

## Puttings things into words. Ethnographic description and the silence of the social

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**Abstract** The article defines a new referential problem of ethnographic description: the verbalization of the “silent” dimension of the social. As a documentary procedure, description has been devalued by more advanced recording techniques that set a naturalistic standard concerning the reification of qualitative “data.” I discuss this standard from the perspective of the sociology of knowledge and replace it by a challenge unknown to all empirical procedures relying on primary verbalizations of informants. Descriptions have to solve the problems of the voiceless, the silent, the unspeakable, the pre-linguistic, and the indescribable. Ethnography puts something into words, which did not exist in language before. To respond to this task, descriptions have to turn away from the logic of recording and develop into a theory-oriented research practice

**Keywords** Conversation analysis · Description · Ethnography · Qualitative methodology · Writing

*What can be said at all can be said clearly, and what we cannot talk about we must pass over in silence.*

L. Wittgenstein

### Ethnographic writing<sup>1</sup>

Some 30 years ago, the topic of this article could still be paraphrased in a casual remark like the following: “What does the ethnographer do? – he writes” (Geertz, 1973: 28). Ever since the 1980s, however, this easy attitude has given way to

<sup>1</sup> This paper has been discussed with many colleagues in Germany, especially with Klaus Amann and Jörg Bergmann. The hard job of bringing its meaning into the English language was done by Michael Liegl (New York) and Roxana Preda (Edinburgh). Many thanks to all of them!

controversial debates on the status of ethnographic writing. While up until that time discussions of methodology had been limited to the relationship between researcher and field, the focus shifted to what was called “the rapport with the reader” (Wolff, 1986: 350). Following the publication of *Writing Culture* (Clifford & Marcus, 1986), numerous monographs on this topic were published in the 1990s, by Clifford (1988), van Maanen (1988), Geertz (1988) and Atkinson (1990); the decade also saw the publication of readers edited by Sanjek (1990), James, Hockey, and Dawson (1997) and Ellis and Bochner (1997), among others. Ethnography became the first research strategy within the field of empirical social research to have the privilege of being textually deconstructed.

Under the title of “crisis of ethnographic representation” (Berg & Fuchs, 1993) the authors dissected ethnography as a literary genre. But if we look at their contributions from a praxeological perspective, we find that they define a new referential problem of ethnographic writing: the “authorization of the author,” that is, the setting up of a speaker position meant to persuade readers that the text is a valid representation of culture. Then the question is: how can absent readers be persuaded of the authenticity of a report? Among the answers that were offered, were a naturalistic rhetoric of authentication (“I was there”), a demonstration of intimate connoisseurship, and a monological displacement of native voices. Deconstruction came into play only at the “end” of the writing process. It dealt with the ethnographic monograph as finished product, problematizing writing from a position quite remote from the praxis of field research, and focused mainly on epistemological and political issues in the context of anthropology’s colonial past.

In contrast, the older approach of ethnological and sociological introductory textbooks looked at ethnographic writing with hardly any epistemological scruple as a simple craft of documentation; its referential problem was the *forgetfulness* of the observer. These older writings were not about ethnography as a literary form, but about writing field protocols and descriptions, as the characteristic type of data emerging in participant observation. However, this type of data has suffered a loss in professional recognition ever since recording technologies – such as audiotape – have allowed for ways of gathering data, which seem much superior to human observers in terms of completeness and neutrality. In today’s research, field-protocols are seen by many as second rate data which are only taken into consideration when recordings are practically impossible (see Reichertz & Schröer, 1994: 63). Why bother with descriptions, which seem mundanely humble compared to interpretive “conceptualization” and causal “explanation,” and at the same time appear hopelessly over-theorized in contrast to tape recordings?

Between the newer epistemological and the older, more skills-oriented engagement with ethnographic writing there is a huge methodological and socio-theoretical disparity, especially compared to the methodological discussions on the status of technical recordings and interviewing that have been going on among empirical social researchers. Against this background, I attempt to elucidate the powers of description by dwelling on another problem, neglected so far, for which ethnographic writing offers a solution: the “silence” of the social. My central thesis is that ethnographic writing puts something into words that, prior to this writing, did not exist in language.

