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1. Introduction

This paper deals with a sensitive and controversial topic – objectivity in ethnographic research. More specifically, I would like to look at how reflexive procedures can increase the objectivity of the knowledge produced by ethnographies. The essay is organized in five parts. I will start by giving a preliminary, summary definition of the two key concepts which are at the centre of my analysis – objectivity and reflexivity¹. I will then give a brief description of the epistemological framework in which my conceptions of objectivity and reflexivity are located. Thirdly, I move on to consider the epistemic status of ethnographic research, and will emphasize that ethnographies are not just “theory-laden”, as many writers have stated, but also “praxis” or “procedure laden”. In other words, I will stress that it is not only theories which are inevitably incorporated in research, influencing what observations can be made; much the same can also be said of the concrete research practices which are supposed to embody the theoretical perspective. Fourthly, I will discuss why it is so useful to employ reflexive practices, and then immediately afterwards will illustrate the ways in which reflexive descriptions can contribute to greater objectivity of ethnographic accounts. In conclusion, I will discuss a number of objections which have been raised against reflexivity.

2. Objectivity and reflexivity: a preliminary definition

The notion of objectivity has been interpreted in various ways, most commonly (and perhaps most obviously) in realist terms, adopting – usually in a tacit way – the correspondence theory of truth. In this view, an account is objective if it offers a true representation of the object it refers to. The notion of objectivity I employ in

¹ A first version of this paper was presented at the 6th ESA Conference, Murcia, Spain, 23-26 September 2003. Research network 16: Qualitative Methods session 3 – Ethnography and video analysis.

my analysis is quite different, in that I assume a concept of “truth” based on inter-subjective agreement, constructed discursively within a forum set up by the scientific community, or rather a specific fraction of the latter. What gives an assertion (or set of assertions) objectivity is not the use of a “right” method (right both in data collection and analysis), nor the “tribunal of experience”, which can decide whether the social world and its representation correspond. Instead, objectivity is conferred by a collective subject – a forum which arrives at a decision, using the tools of argumentation rather than those of demonstration (Perleman and Olbrechts-Tyteca, 1958). This ties down the notion of objectivity to specific coordinates of space and time, and makes the attribution of objectivity status always revocable².

I understand the notion of reflexivity defined by Altheide and Johnson (1994) as an account of the way research was carried out and of the characteristics of the observer. I am therefore discussing reflexivity in a very specific sense – one which is quite different from the way the term is used by Woolgar (1988: 21–4) and by ethnomethodologists (i.e., constitutive reflexivity), and also from the way the practice is employed by post-modernists, which Salzman (2002) dubs “reporting in print researcher’s feelings”.

3. Epistemological framework

The definitions of objectivity and reflexivity I have outlined already give an idea of the epistemological line I take. However, I will elaborate the latter a little. My stance derives from two main sources of inspiration that Derek Phillips profitably sets together in his seminal text, *Wittgenstein and Scientific Knowledge* (1977): the later Wittgenstein, and the theory of non demonstrative discourse developed by Perleman and Olbrechts-Tyteca (1958).

² There is a similarity here with Quine’s notion of science as a continual re-weaving of a network of beliefs.

With their weighty *Traité de l'argumentation. La nouvelle rhétorique*, Chaïm Perleman and Lucie Olbrechts-Tyteca draw attention to (and to a certain extent rehabilitate) a form of reasoning that, at first glance, would seem to have little or nothing to do with scientific thinking. This is “persuasive reasoning”, which, from the first pages of their discussion, Perleman and Olbrechts-Tyteca contrast with the “demonstrative reasoning” typical of the formal disciplines and, by extension, of the natural sciences³. The *Traité* marks the beginning – over and above the designs and expectations of its authors (see Pera 1991: X) – of scientific discourse’s migration away from the domain of demonstration towards that of argumentation, where the Tarskian notion of truth as correspondence gives way to the notion of truth as inter-subjective agreement, an agreement constructed discursively within a specific audience, made up of the (pertinent section) of the scientific community. In such settings, which can include, for instance, the board of a sociological review or a publisher’s scientific committee, net of the distortions that peer review can only partially curb (I am thinking in particular of the reputation and power of authors and reviewers), whether the audience is persuaded hinges on the force of the arguments with which the author defends the “good reasons” supporting the asseribility of his claims. The core of these arguments can be effectively depicted by drawing on the conceptual categories proposed by Stephen Toulmin. Toulmin (1958) distinguishes between six types of statement, identified on the basis of the function they fulfill vis-à-vis the audience. Alongside the Claim (C), whose asseribility is being defended, we thus have Data (D) that present the grounds supporting the conclusions; the Warrant (W) which demonstrates the guarantees that authorize our movement from the data to the conclusions; the Backing (B) that supports, empirically for the most part, the warrant; a Qualifier (Q) which indicates the strength of the empirical support; and a Rebuttal (R) which indi-

