had accumulated huge amounts of data of various kinds. The problems of sorting and accessing them are discussed in Gulliver 1989.

9 In Silverman and Gulliver 1992b, we described our theoretical journey in relation to our Thomastown research. We showed how, in our 1986 local history book, we had altered the dominant Irish historiographic agenda by exploring topics, events and categories of people which had been ignored. We did so without explicitly noting this. Thus, the reviews of



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the book by academics tended to laud its value as 'people's history at its most refreshing and humane' (Kennedy 1987) but, quite rightly, criticised its 'lack of theoretical overview or general interpretation' (Donnan 1987). Within the politics of the academy, and its modes of assessing scholarship, such reviews are construed negatively. This raises questions about the ethical choices which we make as academics when we evaluate the outcome of our ethical choices.

10 We had, very early on, dispensed with the idea of using a pseudonym for Thomastown itself. Some anthropologists in Ireland had done this. The result was that, years later, we were unable to bring their data up to date, or even to use their data, for comparative purposes (see Gulliver 1992: 193–6). In an anthropological world where paradigms and theories change rapidly, old ethnographies become more useful for their data than for their theoretical ruminations. To obscure those data by hiding the regional and national location of a locale seems foolish. In any case, Thomastown people were proud to have a book about themselves; they wanted 'to put the town on the map'.

11 Cases 2, 3 and 4 in this chapter illustrate this textual method.

12 This is different from the use of 'apt illustration' and the 'case method' which was common in the 1960s. Although the outcome may seem similar, the motives are very different. This textual strategy does result in long books, however: *An Irish Working Class* (2001a) is 566 pages in length. For many, then, this ethical solution is obviated by the economics of publishing.

13 As a final comment here: no one in Thomastown that we know of has objected to any of this material.

14 I am not discussing here the new and often exciting topical or analytical ideas which have emerged from postmodern and post-colonial studies. I am querying how we behave, ethically, in everyday life in relation to the people among whom we work.

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