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Ethics and ethnography

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The discussion continues from a paper by Robert Dingwall in this journal on the ethical regulation of social research. This paper focuses exclusively on the conduct of ethnographic research. Here it is argued that current practice of ethical review is itself sociologically and anthropologically impoverished. Ethnographic field research does not merely pose practical problems in relation to anticipatory regulation, but also exposes the inadequate understanding of social life embedded in the assumptions of contemporary regulatory regimes.

Introduction

Robert Dingwall (2008) in this journal is one of several authors to draw attention to the current problems surrounding the regulation of social research through the imposition of ‘ethical’ guidelines and the work of ethics committees. In this contribution to that debate, I intend to develop Dingwall’s discussion with a particular focus on the conduct of ethnographic research by sociologists, anthropologists and others. My aim is to display the impoverished view of field research that is enshrined in current regulatory practice. I argue that one of the greatest problems facing social scientists is the social-scientific ignorance that pervades much of the current ethical regulation of research. I suggest that ethical regulation will not find trust and respect among practising social scientists while it is itself (mis)informed by a poor grasp of social trust and respect; as a consequence, it is—if not reformed—more likely to encourage deviant behaviour on the part of social scientists themselves rather than promote ethically sound action. This is but one aspect of current debates and statements concerning the ethics of field research: for other contributions, see Murphy & Dingwall (2001), Haggerty (2004), Ryen (2004) and Hammersley & Atkinson (2007).

The relationship between ethnography, qualitative research more widely, and ethnography more narrowly, is not a new topic. Indeed, there have been numerous commentaries on the conduct of research and the contemporary practice of ethical oversight (cf. Denzin & Giardina, 2007), and these debates are in turn embedded

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in much wider frameworks of political dispute, feminist debate and postcolonial discourse; in some quarters, qualitative research is explicitly directed towards resistance against American neo-conservatism (e.g. Denzin & Giardina, 2008). These are all contentious issues that impinge on the politics and ethics of ethnographic research, but in this paper I focus on a more restricted range of issues that remain pressing for practical researchers and that demand collective responses on their part.

The anthropological failure of ethics regulation

Ethical regulation has been largely inspired by biomedical research. This point has been made sufficiently often that it does not in itself require further elaboration. The models that are implicitly developed in the social sciences are fundamentally based on those that have been developed for the management of medical research activities, such as clinical trials. Here the primary, though by no means sole, interest has been on the informed consent of individuals who agree to take part in a trial. The significance of informed consent was enshrined in post-Nuremberg codes of ethics from their inception. In one sense, it is hard to quarrel with the general principle. Clearly, one would not want human subjects to participate in experiments or trials unwillingly. Coercion would seem wrong and contrary to our most cherished beliefs. Likewise, it seems incontrovertible that participants should not merely consent, but should do so with knowledge and understanding of what risks might be involved. There is, therefore, a form of contract between the researcher(s) and the participants that establishes an appropriate balance of rights and obligations, freely entered into, and fully understood on both sides.

The biomedical model has been widened to apply to many other forms of research involving human subjects. Given their nature, the social sciences are involved in ethical review and approval more than any other field of research outside of biomedicine. Yet the models and their implicit assumptions about the nature of research are themselves sociologically or anthropologically deficient, and they rarely apply in any satisfactory way to the conduct of ethnographic research. This is not merely a technical issue of research design, nor is it simply a narrowly sectarian issue, based on differences in disciplinary cultures.