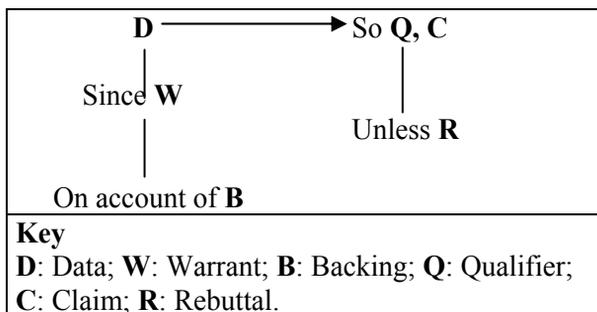
³ Some years later, Jerome Bruner was to return to this approach to shaping the things of the world, distinguishing between narrative thinking and the reasoning *more geometrico* of the natural sciences as two distinct modes of “cognitive functioning” (Bruner 1986).

cates the counter-arguments that challenge the conclusions⁴. Toulmin's model is concisely illustrated in Figure 1 below⁵.

⁴ In the social sciences, and in ethnography in particular, the persuasiveness of an argument also stems from other, as it were, warmer, aspects, such as the esthetic quality of the writing, or the author's ability to inspire emotions in the reader that approximate the experience of "being there". Opinions regarding the legitimacy of these persuasive devices differ, ranging from a stern rejection of the language of the emotions, to its enthusiastic celebration (see Denzin and Giardina 2008: *Introduction*). The radicalism of both these positions can be refuted with reasons that are more solid than those expressed by the adage, "in medio stat virtus". The efficacy of combining the two communicative registers – the analytic and the esthetic-emotional – is sustained by Gregory Bateson, who coined the term "double description" for this style of expression (1979). The two expressive registers, notes Bateson, are complementary, and combining them provides a more accurate portrayal of the object of study, thus offering – I might add – more elements whereby the audience can decide whether it is appropriate. I thank Sergio Manghi for drawing my attention to this important aspect of Bateson's work.

⁵ To illustrate Toulmin's model of argument, I will take an example from a recent study of mine (Cardano, in press). The study addresses the experience of psychic suffering, as exemplified by four "flesh and blood" ideal types. For our purposes, attention here will focus on comparing two cases, Marta and Serena, who faced with the same experience – verbal auditory hallucinations, or hearing voices – adopt radically different coping strategies. Whereas Marta interprets her experience as the outcome of a demonic possession, Serena sees her voices as a gift; where Marta tries to silence the voices, Serena decides to listen to what they say. One of the conclusions I reach from comparing their illness narratives fits Toulmin's model. CLAIM. The consequences of an undoubtedly disturbing event, such as hearing voices, are from many points of view socially-constructed, dependent on the characteristics of the context in which they take shape. The experience of hearing voices may cause people to hesitate, uncertain as to whether they are beginning a "psychiatric career" (with the stigmatising label of schizophrenia), or commencing a new life in which this experience can be seen as a charisma, a divine gift, which opens the way to a new and higher sensitivity. QUALIFIER. The expression "may hesitate" alludes to a tendential relationship (which does not always exist and/or may not exist for all subjects) between the characteristics of the context and the impact that the experience of hearing voices has in the life course. DATA. Large quotations from the illness narratives of Marta and Serena. WARRANT. Methodological guarantees regarding the legitimacy of drawing conclusions about events in life starting from their narrative reconstruction, acquired during an in-depth interview. BACKING. Empirical documentation which makes it possible to assess the match between "life history", or in other words the complete reconstruction of an individual's biography from all available documents, and "life story", i.e., an autobiographical account (see Denzin 1989). REBUTTAL. The claim will not stand if the chain of biographical events following the verbal auditory hallu-

