

Ethnographic Cognition and *Writing Culture*¹

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One of the best ways to pursue and go beyond the programme of *Writing Culture* (Clifford and Marcus 1986), I suggest, takes as its point of departure the *cognitive anthropology of anthropology*. Situating *Writing Culture* with regard to this field of research can contribute to its further development. It is, after all, sensible to start the anthropological study of anthropology with an analysis of its own cultural productions: ethnographic texts. The analyst can then identify the relevant properties of such cultural products and track down their causes. These causes include especially the cognitive processes of working ethnographers.

Starting with textual analysis, I will argue that some of the rhetorical conventions that are viewed critically by contributors to *Writing Culture*, rather than being misleading, actually serve to inform the reader about the cognitive genesis of the ethnography. The information conveyed when complying with these conventions enables readers to evaluate the reliability of ethnographic accounts and anthropological analyses.

Following the textual analysis, I specify some of the cognitive processes at work in the production of ethnographies. These include, for example, a reflexive and critical cognition that is distributed among the community of anthropologists and also 'mind-reading' – a cognitive process, much studied by cognitive psychologists, that enables ethnographers to make sense of the behaviour of indigenous people² by attributing mental states to them (beliefs, intentions, desires, feelings).

***Writing Culture* and the Cognitive Anthropology of Anthropology**

Writing Culture as a Project in Naturalized Epistemology

The core project of *Writing Culture* (Clifford and Marcus 1986) consists in adopting a reflexive attitude with regard to the production of ethnographic writings. There is no doubt

that a reflexive attitude is worth pursuing as part of any scientific enterprise; what could be controversial, in *Writing Culture*, is the content of those reflections. James Clifford, in his introduction to *Writing Culture*, foresaw that the authors of the contributions to the book would be 'accused of having gone too far' (1986a: 25). Indeed, many anthropologists are weary of the ever-present reflexivity in post-modern writing, which results in ethnographies in which self-contemplation takes precedence over information about the life of the indigenous people. Such criticisms pertain to the way in which specific *prescriptions* made by the authors of *Writing Culture* have been applied by others. The *Writing Culture* movement has also been viewed as a critique of an over-confident positivism, which was initially justified and beneficial but is now no longer fruitful. Criticisms of this type pertain to the *epistemic evaluations* of the ethnographic project of the authors of *Writing Culture*.

The way to refute, counterbalance, and/or update the ideas announced in *Writing Culture*, is, nonetheless, to continue thinking about what ethnographic writings are, could be and should be, and to continue developing the reflexive project of *Writing Culture*. Indeed, nobody would claim that a full understanding of the complex social and cultural processes that yield ethnographies has already been achieved. In this sense, the anthropologists involved in the *Writing Culture* project, which is understood as a reflexion on ethnographic practices, have not gone 'too far'.

The reflexive project of *Writing Culture* includes three programmes of research: (1) a descriptive, (2) an evaluative and (3) a normative programme.

1. The descriptive programme attempts to bring into the open the ways in which ethnographers explain other cultures in written texts. With *Writing Culture*, a set of largely unquestioned social phenomena comes under scrutiny: the book analyses the work of the ethnographer, his social relations with the people about whom he writes, the rhetorical conventions he uses, and his epistemological beliefs.

2. The evaluative programme consists in assessing the value, scientific or otherwise, of the ethnographers' productions. Biases, such as those resulting from the cultural constellations and power relations that emerged during the history of colonialism, are denounced. The authors of *Writing Culture* also reveal the 'masking' in ethnographic descriptions and the 'mystifications' they entail, qualifying ethnographic truths as 'inherently partial' (Clifford 1986a: 7).
3. Finally, the normative programme proposes or prescribes methods for producing better ethnographic representations. In particular, the contributors of *Writing Culture* advocate the production and use of new rhetorical conventions.

In *Writing Culture*, as in many critical enterprises, these three programmes are strongly interconnected and not explicitly distinguished. It is fruitful, however, to distinguish between them, first, so that one can specify the programme that one is pursuing in developing further the project of *Writing Culture*; and, second, so that one can show how one intends to relate the three programmes to one another. For instance, Logical Positivists conceived of epistemology as an *a priori* reflection on science and its method. They conceived of the normative programme as being independent of the empirical analysis of actual scientific practices. By contrast, Naturalized Epistemology put the descriptive programme prior to the evaluative and normative programmes. From this perspective, epistemology is based on empirical investigations about how knowledge is produced, which subsequently inform prescriptive reflections (for reference texts on Naturalized Epistemology, see Kornblith's 1987 anthology).

A similar view of the relation between description and prescription is also present, although less explicitly, in critical theory of literary studies, from which *Writing Culture* takes its inspiration. The authors who contributed to *Writing Culture* endeavour to describe and criticize ethnographic practices before proposing some new practices: self-consciousness and new rhetorical devices. Clifford's work (e.g., 1982, 1986b) is a case in point. His first action

is to analyse the rhetorical devices that were used in major ethnographic writings. He then denounces the mystifying effects of these devices, such as unjustified attributions of authority. Finally, he proposes some new rhetorical tools for achieving the goal of ethnography, viz. polyphonic writing. For Clifford, an attack on the book *Writing Culture* for its relativism should ‘make clear why close analysis of one of the principal things ethnographers do – that is write – should not be central to evaluation of the results of scientific research’ (1986a: 24). Here again, the project is presented as being based on empirical analysis, followed by evaluation. The distinction between programmes is made for methodological purposes. The goal is to obtain descriptions that are as independent as possible from various possible normative ideas of science³, while normative epistemology should be as realistic as possible – adapted to actual practices and to the possibilities of implementation.

A consequence of this hierarchical organization of epistemological thinking, from the descriptive to the normative, is that, if the description is flawed, then the evaluations are likely to be wrong and the prescription inappropriate. A good way, therefore, to further the *Writing Culture* movement critically is to start with the descriptive programme. Indeed, it may be noted that most attacks on the *Writing Culture* movement have not fully addressed the descriptions of ethnography³ that can be found in the book, but, rather, have focused on the particular applications of methodological advice. Finally, describing socio-cultural phenomena ought to be what anthropologists do best. Describing ethnography fits squarely within the traditional tasks of anthropologists, belonging to the anthropology of science. It is an anthropology of anthropology, a genuinely reflexive project.