Let us pause and consider things a little more concretely, however. A cursory inspection of most ethics committees' protocols will demonstrate how problematic they can be. It is common for applicants for ethical approval to have to answer a checklist of closed questions, and to amplify on any of the answers if any of them is the dispreferred response (usually 'no'). Most ethnographers will, however, know that the answers they give will be at best half-truths, and that they are often at risk of misrepresentation (at least, in the eyes of an unsympathetic and literal-minded investigator). Consider, for instance, the seemingly innocuous question 'Will participants be informed of their right to withdraw from the research at any stage?', some version of which is enshrined in most protocols. At first sight, it seems impossible to quibble with the basic right of a research participant to withdraw, and to do so at any stage of the research process. Her or his rights to do so would seem self-evidently

to override the interests and convenience of the researcher. From the ethnographer's point of the view, however, such an issue is far more complex. It goes back one stage to the nature of 'participation' and 'participants'. For the right to 'withdraw' from a research project is predicated on the assumption that one 'participates' on an individualistic basis, and that any participant is, in principle, equivalent to any other. But the reality of anthropological or sociological fieldwork suggests that a quite different form of social contract must underpin it. In simple terms, an individual cannot withdraw from an ethnographic project, if he or she is a member of a collectivity, without in effect vetoing the participation of all others who are willing, even enthusiastic, research hosts. The ethnography of, say, a research laboratory cannot proceed if one scientist 'withdraws' completely and denies the anthropologist the opportunity to be present in the laboratory, to observe research group meetings and so on. He or she can, of course, decline to be interviewed or otherwise by involved on an individual basis. The difference between the individual interview and membership of the research group is precisely the crux that renders most ethical protocols anthropologically naïve at best. My own ethnography of the Welsh National Opera Company (Atkinson, 2006) could have been rendered quite impossible had one member of the company 'withdrawn' her or his 'participation', while all had the opportunity to talk to me or not on a purely voluntary basis.

Let me develop this point further. As we know, the great majority of ethnographic research projects depend on the successful negotiation and maintenance of *access*. The term 'access' covers a number of embedded issues. It means far more than simply physical access to a given research site. It means that social actors grant the researcher access to their everyday lives: they grant licence to witness, participate in and converse about issues that might otherwise reach a more restricted social circle. It means having privileged access to the everyday activities of organisations, associations and networks that are based on some sense of *membership*. They do not have to be especially secret or esoteric; merely they have to be settings or groupings that are not perfectly open and public. (I recognise that this glosses over the fact that many 'public' settings are by no means unrestricted, as many photographers, skateboarders and other potential users of space know.)

One need only be a lowly employee, for instance, to have 'access' to aspects of an organisation that are not directly open to inspection. The frontstage regions of a department store, for example, are available to any person who chooses to enter the doors, while the backstage regions (offices, canteens, stock rooms) are not. The activities of an evening class are open to anyone who chooses to register and pay a registration fee. Those who have not done so do not normally have the right to wander in off the street and observe the participants. Note, however, that the idea of *membership* in this context is very different from the idea of *participation* that is enshrined in most codes of ethics. The reason for this is simple: as I have already noted, most of them are based on biomedical models. And most of those are based on individualistic modes of enrolment. The individual patient agrees to be allocated randomly to the experimental or control arm of a clinical trial. The paid volunteer enters upon a drugs trial on her or his own account. Each participant is equivalent to any other.

If there is randomisation involved, each has an equal chance of allocation to one or other arm of a study. In the analysis of the data, each participant is treated separately as a data point.

Since each participant is a separate monad within standard biomedical research, it makes perfect sense to treat her or him on this individualistic basis. Individual actors 'enter' or 'enrol' on the research, and they can 'withdraw' on an equally individual, voluntaristic basis. Thought of in this light, the question from the hypothetical ethics committee protocol I used as a starting point makes perfect sense. But why does it not make perfect sense for the ethnographer? For the reasons we have already alluded to. We normally do not enrol a series of individual participants for our research. We are normally dealing with social actors because they are *members* of an organisation, or are *privy* to some activity that we wish to study.