The late effects of the *linguistic turn* in sociology appear to have established a new consensus: that the social essentially consists in language communication, or should best be described in linguistic terms. Within the context of social research, this is

reflected in the interview's unchallenged appeal and the strong preference for discourse- and conversation-analysis. This preference for linguistic primary data might be due to the fact that the linguistic turn amplified a discursive *bias* already prevalent in sociology as a science. While providing sociology with an affinity for the linguistic dimension of the social, this bias also caused research to shy away from anything that happens mutely, as a wordless, inarticulate, "illiterate" process.

Moving this muteness into the center of ethnographic writing means removing ethnographers from the position of *authors* or *documentary note-takers*, and highlighting them as *speakers* who are first and foremost involved in solving problems of verbalization, i.e., putting into words that which is not language. Surprisingly, this problem was never taken up as a topic for *methodological* discussions,<sup>2</sup> perhaps because the liberties that come along with its solution invite us to consider the problem merely literary – a matter of narrative style, human understanding and of self-construction (Richardson & St.Pierre, 2005).

In contrast, I want to sketch a *methodology of description*, which both in terms of research pragmatics and epistemology is clearly distinguished from a methodology of recording or interviewing. My goal is to highlight ethnographic description as a complex "cultural technique" in sociology – just like skills in SPSS, transcription, or conducting an interview. In the following section I discuss ethnographic writing as it is characterized within the canon: as a solution to the problem of memory. While for a long time description has been viewed as a simple documentation method, comparing it with current methodology of advanced recording technologies shows what description is *not*. The logic of the recording explicates the capacity of description, affirming thereby Geertz' insight that "the photograph has not only not rendered the sketch obsolete, but ... has pointed up its comparative advantage" (1988, 67). In the following I replace the "memory problem" with the new central problem of ethnography: putting into words that which is "silent." In this section, I develop this *problem* along six different dimensions and conclude with a reflection on the limits of the sociological verbalization of the social. In the closing section I discuss what makes description special and how it can achieve an independent status as a research strategy in sociology. I argue that this status is lost the more description tries to be "documentation," it is retained the more decisively it fashions itself as a theory-oriented practice of writing.

### **The problem of forgetting: noting and recording**

Viewing ethnographic writing as a type of data rather than a literary format leads to a specification (in the sense of narrowing down) of our topic. The ethnographic monograph is a kind of literature; ethnography as a research strategy, however,

<sup>2</sup> Not even in that seminal text which characterized ethnography as "thick description." There, Clifford Geertz does not discuss the "description" part at all, but only the "thick" part of it. Ethnography, according to him, is less an issue of observation, than of interpretation (1973: 9). Thus, the ethnographer does not act as an author, but immediately as a reader, or rather a "literary critic" trying to "read a manuscript . . . written not in conventionalized graphs of sound but in transient examples of shaped behavior" (1973: 10). This is a nice metaphor, but how should one read something that has not already been described? The problem of "putting into words" is covered up when cultural reality is assimilated prematurely into the textual universe of cultural studies. Compare Amann's critique of Geertz' notion of culture as text (1997).

combines all kinds of methods of data gathering and analysis, including audio- and video recordings, interviews, analysis of documents, artifacts and conversations, etc. What is characteristic of ethnography, however, is that it brings together such heterogeneous data in an extended field trip, whose specific social form is called “*participant observation*.” It allows for the acquisition of insider-knowledge, enabling the researcher to evaluate individual types of data (such as documents and narratives) in the light of their local mode of production and of the selectivity of their investigation technique (e.g., recordings and interviews). Thus, a story told in an interview, in addition to being naively used as a *resource* complementing one’s own observations, can also be recognized as a *topic*, a discursive practice typical for the field in question.<sup>3</sup>

The specific type of data generated in *observations* are descriptions. Descriptions cannot be characterized as easily as recordings or utterances in interviews, since they are produced in a continuous shifting between various genres, in an ongoing paraphrasing process: from the handwritten *fieldnotes* via the elaborate “postscript”-protocol (written from memory) and *analytical notes* to the complete and articulate “thick description.”<sup>4</sup> Clifford (1990) distinguishes elementary ethnographic writing in three differently *localized* practices: writing down during observation situations (*inscription*), copying the wording of informants’ utterances in dialogical situations (*transcription*), and manufacture of coherent representations at one’s desk (*description*).