Figure 1. Toulmin’s model of the procedural form of argumentation



What Toulmin’s model effectively breaks down into a set of argumentative functions corresponds to the “dialogue” between empirical documentation and theory that Howard Becker puts at the centre of analysis of social phenomena (see Becker 1998: 66). It is the quality of this dialogue, the solidity of theoretical and methodological “warrants” that authorizes the passage from data to conclusions, which can persuade the audience of the asseribility of claims maintained by the author/observer.

When the “game of science” is set in this frame, the role of the players and their decisions must be redefined. In the neopositivist view (and also beyond its confines, as far as Popper), the “game of science” involves two players and a referee. The researcher and the outside world (the cultures, social organizations, institutions and much more) take the role of players. The referee’s role falls to the scientific community, which is called upon to guarantee methodological orthodoxy and to read, so to speak, the verdict of the “tribunal of experience” which establishes the truth value of each assertion, which proclaims or denies its correspondence with the things of the world and hence its truth (*cf.* Pera 1991: Chapter 5). In the new argumentative frame (with clear conventionalist antecedents), the game involves three players: the researcher, the outside world, and the scientific

cinations – as it emerges from the narrations in question – shows differences in the two cases examined that are entirely independent of the context.

community. The researcher's task is to answer a question about this world, while the latter, or rather, the people who live in it, can choose to establish how much cooperation will be afforded to the scientific undertaking⁶. Persuaded of the accuracy of his answer to the query that sent him into the field, the researcher must then persuade the third player, the scientific community who – in this case – will hand down a less peremptory verdict, offspring of the procedure that generated the persuasive reasoning.

This kind of stance is similar to that taken up by Wittgenstein on the subjects of truth, doubt and uncertainty, for Wittgenstein also seems to see these as tied to agreement.

(...) human agreement decides what is true and what is false. (Wittgenstein 1953, sec. 241)

The potentially relativistic implications of this position are mitigated by comments in *On Certainty*, where Wittgenstein links his conventionalist notion of truth to the social and material context within which agreement takes shape (Wittgenstein talks here of “facts of nature”)⁷. The game of science, like any other language game, rests on conventions; but these are limited by the way in which people and their world are constituted (Phillips 1977). In other words, the statements built in the game of science rest on a set of propositions which Wittgenstein sees as “established” (they “stand fast” for us), or determined by the characteristics of the “form of life” in question. A couple of examples make this position clearer.

⁶ Here, I espouse the thesis of Raymond Boudon, for whom “Contrary to a widely held idea, the aim of science is not to explain the *real* — which, *as such*, is unknowable, or at least can be know only through metaphysical methods — but to answer *questions* about the real” (Boudon 1984). Concerning the cooperation between “us” and “them”, it should be noted that, in the case of ethnographic work, the question is crucial not only on the epistemological plane, but also ethically and politically – though these are aspects I will not deal with here.

⁷ See, for example, Wittgenstein's comments on the conventional character of units of measurement in *Philosophical Investigations*, sec. 142.

I, L.W., believe, am sure, that my friend hasn't sawdust in his body or in his head, even though I have no direct evidence of my sense to the contrary. (*On Certainty* sec. 281).

I believe that I have forebears, and that every human being has them. I believe that there are various cities, and, quite generally, in the main facts of geography and history. I believe that the earth is a body on whose surface we move and that it no more suddenly disappears or the like than any other solid body: this table, this house, this tree, etc. If I wanted to doubt the existence of the earth long before my birth, I should have to doubt all sorts of things that stand fast for me. (*On Certainty* sec. 234).

These propositions are “established” not by the force of reason (still less by virtue of their correspondence to reality), but because they are deliberately removed from critical analysis, being “there – like our life” (*On Certainty*, 1969, sec. 559)⁸. In particular, Wittgenstein observes that it is only by virtue of some established assertions, taken as certain, that doubt is possible. “Doubt itself rests on what is beyond doubt” (sec. 519). What is seen as established varies from one time and place to another; but some taken-for-granted notions are always necessary as a background in order to distinguish “true” from “false”.