Obviously, our access negotiations involve ethical issues. Discussions of access have for a long time dealt with the kinds of bargains that researchers enter into with their host. But again it needs to be understood that these are largely about the collective rights and interests of the members of the social setting. We guarantee individual anonymity, certainly. But we also give undertakings not to harm the organisation, or indeed to divulge its identity in some (not all) cases. But access must imply a commitment that is more than merely individual. In concrete terms, how can it make sense for the would-be ethnographer, who has carefully negotiated access to, say, a research laboratory, with all the reasonable undertakings and assurances that might be expected, to say that the research can be brought to a complete halt if one individual person seeks to withdraw? For that is what it would probably mean. It is not possible to study an organisation on the basis that one or more individuals has withdrawn consent. Can it make sense for one school pupil to change her or his mind, and so exclude the researcher from observing all the other pupils in a class? Does it make sense for the researcher studying the laboratory to agree to abort the study if one laboratory technician decides to withdraw goodwill? In some contexts, of course, one disaffected or unwilling member can render the research impossible, but it is not clear that the regulation of research makes sense if the assumption is built in from the outset that this is part-and-parcel of the research design.

As may be envisaged, the ethnographer—confronted with the ethics committee's questionnaire with its deceptively simple item—needs to answer 'Yes, but ...', or 'No, but ...' and expand on the responses at considerable length. Now this is not simply a matter of filling forms, for what is illustrated in a concrete fashion is the profoundly mistaken view of social research enshrined in such protocols. Let us, for instance, consider further the elementary and foundational issue of 'informed consent'. It seems like the sort of ethical principle that we would all want to subscribe to. But in reality it is far from clear what informed consent actually means in most research, and certainly far from clear what it can mean for the conduct of ethnographic research. We need the sort of analysis of ethics protocols that Cicourel (1964) famously performed for the survey interview. In the context of real-world research, all such questions require considerable interpretative work to render them sensible. But it is precisely that background understanding that most ethics protocols

transform into a 'checklist' form of anticipatory audit, and are therefore hard, if not impossible, to answer in that form and in good faith.

One may, after all, consent to take part in a research project in the conventional way. It is not clear, however, how much beyond data collection most informed consent procedures go in practice, or can go in principle. I may agree to be interviewed about my consumption preferences. I may give consent on the basis that my personal information will not be divulged. But to what extent do I give consent for my data to be pooled with that derived from others, and then subjected to statistical manipulation? Should my consent be sought to manipulate the data in accordance with basic demographic, face-sheet data? Should I give my consent to have my personal information aggregated and cross-tabulated in order to generate, say, gender differences, or ethnic differences in consumption? Should I be asked if I consent to having the information transformed into ideal–typical models of taste and *habitus*, in the style of Pierre Bourdieu? The answer to these questions is that most researchers would find it bizarre to have to predict every possible analytic *outcome* and every unanticipated *finding* of the analysis. And in reality, most researchers are asked about the collection of data and the protection of individual subjects' identity: they are rarely called upon to anticipate the findings of research. As a consequence, most 'informed consent' is so minimally informed as to be virtually worthless. Most researchers are not expected or required, for instance, to go back to their survey informants to ask their consent to publish the result that many people with their particular characteristic score highly on a scale of authoritarian attitudes (for example). The field researcher's problems are a particular example of a much more general set of problems relating to consent, but they do throw into relief the conceptual poverty of most current regulatory practice. The publication of ethnographic research, on the other hand, raises complex issues of responsibility concerning the textual representation of social worlds.

Trust, values and consent

Ethnographers would find it especially difficult to establish the boundaries of informed consent in any case. This case has been argued persuasively by a number of authors, including Murphy & Dingwall (2007). This is not because we wish, in most cases, to engage in covert research, but because the nature of the research itself is so profoundly an emergent property of the processes of data collection and research design, that are themselves emergent, unfolding processes, that it becomes all but impossible to solicit consent to the research that is 'informed' in the sense of being predictable and explicable before the research itself is carried out at all. If the outcomes of an ethnography were entirely predictable, then there would be virtually no point in conducting the research at all. It is, after all, possible to discover issues that are critical of the institution or association studied, quite unpredictably, that cannot be incorporated into undertakings before the event. It may, for instance, be a research outcome that educational institutions have practices that have deleterious consequences for students, based on gender, ethnicity or social class. One may document similar sources of bias among the police or other agencies of social control. A clinic