The first two of these aspects are well-established topics in the sociological writing on method (cf. Lofland, 1971; Schatzmann & Strauss, 1973). Here the observer’s notes are regarded as a form of *documentation* of data; consequently the discussion deals with practical hints concerning the “when,” “how,” and “what” of taking notes. For example, there is the question of how to master the task of jotting in competition with the two other tasks of fieldwork: participation and observation; or the race against fading memory: “if you don’t write it down, it’s gone!” (Bernard, 1988: 181) and then the advice that in the face of “rapidly cooling notes” (Mead, 1977: 228f) one should never wait too long before working out one’s protocols.

“Note-taking” is a basic documentation activity using primitive means (pencil and notebook). It is a mnemonic technique in three respects. Already during observation, the compulsion to write facilitates a mnemonic state of awareness: perceiving sequences in a way that allows you to remember them later and “put them down on paper.” In addition to those mental “*headnotes*” the jotted keywords stimulate

<sup>3</sup> An obvious alternative to grounding ethnography in participant observation, which can be found in the ethnography of communication, for instance, is to use participation only as a means of gaining access whereby the researcher is able to position himself or herself favorably in order to skim all kinds of data, and especially to get audiovisual recordings (Knoblauch, 2001). The recording situation in that case is not a field situation, with the task of verbalization of experiences, but an arrangement for the efficient production of data. Whether the brief visits in the field of a “*quick and dirty ethnography*” can still be called ethnography at all, is a terminological question. But if the goal is to follow long-term processes in a field at their own pace and if we are really interested in a reliable reconstruction of the participants’ patterns of relevance, it is indispensable to draw out the generation of data. In terms of terminology, I prefer a narrower notion, which takes the second part of the word “EthnoGraphy” more seriously, since the first part seems so questionable to me, that one might just as well speak of “praxeography” (Mol, 2002.) (This cannot be discussed in more detail here).

<sup>4</sup> For a distinction between these genres see Sanjek (1990); for a presentation of the whole process see the excellent textbook by Emerson, Fretz and Shaw (1995).

memories when the observer drafts protocols at her or his desktop. Furthermore, continuous writing enables collecting and archiving. Over time, a textual corpus is produced that can be repeatedly accessed like any other data storage device.

Against this background, can ethnographic writing be conceived as “documentation” in a strong sense of the word? Two arguments have been brought against this position – the first of them comes within the context of the debate in cultural anthropology. One of its prime targets was a formulation from Geertz’ article on “thick description” in which he conceived documentation not simply as a method, but in a higher sense: “The ethnographer ‘inscribes’ social discourse; *he writes it down*. In so doing, he turns it from a passing event, which exists only in its own moment of occurrence, into an account, which exists in its inscriptions and can be re-consulted” (1973: 19).

Geertz was not only attacked for his neglect of the rhetorical aspects of ethnography but also for his conceptualization of writing within a paradigm of memory. Ethnographic writing was thus oriented towards the preservation of the ethno-semantics and myths of endangered oral cultures (Clifford, 1986: 113). Only in this special case did the forgetfulness of the ethnographer and the disappearance of the subject-matter coincide in ethnographic writing in a *documentary sense*: The island in the South Seas may have long been flooded by the waters of the melting icecaps while the *Argonauts of the Western Pacific* is safely collecting dust on its bookshelf. Against such documentarism, Clifford (1990) argued that ethnography – especially when conceived as “oral history” – does not deal with ephemeral events but with pre-formulated discourses, with the participants’ *self-descriptions* similar to the ones given in interviews. Thus, he argues, it would be better not to speak of “inscription,” but rather of “transcription” or “translation.”