The Study of Ethnographic Practices

Anthropology itself has been studied from a historical point of view by historians and anthropologists themselves (e.g., Stocking 1992; Spencer 2000; Kuper 1983 and 1999), from a philosophical point of view (especially the question of rationality, as in Hollis and Lukes

1982; Wilson 1970) and from a methodological point of view by those authoring research guides on anthropological methods. Spencer (2000) shows how the community of British anthropologists defines anthropology during the course of events such as research seminars. This shows that the reflexive attitude can be found in many anthropological activities. Since any discipline is to a large extent auto-constitutive, it is bound to declare, more or less explicitly, what it is and what its aims are. Nonetheless, the anthropology of anthropology is far from constituting a systematic and productive field of research.

Ethnographic practices and the making of anthropological knowledge are themselves socio-cultural phenomena that deserve the anthropologist's attention. One specific contribution of *Writing Culture* to reflexive anthropology is in its detailed description and analysis of written representations as cultural products. The focus on texts is justified, because writing is constitutive of the ethnographer's work; it is always present, and the ethnographic enterprise importantly relies on the written mode of communication. Among the many processes and properties of scientific communication, it is mainly the 'rhetorical conventions' used by ethnographers that are analysed in the contributions to *Writing Culture*.

One way to go 'beyond *Writing Culture*', as the editors of this volume advocate, is to further the study of texts through the study of the causes and effects of the properties that textual analyses have revealed. In the introduction to *Writing Culture* (1986a), Clifford says: 'ethnographic writing is determined in at least six ways: (1) contextually ...; (2) rhetorically ...; (3) institutionally ...; (4) generically ...; (5) politically ...; (6) historically' (1986a: 6). Mind and cognition are absent from this list, and yet they are important determining factors: how and what people communicate depends on what they think, which in turn depends on their cognitive abilities. Against the assumption that the ethnographer's mind furnishes nothing but the locus where social and cultural determinations come together to influence the ethnographer's behaviour, findings from cognitive psychology suggest that the specifics of the human mind do impinge significantly on ethnographic production. In this chapter, I will

describe some of the cognitive aspects of human communication and the cognitive abilities with which humans come to understand the intentions of others.

The approach I advocate consists in situating the production of public representations – ethnographies in our case – within the chain of production of representations and their cognitive effects. I propose to use cultural epidemiology (Sperber 1996) to study ethnographers' practices and beliefs and the cognitive causal chain that produces ethnographies and ethnographic knowledge. The underlying pattern for thinking about ethnography in *Writing Culture* seems to be the simplistic chain shown below, upon which power relations and other macro-social events may subsequently be imposed:

fieldwork → ethnographic data gathering → ethnographic description

Instead, I suggest investigating the details of the processes through which representations are created and transformed, going from minds to the environment, through behaviour such as speaking or writing, and back again into other minds. This approach should serve to restore to cognitive processes their proper role in the production of the public representations being investigated. While *Writing Culture* has limited its analysis to the contingencies of language, rhetoric, power and history, I propose that the thinking processes by means of which ethnographers produce ethnographies and understand the ethnographies of their colleagues be taken into consideration. This approach implies looking at the infra-individual level, eventually making reference to the properties of the human mind. I illustrate this approach in the last section, where I argue that ethnographers in the field rely significantly on their cognitive abilities when they attribute intentions to others.

In cognitive anthropology and socio-cognitive research, emphasis falls not only on the infra-individual level but also on the transmission and transformation of representations in the environment and through social interactions. In other words, cognitive processes are not

restricted to working brains; rather, cognition, as cognitive anthropologist Ed Hutchins (1995) puts it, is distributed. Let us call ‘ethnographic representations’ those representations that have a significant role in the causal cognitive chains that lead to the production of published ethnographies. An epidemiological approach in the anthropology of anthropology would consist in tracking down ethnographic representations, from the perceptions of the anthropologist in the field to the dissemination of a published ethnography in the scientific community, and, thus, specifying the flow of these representations and the transformations they undergo. There is a rich set of cognitive processes at work in making sense of indigenous people’s behaviour. These processes are implemented in the field, at the ethnographer’s home university, when she is reading field notes, when she is phrasing the account for communicating with their colleagues, when the account is understood (or not), when feedback is given, when reviewers comment on texts, and when texts are edited. This incomplete list of the events that make up the cognitive causal chains that produce ethnographic knowledge shows that ethnographic cognition implies *distributed cognition*. I will return to distributed reflexive cognition as an important process that finds expression in ethnographies. In this epidemiological theoretical framework for reflexive anthropology, analysis of ethnographic texts has its own place: it is a key public representation that carries information, as I will argue, about the social cognitive causal chains of which it is an output.

To date, there are relatively few published contributions to the cognitive anthropology of anthropology, and even these are not always presented in these terms. Paulo Sousa’s paper (2003) is a remarkable case, as it is a self-declared work in the cognitive anthropology of anthropology, attempting to explain the history of the anthropology of kinship. In the last section, I will briefly refer to the work of Maurice Bloch (1998) and Dan Sperber (1996: chapter 2), which I will relate to the approach and findings of *Writing Culture*. Now, however, I turn directly to a discussion of the work of Pierre Bourdieu, which will allow me to specify my understanding of the place of *Writing Culture* and textual analysis in the

cognitive anthropology of anthropology. I will show that Bourdieu's understanding of the *Writing Culture* movement could be more charitable, and that the limits of his own reflexive project are in some senses similar to the limits of the *Writing Culture* approach: neither gives sufficient attention to the specifics of ethnographic cognition.

Bourdieu Versus the Writing Culture Movement?

Bourdieu describes his programme of 'participant objectivation' as the cognitive anthropology of anthropology, or more precisely as '*reflexive cognitive anthropology*' (2003: 285). What is especially 'cognitive' in this sociology/anthropology of science? Bourdieu refers to Durkheim's research programme in the sociology of knowledge (Durkheim 1912), which involves uncovering the categories of thoughts and their social origins. Applying this to the academic community, Bourdieu speaks of the 'academic transcendentals' in order to designate the categories of professorial understanding: for instance the classificatory schemata that French teachers implement in assessing students. He is, thus, applying his theory of *habitus* (1972) to French academia.