may implicitly ration health care on the basis of social characteristics. It is hard to guarantee—for the purposes of ‘informed consent’—that nothing will be discovered to the disadvantage of the institution, even if individual members’ identities are not to be divulged. Admittedly, we often have to persuade potential research hosts that we are *not* in search of the discreditable or the scurrilous, and in most cases we are more interested in the humdrum routines of everyday life than most people give us credit for. But however much we may stress that for ethical and analytic purposes, it is still hard to ensure that no possible criticism of an institution or association will be implied by our findings. As Murphy & Dingwall (2007) point out, the iterative nature of ethnographic inquiry means that access is always tentative and conditional, that ‘consent’ is always relational and sequential, rather than based on a one-off contractual agreement, and that ethnographic researchers will never find it possible to specify at the outset all that her or his research will involve. Covert research can, of course, uncover phenomena that would otherwise remain inscrutable (for an example documenting sexism in a police academy, see Prokos & Padavic, 2002), but my argument is not about that: the problem is, rather, the anthropological impossibility of ‘informed consent’ in any meaningful way.

A good example of unanticipated research, that incidentally reflects back on the status of the clinical trial as the ‘gold standard’, is furnished from my own research on haematologists (Atkinson, 1995). I attended many clinical conferences and meetings at which cases were ‘presented’. My main interest was in the rhetoric and performance of such clinical work. But in the course of those case conferences, it became apparent that the precise diagnosis and classification of some patients was being contested by professional colleagues. The description and staging of some tumours by at least one clinician were being queried. The strong suspicion was that these particular patients, who were being enrolled into a multi-site clinical trial, were being described in such a way as to make them conform to the particular clinical requirements of that trial. This was not a matter of wholesale professional misconduct, but at the margins, where diagnostic criteria were not clear-cut, the patients were being included in the trial in accordance with the clinician’s interests, rather than in terms of the collegial consensus being expressed by clinical pathologists and other specialists. Now this was not a central part of the research, and the detail of that part of the ethnography has never been published; there is no pressing reason to do so. But it illustrates quite vividly the sort of ethical dilemmas that can arise in the course of conducting and analysing ethnographic work. Incidentally, of course, it illustrates some of the problematic issues of the clinical trial itself as the model of research. The diagnostic criteria for inclusion or exclusion depend upon the practical judgements of medical practitioners, and while the allocation of patients to one or other arm of the trial may be based on chance, and be double-blind, the diagnostic work that determines their eligibility for participation is not and cannot be.

I have illustrated just some ways in which features of contemporary ethics discourse and practice are poorly suited to the ethnographic enterprise. This is not because I am proposing a form of ethnographic work that is covert, or unethical. Rather, the individualistic assumptions built into contemporary practice, and derived from unsuitable

models, do not fit the purposes and the practical conduct of most ethnographic research. This is, incidentally, one way in which the conduct of ethnographic fieldwork differs from much of what is currently done under the rubric of 'qualitative research'. Much of the latter is in fact based on interviewing series of individual informants (cf. Atkinson & Silverman, 1997). While there remain significant ethical issues about the proper representation of such informants' interview talk, the general models of informed consent approximate much more closely when one is conducting one-off informant interviews. Genuine *field* research, on the other hand, escapes the assumptions that inform current regulatory practice.