The second argument against the conception of ethnography as “documentation” was outlined against the background of a different type of data, preferred in conversation analysis and related microsociological research strategies. Here, it is the documentary capacity rather than the documentary purpose of ethnographic writing that is called into question. This argument refers back to a problem that is related to the forgetfulness of the ethnographer and the extinction of cultures, namely the ephemerality of the social, resulting from its being a process unfolding and disappearing in time. This challenge to sociological research was defined in an important paper on the methodology of *recording* (Bergmann, 1985)<sup>5</sup> and deserves a more detailed discussion, since many sociologists’ high hopes for the potential of audio recordings have led to a depreciation of description.

Bergmann (1985) defines tape recordings as “preservation by *registration*,” which clearly differs from “preservation by *reconstruction*” working both in everyday life and in other types of data. “Registration” preserves data simultaneously and without interpretation, while “reconstruction” happens after the fact and always already implies interpretation. Following this distinction, Bergmann argues that while field notes show some features of registration, due to the limited “recording capacity” of

<sup>5</sup> See also Bergmann (1993). Bergmann’s article is written from a conversation analysis perspective and refers primarily to *audio* recordings, as the most common practice of almost all forms of qualitative social research. By comparison, the use of the camera is a more specific practice, which is by no means more recent (Petermann, 1991), but which has not yet developed a methodology for visual data that could be compared with the one in conversation analysis. However, see Mohn (2002).

human observers, they share the following weaknesses with data derived from interviews and official statistics:

“1. These data themselves (and not only their processing after the fact) are the result of secondary processes of sense making, a fact which impenetrably eclipses the primary horizon of meaning. 2. Through these data, the social original has been transformed into the formal structures of reconstructive genres ...” (1985: 306)<sup>6</sup>

Preservation by registration on the other hand, “exclusively follows the event.” It facilitates “continuous documentation preserving the richness of an occurrence over a sufficiently extended period of time” and thus preserves “its authentic eventfulness” (312). Refraining from sense-making, “recording(s) leave the primary structure of meaning of an event untouched. By comparison, every linguistic actualization of an event *ex post* must be considered an interpretation, i.e., a re-creation” (305).

Compared with recordings, field-protocols show at least two serious weaknesses: (1) Given the forgetfulness of human observers and measured against the recordings’ high capacity for detailed documentation of ongoing processes, the documentary performance of field-protocols is quite modest. They only seem worth considering when we take into account that recordings also come at a price. On the one hand, for research economic reasons recordings can only be generated for short-lived instances of practice (i.e., they are only useful in a micro-sociological approach), and even then only under the condition that the field can be accessed with technical recording equipment. In comparison, ethnographic descriptions are less fixed on the present. They allow for the study of moves in a chain of action while keeping a long-term perspective, thus noting the connection between individual actions and their context. This is especially useful when dealing with long wave processes (e.g., court cases), which would otherwise be fragmented in the snap-shots of a recording.

On the other hand, instead of the selectivity of an observer, recordings yield to the selectivity of their media. Not even an audio-tape can guarantee the “immaculate conception” that many of its users expect from it.<sup>7</sup> It registers events at a certain point in time and from a certain vantage point; it records only certain sounds and ignores others; and, of course, furthermore, it is blind. The video-camera as well only produces excellent documents when we grant to the selected detail in the camera’s display what we would not grant to any human participant: that it is preserving what has “actually” happened. Here, field notes can provide additional observations of context, which the audio-tape does not know of and which escapes the selection of the camera’s view finder.<sup>8</sup>

(2) However, this possible complementary relationship between the recording focus and the capturing of context does not alleviate the second more severe weakness of field-notes: the “incurable” hybridization of presentation levels, the

<sup>6</sup> For example, according to the requirements of a good story, where “unimportant parts” have already been sorted out.

<sup>7</sup> Bergmann, too complains that the sociological use of audio-recordings is not sufficiently concerned with the transformative quality of its data (1985: 317).