But I did not get my picture of the world by satisfying myself of its correctness; nor do I have it because I am satisfied of its correctness. No: it is the inherited background against which I distinguish between true and false. (*On Certainty* sec. 94).

4. Ethnographic research as theory-laden and procedure-laden

By now it has become well “established” that any ethnography is necessarily the representation of a culture from a particular point of view. As Hilary Putnam points out, it is not possible to think of describing the world with “God’s eye view” (Putnam 1981)⁹. The most common argument for seeing knowledge as inevitably perspectival is that every act of observation is, as Hanson (1958) pointed out, “theory

⁸ There are important analogies here with Schütz’ discussion of common sense.

⁹ Cf. also the comments Smith and Deemer (2000) make on the unavoidability of relativism.

laden”. I fully agree with this, but I would like to add a point (sec. 4.1 below) and make a specification (4.2).

4.1 On “*procedure-ladenness*”

The content of an ethnography is not shaped solely by the theories which guide observation, but also by the concrete observational procedures employed to apply that theory in the field. This means that doing ethnography is not just “theory laden” but also “praxis” or “procedures laden”. In other words, the observational role taken up, the research techniques used, the implicit or explicit forms of sampling adopted, and more in general the line taken up during ethnography – all these, as well as theoretical preconceptions, go towards determining the representation of the culture being studied¹⁰.

This means that we must open to inspection, not just the theory being used and the “pre-comprehension” (Gadamer 1960) which lies behind the way a culture is interpreted, but also an outline of the ethnographic experience which had an equal influence in shaping the ethnographic text. This is the purpose of a reflexive account which gives information about the methods used – methods in the etymological sense of “path by which” we arrived at a particular representation and construction of the object in question¹¹.

¹⁰ I believe it is even more essential to give an account of the circumstances and methods of the research than a description of theoretical assumptions, for the latter can often be inferred from the body of the text – e.g., in the authors cited, in the “idion” adopted (theoretical terms, model of exposition, etc.)

¹¹ From this standpoint, the reflexive account performs a function similar to that fulfilled in other contexts by the operational definition. In this case, it is an emerging operational definition, which takes shape, not before the documentation is built up, but only when this stage of the research process has ended; something that tells – after the fact – how we “measured” what we encountered in the field.

4.2 *There are theories and theories*

A couple of distinctions are in order regarding the theory-laden nature of observation. The various theories which, like “coloured spectacles” (Mills 1963), shape the way we experience and represent the culture being studied differ amongst themselves. They differ in terms of their origins (scientific or common sense language¹²) and in terms of the forum which assesses their plausibility (a section of the scientific community or the totality of reasonable individuals). For example, an ethnographer studying South Africa might make use of Parkin’s theory of social closure (1979), but also a much more widely-shared common sense theory which allowed him to distinguish between Whites and Blacks. Thus, ethnographers studying apartheid are able to orient themselves in the various neighbourhoods of Johannesburg and recognize processes of “community closure”, “exclusion”, “usurpation” or “double closure” (Parkin op. cit.) due to a series of established propositions enabling them to identify Whites and Blacks as such.

This distinction between kinds of theories has evident implications for assessing the extent of relativism (a theme I can only refer to briefly). The fact that representation of a culture is based not only on propositions which are theoretical in the explicit sense (i.e., in the strict sense which Maxwell 1992: 291–3 gives the term), but also on widely-shared knowledge – what I term established knowledge – means that we can draw on the latter to separate the terrain between the (various) plausible interpretations and those which are implausible. For interpretations which conflict with notions which are established, with images of the world which the totality of rational individuals adheres to, will be implausible. These propositions make it possible (to use a recent incident taken from the Italian news) to decide whether the experience of opponents of the Fascist regime in Italy was that of a “holiday resort” or of “political confinement”. In other words, the construction of the interpretative edifice intended to describe a culture rests on a set of

¹² On the contribution which implicit or common sense theories make towards the representation of a culture, see Maxwell’s comments on the idea of “descriptive validity” (Maxwell 1992: 287).

established propositions. Numerous edifices can be constructed on those foundations, but no edifice can remain without resting on them¹³.