It is instructive to reflect on the extent to which social scientists are hindering their own research, and the existence of double standards in public life. The world is full of intellectual and popular work that would never pass muster with the average research ethics committee, but it is not governed by such committees, nor does anybody seem to call its practitioners to account. There have been several recent publications based on various forms of deceptive, surreptitious and covert research. In one case, the act of deception was absolutely central to the theme of the book. In her first book, *Self-Made Man* (2006), Norah Vincent describes a protracted period in which she dressed as male, made herself up top look male (including the creation of reasonably convincing stubble) and learned to act as a male in contemporary America. But the acquisition of personal experience was completely unethical, judged by the standards of academic research. Nobody seems to have remarked on the ethical aspects of the exercise, which was, by design, based on deception. Popular works of this sort can be justified in terms of the extent to which the author disguises individual identities, and muddies the water by fictionalising individuals and reported events. This is, quite explicitly, the approach taken by Julian Baggini (2007), who set up house in the English town of Rotherham, in order to discover at first hand, the 'facts' of everyday life in the most representative postcode neighbourhood in England. He provides a sort of classic 'community study' account. He acknowledges, however, that his acquaintances had not given permission to be quoted or identified, and so he mixes identities, and creates composite characters. The crucial point, of course, is that no social scientist would be allowed to behave ethically *post-hoc* in the absence of explicit informed consent. Popular works escape such constraints.

There is a long, and perfectly honourable tradition of exploratory and investigative work by journalists and other authors. It overlaps with a similar genre of autobiographical writing in which the author embarks on a personal quest or adventure, writes about her or his experiences, and in the process writes about a variety of other people as well as providing graphic descriptions of distinctive social settings and locales. Recent examples include an account of a young English man learning the martial art *aikido* in Japan (Twigger, 1999), an unmusical Englishman following up the quixotic decision to play Cuban music in Cuba and to find *la bomba*, the spirit of Cuban soul (Neill, 2005), a man's quest for authentic flamenco guitar and its spirit of *duende* (Webster, 2004), and the quest for expertise in blues guitar (Hodgkinson, 2006), and other quests to engage with martial arts (Preston, 2007; Polly, 2007). Clearly these and similar works constitute a genre in their own right, and are

predicated on the twin themes of personal discovery and the search for authentic experience. But they have the common property of being silent as to whether the many people who have taught and helped the author granted explicit permission for the author's activities to be turned into a book.

At the other end of the spectrum from the professional social scientist is the work of the author of fiction or the professional actor. Sebastian Faulks researched a novel (*Human Traces*) by interviewing psychiatrists and patients in order to inform his account of mental illness. One of his key informants is acknowledged by name; a mental health media volunteer, she no doubt helped the novelist in the full knowledge of what she was doing, but social scientists would normally be reluctant to name their gatekeepers, sponsors or informants. Siri Hustvedt taught as a volunteer in the Payne Whitney Psychiatric Clinic in order to research one of her novels, *The Sorrows of an American*. Now I do not mention these examples in order to say 'Look at what journalists and fiction authors get away with' in order to justify a free for all. But these examples—that could be repeated many times over—help to illustrate a number of things. First, social scientists are in danger of handicapping themselves, for no very good reason, and leaving it to others who are not so obsessively regulated to produce the equivalent of ethnographic insight. Second, journalists, actors and other investigators could reasonably ask 'What harm has been done to anybody?' Indeed, one can point to the value of researching books and articles, in order to ensure their accuracy, and propose that—in the absence of any harm—there is every justification for doing such work. Once you designate something as professional research, however, you are immediately drawn into the nexus of ethics committees and similar institutional bodies.

It is puzzling that social scientists have found themselves on the back foot in recent years. It is doubly ironic that ethnographers should sometimes find themselves especially wrong-footed by ethics committees and similar review boards. There is singularly little evidence to be found that the conduct of ethnographic research has actually led to any harm whatsoever. Even field research that might be viewed as contravening current standards of ethical approval rarely, if ever, seems to have been *harmful*. On the other hand, the technicalities of medical ethics committees have *not* prevented actual (and in some cases, grievous) harm to trial participants.

My examples from the world of literature and journalism also help to remind us of the sheer complexity of the social world and the multiplicity of ways in which we can engage with it and document the social world. We are in danger of allowing the quite proper concerns for research ethics in general to transform the entire research process into a formulaic one, such that there are only a very limited number of permissible research designs, determined not by their general epistemology, nor by their validity, but by their capacity to yield simple research protocols that can be checked against a set of simple (but often inappropriate) criteria. Anticipatory audit is the tail that wags the research dog.