<sup>8</sup> Linking up with the aspect of selectivity we may note a complementary relationship between recording and description: On the one hand, protocols can and must substitute technical recordings, which are useful auxiliaries and release researchers for observation, thus increasing their opportunities for description. On the necessity to consider selection in research not only as a problem of technical equipment but also as a problem of directing attention see Amann and Hirschauer (1997: 22).

contamination of data with their interpretive analysis. Descriptions remain suspicious for they cannot present the equivalent of what is called – using a set of industrial metaphors – “the raw data” of a survey: the freshly retrieved material of a “sample,” still bearing traces of dirt from the mine and of the explorer’s sweat.<sup>9</sup>

The jotted notes *fall below* this level, just like the mere impressions or memories the ethnographer brings home from the field. Both are considered proto-data at the most. The field-notes worked out at the desktop, in contrast, considerably *exceed* the level of raw-data. Compared with the apparently “naked” audiovisual reduplication (Bergmann, 1985: 301) of social phenomena, even the primitive accumulation of ethnographic knowledge always already includes additions, omissions, accentuation, turns and twists, in brief: a re-presentation.

Compared to this, recordings really do appear like an accurate copy of a “social original” – a copy, which can be considered a preservation of the “primary structure of meaning” of an event, even if it is not an exact reproduction.<sup>10</sup> But what does this standard, which makes descriptions in qualitative social research look like a “substitute to be avoided whenever possible” (Oevermann, 2000: 113) consist of? At first, we have to bear in mind that most recordings gain their documentary power only after a translation: the piece of material most often used for data-analysis and presentation is the transcript.<sup>11</sup> When put into writing, sounds at first have to be acoustically identified and isolated as individual utterances; what happens simultaneously (in the audio) has to be sequenced, and decisions have to be made by applying standardized rules about what to neglect as background noise, and how to deal with overlaps, pauses, intonation, and paralingual phenomena (Psathas & Anderson, 1990). In that sense, recordings operate with linguistic reconstructions too, namely the written reconstruction of a transcriber making sense out of sequences of sound. I could now insist on the *transcription’s* relative interpretive reticence when compared with the *description*, but I would rather leave aside this realist claim of reference to an “unspoiled” object and focus instead on the merits of the recording in the context of sociological knowledge processes.

### Excursus: Can recordings preserve an original?

Recordings and transcripts create properties for a conversation, which do not exist for its participants. For example, they register noises and sentence interruptions,

<sup>9</sup> Here we only find the already mentioned soft differentiation of genres: The word for word documentation of the participants’ statements, just like *fieldnotes*, are of course “rawer” than memos, but even for those Clifford’s formulation regarding the “raw” (1990: 58) holds: “Fieldnotes contain examples of my three kinds of writing: inscription (notes, not raw but slightly cooked or chopped prior to cooking), description (notes sautéed, ready for the later addition of theoretical sauces), and transcription (reheated leftovers?).”

<sup>10</sup> Against any naïve-realistic expectations towards recording Bergmann (1985: 317f.) explicitly calls for epistemological “caution in dealing with audio-visual recordings” and he points to a “constructive aspect.” I will come back to these issues.

<sup>11</sup> Here two kinds of differentiations have to be made, the first one regarding the form of recording: transcripts are primarily relevant for audio recordings, which make up the majority of all recordings. However, occasionally, we can also find recommendations for a “textual taming” of video-recordings through notation systems (Oevermann, 2000: 112ff.). Secondly, we find varying ways of dealing with recordings within empirical social research: Hermeneutical analyses or content-analytical interview studies are usually based on the wording in the transcript, whereas conversation analysis, which is more interested in the process of speaking, makes use of the sound- or video recordings during data analysis. Only by publication does the transcript displace the recording.

which usually stay and have to stay below the participants' level of consciousness in order to not interfere with their production of meaning. Recordings not only solve the problem of ephemerality, i.e., the fact that things happen and immediately disappear; they also function in a complexity of context of which the participants can never be completely aware. Thus, the issue is not so much that recordings should be the truest copies of what the participants *have* indeed *done*, but that from the start (i.e., before beginning with the data analysis) they *outdo* what participants could *have known* about the situation.<sup>12</sup>

Furthermore, recordings and their transcripts produce a conversation that is identical to itself. In everyday life such conversations do not exist, since it is exactly the dialogical and reconstructive mode of conversation that keeps open the possibility of a continuous re-interpretation (of what has been spoken) by the various participants. However, while being reinterpreted, events don't hold still, they wobble and change. Just imagine a "*respondent validation*" of a recording of a marital dispute. The couple reads and comments on our transcript: It wasn't the utterance, it was its connotation; it wasn't what was said here and now, it was what was said before; it wasn't so much the words, it was the tone of voice; and even if it was actually said, "it wasn't meant that way." It is hopeless. The domestic quarrel "as it really happened" never existed (unless the participants themselves actually agreed on such a "final" version).