Assessing the extent to which Bourdieu's work may be deemed a contribution to the cognitive anthropology of anthropology would require questioning the truth of the psychological assumptions that are developed in his theory of *habitus*. Here, it must suffice to note that the theory of *habitus* is, understandably, not informed by current theories in cognitive psychology. Like many theorists in the social sciences, Bourdieu often assumes that the human mind is a blank slate upon which culture and society write. He attempts to decipher what is being written on the blank slate in terms of *habitus*, but the specificity of the human mind is not given its proper role. Consequently, Bourdieu's reflexive cognitive anthropology departs far less from the project of *Writing Culture* than he suggests. The two projects remain fundamentally similar: the goal, in each case, is to reveal the epistemologies underlying ethnographic texts and the social context in which they arose.

Bourdieu (2003) insists on the importance of scientific analysis of the socio-cultural conditions of ethnographic knowledge production, and, indeed, he has dedicated much of his work to the study of the 'academic world' (1975, 1976, 1982, 1984, 1997, 2001, 2003, 2004). His inaugural lecture in the Collège de France (1982), for instance, is a sociological analysis of inaugural lectures in the Collège de France, viewed as academic rituals and institutions. In his book, *Homo academicus*, Bourdieu attempts to 'unveil and divulge ... the objective structures of a social microcosm to which the researcher himself belongs, that is, the structures of the spaces of positions that determine the academic and political stances of the Parisian academics' (2003: 284). When applied to the social scientist himself, and, thus, also to the ethnographer, the programme is called 'participant objectivation'. It consists in the objectivation of the ethnographer, viewed as an objectifying agent.

Attempting to clarify his programme, Bourdieu spends some time distinguishing it from the *Writing Culture* movement. Unfortunately, he employs a dismissive tone, rather than a well argued refutation of the points of Writing Culture, as is evident in the following quotation:

[R]eflexivity as I conceive it does not have much in common with 'textual reflexivity' and with all the falsely sophisticated considerations on the 'hermeneutic process of cultural interpretation' and the construction of reality through ethnographic recording. Indeed, it stands opposed at every point to the naïve observation of the observer which, in Marcus and Fisher (1986) or Rosaldo (1989) or even Geertz (1988), tends to substitute the facile delights of self-exploration for the methodological confrontation with the gritty realities of the field. This pseudo-radical denunciation of ethnographic writings as 'poetics and politics' to borrow the title of Clifford and Marcus's (1986) edited volume on the topic, inevitably leads to the 'interpretive scepticism' to which Woolgar (1988) refers and nearly manages to bring the anthropological enterprise to a grinding halt (Gupta and Ferguson 1997). But it does not suffice either to explicate the 'lived experience' of the knowing subject, that is, the biographical particularities of the researcher or the *Zeitgeist* that inspires his work ... or to uncover the folk theories that agents invest in their practices, as the ethnomethodologists do. For science

cannot be reduced to the recording and analysis of ‘pre-notions’ (in Durkheim’s sense) that social agents engage in the construction of social reality; it must also encompass the social conditions of the production of these pre-construction and of the social agents who produce them (Bourdieu 2003: 282).

I want to defend the *Writing Culture* authors from Bourdieu’s attack by suggesting that there is no qualitative difference between their research and his. Both are engaged in a descriptive reflexive project. In contrast, one may renew and improve reflexive research in anthropology by taking the results of cognitive science seriously and using them as a basis for understanding of ethnographic practices.

Bourdieu’s first criticism is directed against the scepticism induced by *Writing Culture*. Arguably, it is valid to dismiss an assertion, not on the basis of its content but on the basis of its epistemic consequences; but the point is that *Writing Culture* does not necessarily lead to destructive scepticism. A radically sceptical attitude is arrived at only if, after the descriptive analysis, one pursues an evaluative programme leading one to conclude that viable criteria of scientific investigation (e.g., an idealistic positivist model of science) are unattainable. Although such thoughts are lurking in *Writing Culture* (for instance, when Clifford laments that only partial truths can be obtained in ethnography), the authors of the volume do attempt to propose new modes of scientific knowledge production, rather than dismissing the ethnographic project altogether.

Bourdieu’s second criticism concerns the scientific status of the targeted reflexive enterprises. He provides a well detailed analysis of the ‘social conditions’ that determine academics’ pre-notions, and his analyses are developed using the explanatory power of sociological tools and concepts. *Homo academicus*, for instance, includes a number of graphs and statistical data – an ostensibly scientific approach that the authors of *Writing Culture* did not consider. Bourdieu’s contribution to the sociology of the social sciences puts more emphasis on scientific analysis than does *Writing Culture*. However, it remains the case that

both Bourdieu's project and the *Writing Culture* project are reflexive in a way that gives prevalence to the analysis of the making of science. The textual analysis of *Writing Culture* is no less empirical than Bourdieu's clearly sociological analysis: textual analysis takes texts as cultural objects, describes their properties and studies their causes and effects.

The third attack on the *Writing Culture* programme addresses the substitution of 'the facile delights of self-exploration for the methodological confrontation with the gritty realities of the field'. Self-consciousness is not the only proposal of *Writing Culture* – there is also, for example, the advocacy of polyphonic writing – but it is admittedly among the most prominent effects of *Writing Culture* on anthropological practice. Independent of the value of these self-exploring works for the advancement of anthropology in general, I think that these works can provide valuable information for the development of the anthropology of anthropology. Finding ways to exploit these works, rather than just dismissing them, is an important step in going 'beyond *Writing Culture*'. In Bourdieu's work one can observe that the more reflexive he gets, for example, the more he focuses on his own practice as a sociologist and anthropologist, the closer he comes to the self-exploratory texts of the *Writing Culture* movement.⁴

The 'study of the social conditions of the production of pre-construction', which is how Bourdieu characterizes his own reflexive research programme, is already present in *Writing Culture*. While the emphasis is on textual analysis, it aims at understanding the social and cultural origins and effects of rhetorical devices. Presenting the *Writing Culture* volume in the introduction, Clifford explains as follows: 'Most of the essays, while focusing on textual practices, reach beyond texts to contexts of power, resistance, institutional constraint, and innovation' (1986: 2). For instance, the underlying epistemology of the anthropologist is shown to justify the rhetoric of scientific authority, which itself reinforces the epistemology. The effects of colonialism on ethnographic practice are also analysed. As defined by Clifford, the programme of *Writing Culture* sounds quite rich, while we might wonder whether

Bourdieu's programme ever reaches beyond the study of institutions and their modes of constraint. A more modest criticism of *Writing Culture* can be formulated however. Its strength, which is the focus on texts, is also one of its weaknesses, for the analysis of the contextual determinations of ethnographies remains incomplete.