So far I have not even considered the very limited view of 'ethics' that is enshrined in the procedures of committees. As we have seen, they seek to enforce a model based on individual participants, and are concerned primarily about their informed consent.

There is singularly little attention granted in the written codes and guidelines about other—arguably more important—issues. The following are some issues that might well be thought to be significant: giving adequate voice to muted, marginal or subaltern social categories; ensuring that the rhetoric of publications does justice to the reported actions and actors; that research promotes the interests of social justice. There are many such ‘ethics’ that can be derived from critical, feminist, post-colonialist and other standpoints. But ethics regulation is all too often predicated on a model of positive science, with no regard for more general cultural and political considerations.

In contrast, the conduct of ethnographic research has been predicated on a set of commitments and values that arguably render it much more sensitive to the interests of ‘participants’, and make the personal values of the researcher(s) more central than most other forms of research. Ethnographic research calls for a greater personal commitment to the field and its members than virtually any other mode of research. Ethnographers spend months and years of their lives working closely with social actors as they go about their daily lives. There is a commitment to engage with forms of social life that goes beyond virtually any other research strategy. Indeed, it is noticeable in the examples cited above that when authors such as novelists want to explore the authenticity of a given cultural form, they find themselves engaging in participant observation, even though they do not grace it with the term.

In the ideal world, the ethics of social research would be predicated on a different set of approaches. It is worth reminding ourselves that the word *protocol* can have different connotations. In the sense most used in today’s research communities, it means a prescriptive set of injunctions and prohibitions that regulates research. It captures the sense in which research and its proper management have been treated in *procedural* terms, reducible to checklists and formulae. On the other hand, the term ‘protocol’ can also refer to proper conduct. And we ought to think of research conduct in this more general, and indeed more social, sense. We need to work to refine the collective sense of research protocols in terms that are driven by *values* rather than by *procedures*. For instance, many ethnographers spend a good deal of time developing trust with their hosts and informants. The promotion and development of such a positive interpersonal working relationship might provide a more anthropologically and sociologically informed basis for proper conduct than the jejune notion of informed consent. Likewise, the establishment of social relationships in the field should be recognised for what it is—a *process* rather than an event that can be predetermined and inscribed within a single document.

The extent of indeterminacy and unpredictability in field ought to be appreciated in the course of research design, and hence in the process of ethical approval. This is not tantamount to *carte blanche* based on a claim that nothing can be foreseen. But if research is guided by values and general principles, and their general application outlined, rather than highly specific and prescriptive checklists, then research can be carried out humanely, sensibly and in accordance with positive values. The big problem that arises in contemporary practice reflects the fact that it is not congruent with general sociological or anthropological imagination. Because of its individualistic

emphases on informed consent, it does not map well onto the realities of ethnographic research, as we have seen. As a consequence, ethical regulation by contemporary ethics committees can have very undesirable unintended consequences. It can force scholars who have a very thorough commitment to working well with their research hosts into a form of deviance. Because ethics protocols are sometimes half-baked, they force the researchers into half truths. It is clearly undesirable if ethical issues in general are perceived as something to be worked around, rather than providing a positive framework for practical research conduct. Equally, requirements for formal documentation of informed consent can radically transform emergent (or even established) social relations in the field, by imposing an inappropriate degree of formality on otherwise informal relations that are embedded in the ordinary give-and-take of social life. It transforms the pre-contractual and mutual nature of everyday life into the contractual obligations of individual self-interest and protection.