Thus, the special quality of technical preservation does not reside so much in its capacity to make a literal copy, or in the neutral manufacture of a textual double, but in creating something entirely new: the singular, self-identical conversation. Artificial singularity, however, is exactly what makes an "original" in the literal sense of the word: the unique testimonial of a creative moment. This can be said about the achievements of the transcriber, but not about everyday social processes, whose temporality is not only momentary but also episodic, biographic or historical, and whose testimonies are almost never singular, but plurivocal and ambiguous. The transcript, then, does not produce a copy of a conversation the way it happened for its participants, but an "original" (i.e., a referent) for the discourse of its sociological observers. The social original and its "primary structure of meaning" is an artful sociological reification of an ongoing re-interpretation of time: *one* witness (the microphone), *one* testimony (the recording), *one* copy (the transcript), *one* referent (the conversation "as it really happened"). Given back to the couple in my example, this singular character would immediately dissolve again as the conversation is further de- and reconstructed.

The decisive feature of the recording is its potential for *decontextualisation*. What seems like an exact conservation within the framework of a second-by-second temporality, viewed within the time-frame of biographic or historical processes, must appear like a snapshot, abruptly freezing a continuum of constantly shifting movement. Thus, recordings deprive the participants of their power to define anew what has happened; they snatch away a "definitive version" from this continuous reconstruction of meaning. Once recorded, everything they said "can be used against them." We have emancipated our "data" from their producers.

<sup>12</sup> That is why it seems questionable when sequence analysis, (in conversation analysis and objective hermeneutics alike), in its prohibition of retrospection, invokes so strongly the standard of current participant-knowledge. This cannot count as a "gold standard" for a procedure which, by the selectivity of its medium, chronically stays below participants' knowledge, while at the same time, by the exactness of its registering, leaves this knowledge so far behind.



The social original is a sociological fiction, an idealization intrinsic to recording technologies. This fiction demands that the written representation of the recording (the transcript) be considered an “inviolable” document of such an original, *stabilizing* certain parts of one’s own text production. (In a similar fashion, another part will later be stabilized as a citable publication, as a manifestation of “intellectual property.”) This practice is conventional to the extent that it would be just as possible, when transcribing, to aim for maximum variation rather than for copies true to the original, i.e., to take all hearing experiences as seriously as alternative readings in hermeneutics are taken, and then to choose the one that makes most sense to the community of transcribers. The reason we do not do that (but rather limit ourselves to occasional changes) lies in the fact that the assumption of an original is a most *valuable* fiction (Mohn, 2002) for sociological knowledge processes:

First, recordings allow for a strict sequencing of the research process, a sequencing that clearly delimits the generating of empirical material (by *black boxing* its constructive moment) from its analysis. “Data” come into existence in the “blind spot” between those two processes. Recording functions in this sequence as operationalizations do in standardized social research, where a research operation is substituted for the phenomenon in question as the referent of sociological analysis. Analogously, here, a transcript is considered a double of the “primary structure of meaning.”

Secondly, this fiction performs a function for data-analysis. The recording stands for an “authoritative version” steadying and disciplining sociological discourse like an artificial referee. In hermeneutical analyses, for instance, without the ultra-stable text of reference, the carousel of interpretation would pull away from its anchor. Recordings create space for extensive sociological sense making, suggesting little islands free of interpretation, “a firm ground under one’s feet” so to speak, on which to build the most unlikely interpretations.