One of the reasons why Bourdieu does not manage to 'go beyond' the *Writing Culture* movement is that he explains the behaviour of the ethnographer only with reference to the social conditions in which she is embedded. When it comes to the analysis of the behaviour of an individual scientist such as himself, Bourdieu inevitably ends up – despite his denials – in an autobiographical mode: the search for the cause of his own *habitus* consists in retracing his own life. If, in contrast to Bourdieu, one abandons the erroneous blank slate image of the mind, then one opens up the investigation of a whole range of determinants of the behaviour of the ethnographer: those that derive from the specific nature of the human mind. Self-exploration is then relegated to one factor of explanation, which must be understood in conjunction with what is known of human cognition, i.e., taking into account the results of psychology and cognitive science. Another point that limits Bourdieu's analysis concerns his understanding of the social phenomena involved in ethnographic cognition, confined as it is to the transmission of values, practices and more generally *habitus*. However, the production of ethnographic writing involves socially implemented cognitive processes that are highly relevant for understanding ethnographies. Bourdieu's analysis must be enriched with the idea of distributed cognition.

From Texts to Thoughts: Referring to the Cognitive Genesis of Ethnographies

I argue in this section that some rhetorical conventions used in ethnographies provide information about the cognitive means that ethnographers have employed in acquiring ethnographic information and in formulating theories. These rhetorical conventions are used to communicate relevant information to the reader for assessing and understanding the truths

of ethnographic descriptions. I review three such rhetorical conventions. The following two conventions are well described in *Writing Culture*: the autobiographical section at the beginning of ethnographies; and the absence of the author in the main body of the texts. I also point out a third convention: the use of acknowledgements. One of my goals here is to relate the textual approach of *Writing Culture* to the cognitive anthropology of anthropology.

Why We Write and Read Acknowledgements

Here is a quotation from *Writing Culture*:

I would like to thank the members of the Santa Fe seminar for their many suggestions incorporated in, or left out of, this Introduction. (I have certainly not tried to represent the “native point of view” of that small group.) In graduate seminars co-taught with Paul Rabinow at the University of California at Berkeley and Santa Cruz, many of my ideas on these topics have been agreeably assaulted. My special thanks to him and to the students in those classes. At Santa Cruz, Deborah Gordon, Donna Haraway, and Ruth Frankenberg have helped me with this essay, and I have had important encouragement and stimulus from Hayden White and the members of the Research Group on Colonial Discourse. Various press readers made important suggestions, particularly Barbara Babcock. George Marcus, who got the whole project rolling, has been an inestimable ally and friend (1986a: 26).

You will have recognized this usually small part of academic texts, written in smaller characters and occupying a relatively modest place within the written artefact, most often in an endnote. These are the acknowledgements. As modest as their positioning may appear to be, acknowledgements form a genuine part of academic texts. They can be the subjects of textual analysis, as they constitute a genuine literary convention in the academic world. I am not aware that this convention has been studied by the authors of the *Writing Culture* movement, but acknowledgements provide a rich source of information on their favourite topics, such as power relations.

The acknowledgements quoted above are James Clifford's, located at the end of his introduction to *Writing Culture* (Clifford 1986a: 26). The passage in question is rather long, when compared to acknowledgements in most articles, and it contains many names; but it is relatively short for the acknowledgment section of a book – it is in between. Not everything can be done in an acknowledgement section. Jerry A. Fodor, a philosopher of mind, allows us to see the conventional and very formal aspects of the acknowledgment section by mocking it. After telling the usual story of how his book arose in a series of lectures, etc., and after acknowledging the feedback given to him by his peers, he continues, saying: 'Not one red cent was contributed to the support of this work by: The MacArthur Foundation, the McDonnell Pugh Foundation, the National Science Foundation, or the National Institutes of Health. The Author is listed alphabetically' (Fodor 2000). Are acknowledgements purely formal artefacts with no genuine function other than allowing the author to express his debts – as if repaying these debts were a pressing desire that authors indulge in, and that editors and readers put up with? What do we learn from Clifford's acknowledgements? Why did he write them? Why did he think it important to include this boring list of names in his text? Having perused a large number of acknowledgements, Cronin, McKensie, Rubio, and Weaver-Wozniak (1993) find that people or institutions are acknowledged for their moral support; their financial support; for having provided access (to facilities, data, etc); for clerical support; and for peer-interactive communication. In general, the result is a somewhat cryptic but sufficiently informative description of the processes through which the article or book has been produced. The above acknowledgements are a case in point. We learn especially how much James Clifford's ideas have been discussed: with the participants of the Santa Fe seminar; with some graduate students at Berkeley and Santa Cruz; and particularly with Paul Rabinow. The list then continues with people who have had an impact on the written article itself. Being mentioned in acknowledgements is not only pleasurable, because it is a token gesture from the author, but it is also a way in which one can acquire prestige and power. The

case of the funding bodies makes the point apparent. The more a funding body is acknowledged in important and valued texts, the more prestigious it becomes. But an inverse flow of prestige is also occurring: acknowledgements are rhetorical devices that confer authority to the author who acknowledges. Acknowledging the contribution of some authoritative person or institution is also appealing to this source of authority as a guaranty of quality. Often, the acknowledgements boast of a network of colleagues, as if one were aligning one's allies in order to impress those readers who may wish to criticize the text. This is the case in *Writing Culture*, in which seven out of the ten contributions (excluding the preface and afterword) include acknowledgements, all containing a list of the names of authorities in some relevant field. A social networking analysis of acknowledgements, questioning for instance the social conditions of reciprocity, would not be out of place in a study within the anthropology of anthropology. A few, quite informative studies on acknowledgements can be found in literature from the field of bibliometrics (Cronin 1995, 2004). In sociology journals, three quarters of the articles include acknowledgements and more than half include acknowledgements which attested to interactive communication among peers. Among those acknowledged, only a very few are frequently included. The analysis revealed no relation between citation frequency and the frequency of being acknowledged (Cronin et al. 1993).