5. Reflexive accounts for what?

What I have called “procedures ladenness” constitutes, in my opinion, the most convincing argument for including in all ethnographies a careful reflexive description, which as Altheide and Johnson (1994) say, helps the reader and the wider “audience” to understand: i) where the author is coming from; ii) what form of observation relationship enabled the ethnographer to distill a representation of the culture being studied (or an answer to the question from which the study moved)¹⁴; and iii) what is the specific, case by case, link between the various statements an ethnographic account is made up of and the observational experience which inspired them.

A reflexive description is *one* (not the only, and perhaps not the most important) thing to which the scientific community can refer when deciding which aspects of the ethnographer’s representation to “adopt” (Goodman and Elgin 1988) and which to reject. In other words, the reflexive account allows readers to conduct a kind of “thought experiment” whereby the research itinerary followed by the ethnographer is followed through in imagination, so that the reader as-

¹³ In a recent article, Hammersley (1999: 577) recalls the metaphor used by Otto Neurath, which “compares the task of scientists to that of sailors on the open sea who must reconstruct their ship but are never able to start from base”. In order to be able to repair or partially re-build their ship on the open sea, sailors need to be able to rest on a set of planks which they know will float. Established propositions have a similar function to these planks: the work of constructing an interpretative edifice rests on these planks.

¹⁴ On the legitimacy of an ethnography oriented, not towards the holistic reconstruction of a culture’s profile, but, more humbly, towards answering certain questions about it, see Hammersley (2008: 50; 135).

sesses the ability of the empirical procedures employed to produce the findings the ethnographer claims¹⁵.

On a more strictly methodological plane, we might say that the contribution of a reflexive account to the construction of objectivity can be identified in three areas, which I outline in the following three sub-sections.

5.1 Reflexive accounts as instruments for reporting uncertainty

An important contribution which reflexive accounts make is to give readers the tools for establishing the degree of certainty (see King, Kehoanne and Verba 1994: 31–33) which can be attributed – in accordance with the characteristics of the observational relationship – to the various statements making up an ethnographic account (or at least certain key statements)¹⁶. Reflexive accounts enable readers to assess the plausibility of each statement (or at least of the most salient one) by scrutinizing the empirical condition which led to their formulation.

The distinctive features of ethnographic work, its deep-seated dependence on the context, make it impossible to identify general rules that can link the conditions under which empirical documentation is produced to the plausibility of the statements that are, as it were, distilled from it each time. It is hard to maintain, without uncertainty, that the statements extracted from “naturally-occurring data” are usually more plausible to those about the same phenomenon but which have been elicited by the researcher’s action¹⁷. The lack of general rules makes it necessary to scrutinize – case by case – the conditions for

¹⁵ If – by definition – the criterion of “public reproducibility” cannot be met for ethnographic research, a reflexive account can give readers all the elements they need to go through the experience at least mentally.

¹⁶ This is equivalent to the function performed by the Qualifier in Toulmin’s model (1958).

¹⁷ Another position is taken by van Maanen (1979), who establishes a hierarchical relationship between the two contexts for producing empirical data that leads him to count chiefly on naturally occurring data (“operative data” in the author’s terminology) in producing plausible statements.

producing empirical documentation, shedding light on their limits (or at least those that can be intercepted), and identifying the set of questions for which the collected material can furnish an authoritative answer¹⁸. An example may make the idea clearer. If in a description of an esoteric community the ethnographer says that he did not take part in the rites which took place in the secret temple, but obtained a vague and reticent description from a number of members of the sect, his information of that aspect of the culture will be considered less plausible than parts of his description based on practices in which he himself participated numerous times¹⁹.

This function of the reflexive account might be compared with the kind of statistics which indicate a margin of error for an estimate: significance level and interval of confidence.

At the basis of this idea is the conviction that material collected in an ethnographic account should not be considered an indivisible whole – as though it was plausible or implausible *in its totality*. Even for a text which constructs its own interpretation of a culture using one, and just one, image or metaphor it is still possible – drawing on a reflexive account – to establish which aspects of the culture fit the image best and which least.