Third, this fiction fulfills an important function in the rhetoric of data-presentation, i.e., in the text-reader relationship. In publications, “raw-data” play the role of exemplary pieces of proof and evidence for a sociological argumentation. Their presentation spares texts from hermetic closure because it seems to offer to their readers a participation in data-analysis. The data document early stages of sociological knowledge processes (and facilitate their reconstruction), but they are routinely read as documents of the “field” and its “voices.” This reading is most of all due to data-presentation using the same authentication procedures as scientific communication in the author-author relationship: *citation*, i.e., distinguishing between utterance formats (quotation and commentary) as if to offer the reader a comparison between copy (quotation) and original, which would allow for unambiguous verification. Even when dealing with empirical data, this gesture usually suffices to involve the reader in the validation of texts, for two reasons: On the one hand, assertion in the form of a quotation, i.e., the representation of originals within a text, is so well established as a trust-building measure in scientific communication that an actual double-checking of quotations hardly ever happens. On the other hand, the accuracy of transcription is a cross-reference to an impressive warranty of quotation: the combination of the *high-tech* of dynamic microphones and digital recording with that old monastic practice of a faithful transcription before the invention of the printing press.

What follows from our socio-epistemic reconstruction of the performance of recordings and the idealizations intrinsic to the way they work? Recordings do not simply conserve social processes, they de- and recontextualize them in a highly complex fashion. They transfer everyday incidents from their native contexts into the context of sociological argumentation by exceeding and falling below the participants' knowledge, textually reifying a singular event, emancipating "data" from the participants' control, and establishing a stable empirical referent within sociological discourse. It remains to be shown if and how ethnographic descriptions can offer anything comparable. Two things seem already evident: 1. Both descriptions and recordings have to be acknowledged as *sociological artifacts*. 2. Compared with recordings, descriptions offer only soft forms of documentation. They are poor protocols of the second-by-second temporality of social processes, and they cannot offer readers the text-immanent professional control of interpretations. Thus, description seems to be limited to the above mentioned compensatory or residual functions, pragmatically called for by the limits of recording. The question remains, however, whether we have thus really grasped description's central function and its actual problem.

### **The problem of verbalization**

The ideal of a "transcription" of social processes, which should be as neutral as possible, serving as an orientation for the methodology of recording, is not only questionable in its epistemology. It shares a premise with that other model of description-as-translation discussed in cultural anthropology, a premise which limits the model's usefulness to an understanding of ethnographic writing, i.e., that the object already exists in some form of language only requiring translation from one language into another or from orality into writing. If ethnographic writing is conceptualized within such a framework, its specific merit is bound to remain in the dark. This merit is there, *before* the written form of transcribed recordings and even more so *before* the rhetoric of published texts: it lies in the verbalization of the social.

Sociological discourse essentially consists in written communication. Compared with texts, images play a minor role – especially in comparison to the natural sciences – or they are considered the special area of a subfield called "visual sociology." Given these conditions (which are not further discussed in this article) the core problem of all forms of empirical social research is how social reality finds *access* to sociological discourse. In most cases, we trust in the participants' primary verbalization. In empirical social research, the positive data, the "given" is almost all the time the *said*: narrations (oral as well as written), pieces of information, conversations, and discourse.

In discourse- and conversation analysis, the linguistic forms are at the center of attention: properly articulated written communication (of journalists, scientists, politicians, etc.) are all possible forms of oral exchange, as long as they can be recorded. Most forms of interviews, however, view language as a transparent medium without qualities, a window through which the informants allow us to get a direct view of their world. Given this rather naïve approach, interviews not only skim the participants' existing verbalizations, they also try to stimulate them by bringing and keeping their

“informants” in conversational situations in which they feel accountable for what they say. Interviews are verbalization methods presupposing that people have specific knowledge that can be extracted by means of “questioning.”

In this fashion, interviews seek access to past events and experiences, to the particularity of life situations, to closed-off locations and to “inner worlds” (opinions, religious beliefs, etc.). The essential problems of method are differing thresholds of verbalization and limits of the interlocutors’ willingness and ability to provide information: motivational problems, gaps in their memory, limits of knowledge, limits of reflexivity and self-awareness, affects like feelings of shame and guilt, or fear of the social consequences of speaking. Various conversational techniques try to lower these thresholds: the general set-up of the speech-situation (assuring anonymity and that whatever is said will not have any consequences, signaling readiness to listen, and a neutral openness that allows for all possible answers) and specific elicitation methods for getting access to memories, thus exceeding what is usually “available.”