What would the *Writing Culture* movement do and say about the literary convention of acknowledgements? The convention of acknowledgments could, for instance, be denounced: they introduce power relations among academics as tools of persuasion; they are irrelevant to the object of the text; they appear as mystifying rhetorical devices, hypocritically put in a modest place in the text, but they are still meant to be read and still exert their influence on most readers. Pursuing this trend, one could suggest creating some new conventions of acknowledgement, or advocate their total elimination. However, a more descriptive analysis may be needed before evaluations and prescriptions can be recommended. Maybe some

fieldwork would help. Do anthropologists read acknowledgements? What information do they expect to find in them? There seem to be multiple reasons for maintaining this literary convention: authors do derive some pleasure in acknowledging the contributions of friends and colleagues; and saying thanks already lightens the burden of the debt. Funding bodies, also, do insist that they are mentioned in acknowledgements.

Yet, an important reason why the practice of acknowledgment perdures can be traced to the expectations of the readership. In 2002, when one of my informants took her viva for the Ph.D. degree at Cambridge, her two examiners complained that she did not include acknowledgements in her thesis. The examiners wondered, at first, why she did not comply with the literary convention of writing acknowledgements, and then they explained that this omission prevented them from knowing *who* – in other words, which schools of thought and which institutions – had influenced her doctoral work.

For Davis and Cronin, ‘acknowledgements necessarily imply a high degree of social interaction’, and they suggest ‘significant intellectual indebtedness’, especially when they refer to peers in academic life: ‘through their use of acknowledgements as tokens of intellectual indebtedness (“super-citations”), authors seem to conform to a normative behaviour that may be peculiar to a given field or closely related fields’ (1993: 592). Davis and Cronin think that acknowledgements are so revealing of academic life that they suggest using the number of occurrences of names in acknowledgement sections for research assessment procedures. The fact is that attestations of peer-interactive communication are very useful for understanding the cognitive processes through which texts are produced. Acknowledgements communicate something to the reader. They inform her about the cognitive processes that brought about the text and that are distributed among several anthropologists and experts. When Fodor mocks acknowledgements, he reserves his sarcasm for the funding bodies rather than for the peers with whom he actually interacted. Fodor would not want to damage his academic social network; furthermore, information about this

network is relevant to the reader. Situating the author socially helps make sense of the text; it is as informative as reading the bibliography. Thus, acknowledgements refer to a key practice in science – anthropology included – which consists in communicating, discussing, arguing and evaluating the thoughts of others as well as one's own. Acknowledgements tell the readers that the article or book went through the argumentation and the critical processes that are part of scientific practices.

Why The Ethnographer Appears Where She Does

Taking acknowledgement as an illustration, I have started from a textual convention, first asking what reason authors could have for complying with the convention. I have then concluded that acknowledgements communicate something about the cognitive processes that result in the text, and I have suggested that information about these processes helps the reader to make sense of the text and evaluate the arguments. Conventional references within ethnographic texts to the ethnographer herself also bear witness to cognitive processes involved in text production. Both the inclusion and the exclusion of references to the author in ethnographies are denounced in *Writing Culture* as mystifying literary conventions, contributing to the 'textual construction of anthropological authority'. In the words of Rabinow, 'Clifford ... argues that ... anthropological authority has rested on two textual legs: An experiential "I was there" establishes the unique authority of the anthropologist; its suppression in the text establishes the anthropologist's scientific authority' (1986: 244).

I maintain that these conventions are less mystifying than Clifford and other authors of *Writing Culture* pretend. They refer in fact to actual cognitive processes upon which ethnographic writing is based. The reader is not a naïve child being mystified by deceptive rhetorical devices. She is convinced by a text because the rhetorical conventions that it displays correspond to two types of cognitive processes – interpretation during fieldwork and

explanatory reflection in the academic world – that are themselves taken as warrants of good ethnographic thinking.⁵

Respecting the new conventions issuing from *Writing Culture* makes ethnographies harder to read, often awfully boring and difficult to mine for theoretical elaboration. In this sense, it is perhaps the mode of writing of participants in the *Writing Culture* movement (rather than the project itself) that really annoys readers such as Bourdieu. Why, however, are traditional conventions more successful than the conventions advocated by the authors of *Writing Culture*? Why does polyphonic writing have so little appeal, while the good old descriptions of the cultural phenomena, without any details about what the ethnographer feels, are still able to satisfy the majority of anthropologists, despite the criticism of Clifford and his colleagues? One answer is that the corresponding conventions are not, and have never been mystifying. They are understood to be expressive of the way ethnographies are produced and of the cognition involved in their production, and rightly so. A second, complementary answer for explaining the success of established conventions is that they favour scientific communication in one way or another. It is relevant to recount one's own adventures and situation at the beginning of an ethnography, and less relevant in the rest of the book.

One reason why the presence of the ethnographer in ethnographies is fruitfully limited to introductory chapters comes from the remarks made above: the writing of ethnography is, to a large extent, not the product of a single isolated author, but involves collaborative thinking. In anthropology, collaborative thinking is most apparent in departmental seminars. What anthropology *is*, as Spencer (2000) shows, is specified, communicated and learned during departmental seminars, which often impart tacit knowledge about how anthropology must be done. Departmental seminars are places of important social interactions where ideas are discussed and where cognition is distributed among the participants. In seminars, critiques include questions of clarification, attempts to make the anthropological literature relevant (for theory, regional studies, or the speaker's own field data), theoretical pointers about how to

deal with the data, and suggestions that help either in applying theories in order to understand events or in using the data to formulate or refine theories. This latter type of comment shows that anthropology always consists of engaging in dialogue with other anthropologists, not only with nearby colleagues but also with the discipline's classical authors. When writing, one addresses the scientific community. Often, one attempts to answer questions previously asked or to refute or confirm arguments made by others. With reference to quantum mechanics, Mara Beller (2001) has drawn attention to and analysed this dialogical aspect of scientific practices.

Ethnographies are not written in the field. They are written in an academic setting – back at the ethnographer's home university, where he reflects on his fieldwork experience. He reflects by entering into a critical dialogue with both the literature in anthropology and with colleagues. This dialogue is so intrinsic to the writing process that it is possible to say that the cognitive processes of writing ethnographies are always distributed among anthropologists. It is this reflexivity and the process of distributed critical thinking that renders the reference to the ethnographer in the field irrelevant, if not misleading, in the main part of ethnographic texts.