Discussion

While it is desirable for institutional ethics committees to implement procedures that are sensible and sociologically or anthropologically sophisticated, the reality is that not all do so. There is, therefore, a process of ethnographic education to be engaged in, through which the institutional guardians of ethical research approval and the various funding bodies need to realise that the models they have adopted wholesale are inadequate. It is especially unfortunate that even bodies that are representative of the social sciences, such as the UK's Economic and Social Research Council (ESRC), have developed ethical guidelines that are inadequate, in that they do not capture the kinds of real-life decisions that ethnographers have to make, and the kinds of relationships they have to foster. Admittedly, the ESRC's own ethics framework (Economic and Social Research Council 2005) is more attuned to field research than the many insensitive impositions of biomedical models. It does not, however, solve things for the purposes of practical social research. The ESRC's own framework provides minimum requirements for Research Council approval (for postgraduate training and funded research projects), and its overriding requirement is that each university must have its own system of ethical regulation. It is at the level of those local bodies and their mechanisms for ethical approval that the would-be ethnographer encounters the individualistic model.

In the shorter term we have to recognise the reality of the institutional world as it is. What can be done to represent our research honestly and faithfully, in a way that captures the positive values and commitments of the ethnographic tradition? In addition to trying to act individually with local institutions, we need to work within professional bodies such as subject associations and national academies to promote better informed approaches. We also need to point out that the 'gold standard' model derived from biomedical science and from the clinical trial is a chimera, and is not supported from research evidence itself (cf. Featherstone & Donovan, 2002; Timmermans & Berg, 2003). Indeed, one of the ethnographic imperatives facing social scientists today is the documentation of the standards, conventions, protocols

and regulations that permeate everyday organisational, financial, professional and scientific social worlds.

At the same time, we need to write research proposals and ethical submissions that are based on positive affirmations of our professional values. We need to appeal to codes of professional ethics that are well informed and relevant to our research practice. The statement of ethics of the Association of Social Anthropologists, for example, is a more useful starting point—because written by anthropologists for anthropologists—than most current ethics committee guidelines. We ought carefully to write ‘protocols’ that outline the proper conduct of our research, how we engage with collectivities as well as individual members, how we safeguard those members’ privacy and interests, as appropriate, and how we undertake legitimate safeguards. We need to ask ethics committees to help us resolve conflicts of interest as and when they arise, rather than be expected to foresee every eventuality before it happens. We need, if at all possible, to persuade our colleagues that if issues like research access are processual and dialogic, then the relationships between researchers and ethics committees need to be the same in spirit and in form.

There are many other ways in which contemporary ethnographic researchers pay heed to a much wider, and often more significant, array of issues than do most ethics committees. It is now recognised that we ought to pay critical, reflexive attention to how we frame and portray social worlds in the texts that we construct. How we ‘write up’ our research is never a purely neutral matter (Atkinson, 1990). It is a matter of analysis. It also implies a number of moral issues. We clearly have responsibility to those we portray. Notwithstanding the preservation of individual autonomy, the textual representation of social worlds and communities can prove a highly contentious issue, as evidenced by the contributors to the collection edited by Brettell (1993). How we choose to select examples, how we create descriptions and how we reconstruct actors’ intentions or motives all have ethical implications. As we have become more reflexively aware of the role of textual constructions in the analytic and representational processes, so we have become aware of these ethical issues. They escape the procedural approach that is characteristic of today’s research environment yet they are potentially of far greater moment than niceties of the wording of an information sheet or an informed consent form.

The same is true of another major issue that currently escapes the normal scrutiny of ethics protocols: the safety of field researchers themselves. It is clearly incumbent on research directors and managers to ensure that field workers are not exposed to unnecessary dangers in the conduct of field research itself. The avoidance of ‘harm’ to researchers is clearly just as important as the protection of research hosts and participants. Yet any current consideration of such issues—such as guidelines for the safety of lone researchers—as enshrined in standard protocols is overwhelmingly predicated on lone researchers in laboratory, rather than field settings. Yet any attempt to produce sensible guidelines for researchers’ safety and well-being is currently likely to get ensnared in the same trap as research ethics. My own attempts to develop a sensible approach to the safety of research colleagues and doctoral students was rapidly threatened with impossibly elaborate risk assessments of every