5.2 *Reflexive accounts as a methodological criterion?*

A question which is worth considering is the methodological status of reflexivity (in the sense intended in this paper). Can reflexivity be considered a *criterion* for establishing the objectivity of an ethnographic account? I do not feel able to give just one reply to this question, for the answer depends on what we mean by criterion.

Reflexivity *is not* a methodological criterion in the sense that it does not “indicate criteria of success established beforehand” (Rorty

¹⁸ This corresponds to what Jerzy Topolski, historiographer, defines as a document’s area of authenticity, viz., the “sum total of those questions (problems) which a given source is capable of answering truthfully” (Topolski 1973).

¹⁹ I am thinking of my own study of the making sacred of nature, based on ethnographic study of two communities (Cardano 1997).

1991). It cannot be thought of as an algorithm – something we can follow and be guaranteed of ending up in the safe haven of objectivity. Nor can it be considered a methodological criterion in the sense of a set of operative instructions of the kind furnished by, say, grounded theory (at least as the latter was originally intended). Rather, exhortations to reflexivity concern what needs to be done *in addition to and in parallel with* fieldwork.

Reflexivity can, however, be considered a methodological criterion in the sense that a reflexive account gives the scientific community important (though not conclusive) tools for assessing the plausibility of (some or all of) the assertions made in the ethnography.

Lastly, I believe it can be said that a reflexive account gives the academic community “criteria”, in the strict sense suggested in texts of qualitative methodology – credibility, reliability and plausibility (Glaser and Strauss 1967; Guba and Lincoln 1982; Hammersley 1992) To evaluate the quality of an ethnographic work. A detailed reflexive account offers the community of ethnographers all the elements it needs to decide about the credibility, reliability and plausibility (see above) of the contribution. As an aside, I might add that I believe that a detailed reflexive account also provides the elements necessary for assessing an ethnography in terms of more conventional notions of validity and reliability (cf. Kirk and Miller 1986). As I have argued in a previous work (Cardano 2001), this is true even when such notions are understood in a generally realist sense, as tends to be the case among quantitative researchers.

One final point is worth making. If reflexivity can be considered a criterion, rather than being a scientific or scientist criterion, it ought perhaps to be seen as a criterion of common sense, something along the following lines: “If you want me to believe what you tell me you saw, explain to me how you came to see it”²⁰.

²⁰ The difficulty of living with any scientific criterion is convincingly argued by Schwandt (1996).

5.3 Reflexive accounts: creativity versus standardization

I will end my remarks on the methodological peculiarities of reflexive procedures with a brief comment on creativity, as opposed to standardization. The exhortation to include a reflexive account as part of an ethnography should not be seen in terms of a strict methodological canon which must be followed to the letter. Presenting things in this way would inevitably lead to conformism or worse still, to a sort of methodological surveillance (see Paechter 1996). Reflexive practices can be adopted by researchers of all methodological faiths, whether they are adepts of grounded theory or try to follow precepts of narratological analysis. Since they have no ties to any specific methodological approach, reflexive practices do not inhibit, but actually encourage methodological creativity. And no less important, these creative exercises can become a shared heritage that is useful for both research and didactic purposes.

6. What's wrong with reflexivity?

I conclude by examining the main objections made with regard to reflexive procedures as a means for constructing objectivity. Three seem to me particularly pertinent: i) the self-contradictory nature of reflexive pretensions; ii) the danger of untruthfulness; iii) the imperfections of the forum supposed to decide on objectivity, and more in general on the plausibility of a contribution to knowledge.

6.1 Epistemic bootstrapping: the paradox of reflexive accounts

The first and perhaps most stinging objection concerns the epistemic status of reflexive accounts, and particularly their claim to providing ethnography objectivity. Critics who raise this objection²¹ argue that reflexive accounts are needed because it is believed that the ethnographer's description of a culture is necessarily tied to a specific

²¹ I am grateful to Marco Marzano for forcing me to face this problem.

point of view which the reflexive account ought to enable the reader to recognize. However (so the criticism goes), what reason do we have for believing that the description of the work of ethnographic description is immune from the distortions produced by a specific point of view? What special powers enable reflexive ethnographers to lift themselves off the ground and regard their own work from on high, simply by pulling on their own bootstraps, in the manner of Baron Munchausen?