Ethnographies are not facsimiles of some unrealistic scientific ideals. They are to be understood as a means of scientific communication. So the question concerning the presence of the ethnographer in the main part of any ethnography is: what more could be communicated by providing further autobiographical details? The answer is that further information about the ethnographer is irrelevant, because ethnographies are meant to be answers to questions about cultures asked by members of the community of anthropologists. Simply said, ethnographies address an audience that is presumably not so very interested in the life of the ethnographer.

Admittedly, however, the absence of the ethnographer conveys a sense of scientific objectivity, but this conveyed meaning is mystifying only to the extent that science and

objectivity are understood in an ultra-positivistic way. I doubt that this is usually the case. Only positivist philosophers ever thought that scientific descriptions could *be* pure data unspoiled by the person gathering it. For practising scientists, objective knowledge is, rather, knowledge that has gone through the process of critical assessment and that has been confronted with the ideas of other scientists. Objective knowledge is not subjective impression or opinion, because the authors have thought twice and had the help of others before writing. The hypothesis that anthropologists do not tend to interpret the absence of the ethnographer in ethnography as a sign of ultra-positivist objectivity seems to be more plausible than the idea that they were mystified before *Writing Culture* appeared in libraries.

Clifford also remarks that (classical) ethnographies do contain a section dedicated to autobiographical information (e.g., 1982, 1986b). While the bulk of their descriptions do not refer to their fieldwork experience, ethnographers often begin their ethnographies by describing how they arrived in the field, under which conditions they lived, how they interacted with the natives, and so on. Clifford and other authors of *Writing Culture* see in that textual convention yet another rhetorical way of imposing scientific authority (e.g., Crapanzano 1986; Rosaldo 1986). The underlying meaning of the autobiographical section is, they suggest: 'I have been there, so I know'. If many anthropological readers have found the ethnographer's claim to have 'been there' so convincing, one may, once again, pose the following question: is it really because it is mystifying, or is it simply because it is convincing? And since ethnographers make the effort to write this autobiographical section and the readers make the effort to read it, would it not be because it is in some way relevant?

The autobiographical section of an ethnography describes the conditions under which the ethnographer has interpreted indigenous people's behaviour. It provides insight into the cognitive processes of the ethnographer in the field, which he puts to work in living and socializing with indigenous people and which eventually furnish the most fundamental thoughts for writing ethnographies. Knowing the conditions under which the ethnographer has

produced these fundamental thoughts and formulated his basic interpretations of indigenous people's behaviour is highly relevant to the academic reader. By revealing these conditions, introductory information about the ethnographer's arrival and experiences in the field enables the reader to assess the ethnographer's ability to interpret. The autobiographical section, therefore, has a function similar to the acknowledgement section: it reveals the cognitive processes contributing to the production of the ethnography; it is a trace of, and a reference to, the making of ethnographies.

But what are these cognitive processes? How do ethnographers benefit from their time as participant observers and draw information for writing their future ethnographies? One needs to answer such questions in order to understand the relationship between ethnographies and the particular experiences that ethnographers have had in the field. But while sociology and cognitive science of science provide some insight into reflexive processes in the academic setting, the literature in science studies says nothing about participant observation. In anthropology, there are manuals on participant observation, rich in methodological advice on practical matters, but the thinking processes of the participant observer are normally not addressed. The authors who have contributed most to our understanding of what happens in the head of the ethnographer are Maurice Bloch (1991, 1998) and Dan Sperber (1996: chap. 2). I will now briefly review their ideas in relation to *Writing Culture*.

Cognizing Culture

Bloch's (1991) reflections on participant observation begin with a radical critique of the role usually given to verbal statements in anthropology. What is said is important to the extent that it gives some information about what is thought by the indigenous people, but the connection is far from immediate. What is said does not simply reflect what is thought. This is so for two reasons: first, the cultural knowledge of the indigenous people is not necessarily 'language-like'. Bloch points to the importance of tacit knowledge, and draws on connectionists theories

in cognitive science, which assert that mental representations do not have the same structure as language. Non-linguistic knowledge can be rendered into language, but this process changes its character. Therefore, most of the informants' discourse will be post-hoc rationalizations and should be considered as such.

Bloch's contentions go strongly against the method of polyphonic writing. He argues that what people say is just one aspect of their behaviour and that reporting their statements in isolation can be misleading. What can the ethnographer do if the situation is as Bloch describes it? The answer lies, according to Bloch, in participant observation, since long-term presence within a community enables the ethnographer to acquire this cultural, non-linguistic knowledge:

I believe that anthropologists who have done prolonged fieldwork have always obtained the basis of their knowledge about the people they study from informal and implicit co-operation with them, whatever they might have pretended. I am fairly sure that the way I proceed in giving an account of the Malagasy culture I study is by looking for facts, and especially for statements, that *confirm* what I already know to be right because I know how to live efficiently with these people (Bloch 1991: 194).

Thus, according to Bloch, the rendering of this cultural knowledge in ethnographies is based on introspection. Once the knowledge is acquired by the ethnographer through continuous and intimate contact with those whom she studies, it is possible to retrieve this knowledge through introspection and express it in written words.

I suggest that a specific cognitive ability is at work when 'cognizing culture': mind-reading, which is the ability to attribute desires, beliefs and intentions to others. The consequent account of the ethnographer's cognitive processes is, then, somewhat different from Bloch's. Rather than using introspection to retrieve and express the knowledge acquired during participant observation, ethnographers use the knowledge acquired in the field to

inform their mind-reading abilities in order to formulate better interpretations of, and accounts for, the behaviour of indigenous people (including oral communication).

Much of participant observation consists in making sense of other people's behaviour. Attributing beliefs, feelings and inferences to other people is a day-to-day cognitive practice for the anthropologist observing and interacting with the people in the field. Therefore, exploring the cognitive bases of the practice of participant observation requires giving an account of the cognitive processes that enable anthropologists to attribute beliefs, intentions and feelings to others, and, thus, to make sense of their behaviour. The ability to ascribe mental states to others is, in fact, shared by all members of the human species and is used successfully everyday in making sense of other people's behaviour. When dealing with the simplest of social interactions, we constantly reason about other's thoughts, motives and feelings. Mind-reading is also put to work in understanding what speakers mean by what they say (Sperber and Wilson 1986). Anthropologists' communication is no exception and also involves mind-reading. In seminars in departments of social anthropology, it is not uncommon to hear someone ask, 'what do you mean by ...'. It is a request for further specification of what the speaker had in mind, what exactly he wanted to convey.