potential research site (including interviewees' homes and their neighbourhoods) and criminal record checks on every potential research participant. Just as is the case for virtually all other aspects of current ethical regulation, anticipatory audit and a precautionary approach to researchers' well-being will fail to reflect the realities of normal fieldwork in the social sciences. The issues of researcher safety and well-being have been highlighted by Bloor *et al.* (2007) in a pioneering inquiry. Their report, while highlighting many important issues for research managers, supervisors, sponsors, hosts and employers, also illustrates the potential dangers of yet further anticipatory regulation. They recommend, amongst many other things, health and safety audits of all university departments to include scrutiny of researcher safety, and the inclusion of additional obligations on university ethics committees to have oversight of researcher safety, with new items of application forms for ethical approval. Now, as I have suggested, the overall concern with researcher safety is important, and there is no doubt that it should be among the concerns of research managers and supervisors. But loading additional items onto mechanisms of anticipatory protection and regulation would transform the general values that inform the avoidance of harm into a further set of protocols that hamper research and encourage 'paper' exercises. (This brief discussion does not, I know, do justice to the complexity of the issues discussed by Bloor and colleagues.)

We can no longer appeal to what is reasonable or commonsensical. We cannot point to a general lack of harm, or a general set of values. Everything must be explicated in a series of protocols. Never mind that such protocols do not match the social realities that the researchers themselves want to explore. Never mind that tacitly held assumptions about normal everyday social life and social conduct are fundamental to the fabric of social order. We are required to explicate them. Never mind that social actors are inherently unpredictable: we must second guess their actions and the possible consequences. It is in that sense that the contemporary regulation of social science research is sociologically and anthropologically illiterate. It runs counter to all of the most significant things we know about social actors, social action and social organisation.

Does this mean that there is no place for ethical considerations? By no means, but we must start from our anthropological, sociological understandings of the research process and the social worlds we work with. We need to remind ourselves that a research contract between researcher and hosts inescapably rests on pre-contractual bases of mutual trust. We need to ensure that social research is underpinned by practices and understandings that are in accordance with such research itself, and do not run counter to them. This will not be achieved by trying to fit ethnographical fieldwork to the procrustean bed of current procedure; nor will it be achieved simply by tinkering with current paperwork.

Meanwhile, as social scientists contrive to tie themselves in knots over the ethical approval of their research projects, large areas of everyday life go reported with minimal ethical regulation. In the face of massive failures of data protection by financial institutions, government departments and other public agencies, it is hard to take seriously the need for committees to devote much of their time and delay research

activities by micro-managing the detailed wording of consent forms and assurances of anonymity before a social scientist can interview another social actor who is entirely *compos mentis* and capable of ‘consenting’ to granting an interview. It is, of course, hard to imagine just how one might coerce an informant to grant an ethnographic interview or to narrate a life history lasting several hours if he or she did not ‘consent’ to such an activity.

The burden of my comments is not to advocate wholesale rejection of or resistance to the general principles of ethical oversight of research. To that extent, I diverge somewhat from the thrust of Dingwall’s argument, notwithstanding the fact that he and I agree on many points, and indeed have independently used similar arguments and examples. My argument is this: if the general oversight of research is to be taken seriously, rather than a nuisance that invites rule breaking, then it needs to be grounded in an expert understanding of the complexities of everyday social life and of the contingencies of real-life social research. The imposition of ill-informed frameworks does nothing to enhance the quality of research, the credibility of research, or the uses to which it is put. Indeed, the danger is that the manifest inadequacy of current ethical guidelines will lead social scientists into half-truths, if not outright deception. The translation of ‘ethics’ into box-ticking exercises, based on a threadbare conception of the research process itself, is liable to be self-defeating, by encouraging procedural deviance rather than research informed by the best available values and standards of conduct that are derived from and inform research about everyday social conduct.

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