My reply to this objection draws on the distinctions I developed in my epistemological digression in section 3 (thus hopefully justifying the fact that I forced readers to plough through a tangle of quotations). In particular, the distinction between established propositions and propositions open to doubt is relevant here. The reasons which lead me to have confidence in the ability of a reflexive account to promote the objectivity of an ethnography have to do with the different combination of these two types of propositions in the text describing the culture studied, as opposed to the text describing the procedures followed by the research. An effective reflexive account is made up, in my view, of a suitable number of descriptions based on widely shared “theories”, deriving from assumptions which have the character of established propositions. I am thinking, in particular, of the description of the duration of the fieldwork, which will be cast in terms of a number of days, weeks, years. No-one doubts the meaning of these terms (the question of mendacity will be considered below). Descriptions of the observational relationship in terms of whether the ethnographer was inside, outside, or in the vicinity of, the tribe studied have a similar epistemic status. I am thinking here, for example, of the description Humphreys gives of his observational role in *Tearoom Trade* (1975). To sum up: the reflexive account ensures a basis for assessing objectivity through its “descriptive protocols”. Among these are those “coloured spectacles” (Mills, op. cit.) which all reasonable people have on their noses: descriptions which have been worked out on the basis of criteria of relevance (Sen 1982) implicitly chosen by all. It is these which constitute a large share of the ingredients in a reflexive account.

6.2 Mendacity

Salzman (2002) stresses the problem of untruthfulness, asking “what ensures that the writer of the account is not telling fibs, not spinning an account which intentionally confers plausibility to their representation of the culture being studied?” The question seems to me an important one; I will try to reply on two levels.

On the basis of what I have argued above regarding the ingredients a good reflexive account ought to contain, we might reply that the mendacious ethnographer can be unmasked. For many of the assertions made in an ethnographic account are, in principle, open to public inspection. “You say you were in the Raelian sect for two years – ok, let’s go and see what the Raelians say”; “You say you made more than a hundred interviews – show us the tapes and the transcriptions”...

My second reply is that the problem of mendacity does not just regard reflexive practices and it thus seems illegitimate to cite this as an objection to the latter. Researchers can cheat when they are describing their ethnographic experience in the same way as they can cheat (as the history of science indeed shows) with laboratory experiments, with survey research, and so on. The problem of ethics is not just a problem for reflexive ethnographers; if it is a real problem – as indeed it is – it must be taken seriously by the whole scientific community. And on many occasions the scientific community has shown itself capable of defending itself.

6.3 *The problem of the forum being laden with social, academic and media power*

The third objection goes to the heart of the process of constructing objectivity since it questions the deliberative virtue of the scientific community and (in some versions of the critique) maintains that the very idea of a forum in which all participants calmly discuss the claim of this or that ethnographic account to objectivity is unrealistic (cf. Smith and Demeer 2000: 892). In particular, it is argued (this is the position taken up by Smith 1985: 6) that it is impossible to come to a non-constrained agreement, to use Rorty’s term, leaving aside the so-

cial, academic and media power of the decision makers and of the researcher whose contribution is being assessed. These are legitimate objections – so legitimate that the scientific community has set up organizational measures, such as those which are supposed to ensure anonymous review. We all know only too well that these kinds of measures are insufficient. However, recognizing that there is much room for improvement is not the same as denying their effectiveness. The situation is well expressed by Geertz in comments he makes on the key theme of my paper – objectivity:

I have never been impressed by the argument that, as complete objectivity is impossible in these matters (as, of course, it is), one might as well let one's sentiments run loose. As Robert Solow has remarked, that is like saying that as a perfectly aseptic environment is impossible, one might as well conduct surgery in a sewer. (Geertz, 1973: 30).

In the same way, the fact that an absolutely impartial forum is not possible does not mean that we may just as well leave decisions to the tribunal of the Holy Inquisition or to some hypothetical Confraternity of Cheats.

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