Participant observation, I contend, relies quite fundamentally on the specifically human ability to ascribe mental states to others, viz., mind-reading. The mind-reading ability, also called Theory of Mind, has been much studied in cognitive psychology (see, e.g., Nichols and Stich 2003). There are, incidentally, different accounts of introspection in the psychological literature: according to the 'theory theory' of mind-reading, introspection is the application of mind-reading abilities to oneself – in which case introspection would have a less important role in ethnographic thinking than Bloch has hypothesized. According to the 'simulation theory' of mind-reading, we attribute intentions, beliefs and desires to others by 'imagining' we are in their situation (putting oneself in their shoes) and then retrieving the result through introspection. From this perspective, introspection is indeed a cognitive process at the heart

ethnographical thinking, but it serves, I maintain, not to retrieve tacit knowledge but to make sense of others by simulating what they might think and feel.

Saying that cognition in the field implicates human cognitive abilities is not a strong assertion; it is merely a reminder that ethnographers are, after all, human beings. What is of greater consequence is the assertion that one particular cognitive ability – mind-reading – is central to participant observation and at the heart of ethnographic thought. The use the participant observer makes of his/her mind-reading ability constitutes a central characteristic of anthropology. Once the pervasive and inevitable role of mind-reading in ethnography is recognized, new epistemological questions arise. Can we trust our mind-reading abilities when doing anthropology? Under what conditions? If mind-reading is as pervasive as has been claimed, what conception do we have, or should we have, of the ethnographer as a data collector? With Bloch (1991), I maintain that the acquisition of tacit, cultural knowledge is necessary for understanding and accounting of other cultures. Contrary to Bloch, I do not think that the culture is absorbed in the brain, and then exposed in ethnographies through introspection. What happens, rather, is that cultural knowledge informs mind-reading. Mind-reading abilities are more reliable when the person has learned how people normally behave, how people usually express their beliefs, feelings and desires, and about the circumstances of their lives, all of which provide the ethnographer with information about what is considered to be important – what is at stake.

The traditional justification for fieldwork is that the fieldworker can testify: he has observed, and he can tell his community what he has seen. *Writing Culture* shows that this traditional account does not stand up under close analysis. For instance, Clifford states: ‘The specific accounts contained in ethnographies can never be limited to a project of scientific description so long as the guiding task of the work is to make the (often strange) behavior of a different way of life humanly comprehensible’ (1986: 101). I suppose that what he means is that there cannot be a description that does not deeply involve ethnographic thinking,

interpretation, or mind-reading cognition. The ethnographer necessarily and automatically put his mind-reading abilities to work in order to make sense of indigenous people's behaviour. I also hypothesize that mind-reading abilities attain a significant degree of reliability only after minimal enculturation. Consequently, fieldwork is not bound merely to impartial, external observation; fieldwork is understood as social and cognitive training for developing mind-reading abilities appropriate to the cultural environment. One can, indeed, dispense with a naïve view of testimony, understood as the presentation of data unspoiled by interpretation, as the participants in the *Writing Culture* movement argue. There remains, however, the possibility of rethinking the ethnographer's testimonial work as warranted by the cognitive training of his mind-reading ability that is provided by long-term fieldwork. This, in turn, also makes it possible to rethink the process of attributing authority to the ethnographer. In *Writing Culture*, Pratt writes as follows:

Fieldwork produces a kind of authority that is anchored to a large extent in subjective, sensuous experience. One experiences the indigenous environment and lifeways for oneself, sees with one's own eyes, even play some roles, albeit contrived ones, in the daily life of the community. But the professional text to result from such an encounter is supposed to conform to the norms of a scientific discourse whose authority resides in the absolute effacement of the speaking and experiencing subject (1986: 32).

My point, however, is that the subjective, sensuous experiences, while being part of the cognitive training of the participant observer, are irrelevant as such. Most of the time, they do not even need to be conscious and can rarely be expressed linguistically. What is important is that they inform mind-reading, which can then result in plausible interpretations of indigenous people's behaviour. It is these interpretations that will constitute the core of any given ethnography. The effacement of the speaking and experiencing subject is a consequence of a double cognitive process. To begin with, mind-reading makes a detour around personal

feelings (that have previously informed it) and automatically provides interpretations of other's behaviour; then, following that, distributed critical thinking comes into play during the writing up of ethnographies. However, the autobiographical introduction in ethnographies remains relevant, because it informs the reader of the conditions under which the author has trained his mind-reading abilities. It is valuable information for evaluating the ability of the ethnographer to provide a reliable interpretation of indigenous people's thoughts and desires. To my regret, there is little work in cognitive psychology on the role of enculturation on mind-reading (but see Lilliard 1998, along with the comments; and Chiu et al. 2000). In ethnographic fieldwork, the process of enculturation is experienced by adults, rather than by the children who are born into the community under investigation. Fieldworkers learn through participant observation, that is, through actual social interactions with indigenous people. Could the autobiographical, self-conscious work of postmodernist ethnography help in analysing how enculturation informs mind-reading? If so, it would contribute both to cognitive psychology and to our understanding of ethnographic cognition. We might, for example, gain insights comparable to those provided by Ellingson (1998), who shows in great detail how experiencing fear and pain is a necessary prerequisite for understanding the behaviour and thoughts of those others who have experienced similar fears and pains.

Finally, let us reflect on the role that mind-reading abilities have or should have in ethnographic and anthropological cognition. This is what Sperber does in 'Interpreting and Explaining Cultural Representations' (esp. 1996, chap. 2; but the idea is present in all of his criticisms of interpretive generalizations). Sperber recognizes the necessity of interpretation in anthropology, but wonders about the extent to which anthropological explanations should be informed by it. Interpretation is necessary, because it is the means of making sense of others' behaviour: 'we are all producing explicit interpretations when answering questions such as: what did he say? What does she think? What do they want? In order to answer such questions, we represent the content of utterances, thoughts, or intentions by means of

utterances of similar content' (Sperber 1996: 34). I have called this kind of interpretive activity 'mind-reading'. Sperber agrees that mind-reading should enter into ethnographic thinking. His doubts concern the extension of interpretation or mind-reading into domains that are beyond their natural grasp. If the cognitive function of mind-reading abilities is to interpret people's behaviour, can interpretation be applied fruitfully to other phenomena? Sperber concludes as follows: 'In anthropology, however, what gets interpreted is often a *collective representation* attributed to a whole social group ... The lack of a clear methodology makes it difficult to evaluate, and hence to exploit, these interpretations.' In other words, reading the minds of individual people is generally reliable, but attempting to apply mind-reading to social groups can lead to nonsense: whole groups do not really have minds that can be read.

My goal is not to tackle the epistemological and methodological problems that Sperber raises, but to show that opening descriptive analysis to the cognitive processes involved in ethnographic production gives rise to new and interesting questions and addresses some of the concerns of the authors of *Writing Culture*.

In a departmental seminar, held by Prof. Richard Rottenburg at the Martin Luther University (Spring 2005), Akira Okazaki asked the participants why they had chosen to study anthropology. Most of them answered that they wanted to discover other cultures, but one student answered that he wanted to know himself better. This was a departmental seminar, and, as we now know, departmental seminars are contexts within which anthropology is continually defined and redefined. Okazaki was very pleased with this particular student's answer; and, indeed, it is possible to view anthropology as a way of delving further into self-consciousness. One post-modern trend is to understand anthropology as a therapeutic enterprise directed largely towards achieving self-awareness. From this perspective, going abroad for fieldwork may be comparable to lying on the psychoanalyst's sofa. I have argued, however, that this is not the understanding of anthropology that is proposed in *Writing*

Culture. Going beyond *Writing Culture* implies continuing the reflexive project that is advocated by the authors of that book in order to produce better knowledge about social and cultural phenomena. One fruitful way to continue this project, I have suggested, is to go beyond the theoretical limits of *Writing Culture* by studying anthropological cognition.

Clifford already noted in *Writing Culture* that ‘Much of our knowledge about other cultures must now be seen as contingent, the problematic outcome of intersubjective dialogue, translation, and projection.’ (1986b: 109). Doing cognitive anthropology of anthropology means engaging the details of these contingencies, which implies analysing the cognitive practices of ethnographers. I have suggested that anthropological cognition is human cognition (i.e., is based on the fundamental abilities that characterize the human mind) in the cultural milieu of anthropology. I have identified two cognitive processes at the heart of ethnographic cognition: first, the distributed cognitive processes of critical reflexive thinking and, second, mind-reading, the basis for interpreting culturally situated events that include human behaviour. Of course, there are many more cognitive events that are implicated in the making of ethnographies. For instance, mind-reading is also at work during social interactions among social anthropologists, and cognition is also distributed among the indigenous people. However, the cognitive processes that I have specified play a role that is reflected in some rhetorical aspects of ethnographies. The ethnographer appears in ethnographies at the beginning, because this appearance lets the reader know that the author has been in position to make sense of indigenous people’s behaviour. Later references to the life of the ethnographer in the field are irrelevant to most readers, who want to know about the cultural phenomena being investigated. I therefore contend that this rhetorical convention results from social anthropologists’ belief that participant observation is the best means to understand indigenous people’s behaviour. Drawing on cognitive psychology, I have argued that what really grounds such beliefs is the fact that ethnographers’ mind-reading abilities, which they put to work to

make sense of indigenous people's behaviour, become more reliable after some kind of enculturation.

Ascribing beliefs to social anthropologists – the indigenous people of the reflexive enterprise in anthropology – is a way of making sense of some of their cultural practices, such as the ones implemented in writing ethnographies. For instance, I hypothesize that social anthropologists are keen to have at least some minimal information about the distributed reflexive processes to which the ethnographer's ideas were subjected – which is one of the reasons why acknowledgements are often included in books and papers. But my main contention in this paper is that the reflexive enterprise that was begun in *Writing Culture* is best pursued by developing further the cognitive anthropology of anthropology, which implies gathering information about anthropologists' behaviour. And, by the way, did you read the acknowledgments that I wrote?

Notes

1. I thank the editors of this volume for their open-mindedness to a not very orthodox view on *Writing Culture*. Olaf's invitation to participate in the workshop 'Beyond Writing Culture' forced me to consider seriously the work of the *Writing Culture* movement, and post-modernist anthropology. I am very grateful to him for that, as well as for his encouragement and editorial work. Ironically, it is he who reminded me to include acknowledgments (a case of distributed cognition). The paper benefited from comments at the 'Beyond Writing Culture' workshop (Max Planck Institute for Social Anthropology, Halle (Saale)), and from comments during an informal presentation at the Institut Jean-Nicod, Paris. The comments and criticisms of Dan Sperber, Hugo Mercier and Anikó Sebestény have been most useful. The general project of doing a cognitive anthropology of anthropology began during my stay at the Max Planck Institute for Social Anthropology in Halle (Saale), and I benefited from numerous discussions with the fellows. It is, however, Monica Heintz who enabled me to get an insider's view into ethnographic and anthropological practices. I tend to think that she is the one to blame if my views are erroneous.
2. By 'indigenous people' I mean the people belonging to the culture being studied. 'Indigenous people' has no further connotation in my usage of the world. It can be people in an African small-scale society,

stockbrokers in New York, scientists in a lab, etc. In the anthropology of anthropology, the indigenous people are the anthropologists themselves.

3. This methodological point is distinct from the beliefs that scientific descriptions are *de facto* independent from values.
4. His *Esquisse pour une auto-analyse* includes a warning that takes the whole second page of the book: ‘Ceci n’est pas une autobiographie’, and the first pages insist on the aim of the scientific, sociological, objectivation of oneself. Yet, Bourdieu recalls in detail his past in the boarding school, at the École Normale Supérieure; his talks with his father; and includes sentences such as ‘I have to confess that ...’, or ‘I remember very well that ...’
5. Absent from Clifford’s analysis of the rhetorical means through which Malinowski’s authority is established in *Argonauts of the Western Pacific*, remarks Roth (1989: 557), ‘is what makes a claim to authority socially acceptable’. The analysis of what is communicated by rhetorical conventions should take into account how the readers react to the conventions in question. This is what I will now attempt to do.

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