However, when interlocutors fall silent, these forms of empirical social research are immediately confronted with interpretation problems. Whether it is in interviews – *non-opinion*, problems of rapport? – or in conversation analysis – meaningful silence, non-verbal reactions, recording problems? – in all these cases we have a problem if the interlocutor ceases to play ball. Now for observers this is the everyday situation of their work. *From the very beginning*, (i.e., way before the presentation of data and arguments), they are required to put social facts into their own words. Observers are placed in this position because they are also dealing with those aspects of social reality to which participants’ verbalizations can offer no access: material settings, wordless everyday practices, silent work practices, visual performativity etc. Before even attempting to create textual documents as a congealed “material basis” of empirical research, those aspects have to be linguistically liquefied first. They have yet to be transformed into that medium (language) whose textual fixation may supply research with a solid data base.

This position in which ethnographers find themselves has something embarrassing to it. While the discourse of deconstruction brings to light their literary sophistication, as field researchers they stand alone with their experiences, at a loss for words, and only equipped with the means of everyday language. Even worse, they find themselves compelled to be their own source where usually quotations from “other people” are used for the authorization of assertions. This simplest of all tricks of authorization – stepping back behind others (whose statements only “bear testimony”) – does not work when making a description. What a frightening amount of freedom! The primary practical problem of ethnographic writing lies in transferring an embarrassingly private thing such as personal sensory perceptions into the public sphere of scientific communication.

The core of ethnographic authorship thus lies in verbalization. It prompts a series of questions: what *solutions* ethnographic writing has developed for this problem; what *criteria* indicate its overcoming etc. Not all these questions can receive a final answer in what follows. I will focus on the explication of the basic socio-ontological problem that description continuously has to solve, which I call the *silence of the social*.<sup>13</sup> This problem appears in various forms. The ethnographic “front of

<sup>13</sup> Of course, this metaphor does not only refer to the everyday notion of “silence” – an interruption of speech – but to a blank in the kind of research that is restricted to verbal data; moreover, it refers to a mute challenge for description to “make something speak” that resists verbalization.

research” is delimited by various borders of verbalizability along the lines of the voiceless, the ineffable, the speechless, the indescribable, the pre-linguistic, the mute, and that which reveals itself wordlessly.

### The voiceless

Even before ethnographers are confronted with the lonely task of putting things into words, they often face the problem that participants’ verbalizations are available in principle, but for various reasons are not accessible to sociological discourse because they are incomprehensible on a basic level (1), because the field’s power relations confine their articulation (2), or because they are silenced by cultural norms which demand renouncing speech (3).

(1) First and foremost, ethnography is known for having to deal with the basically incomprehensible, with that which “tells us nothing.” The solution to this problem is sought in thick, interpretative description, which based on its familiarity with language and culture, provides information about the contextuality of phenomena. Above all, it was Geertz (1973: 16) who made ethnographic description prominent against the positivist variants of “radically thinned” behavior protocols. Offering exemplary demonstrations of general meaning structures in specific situations, description may be considered a diorama rather than a copy: The decisive representational relationship is not postulated in terms of its exactitude as a copy, as in the methodology of the recording, but rather in terms of the exemplary force that a selected situation may have for a cultural whole (as for example, the cock-fight for Balinese culture). Thick description is not supposed to preserve “the actual case” but make comprehensible *what* the case illuminates.

Incomprehensibility is a problem of description prior to the “silence” of the social, as it occurs not only in mute processes (unfamiliar practices and rituals) but also in voices speaking in foreign languages whose meanings are inaccessible to “us.” They require *translations*, whose task it is to “make available to us answers that others ... have given, and thus to include them in the consultable record of what man has said.” (Geertz, 1973: 30). A translation always relies on understanding procedures which, instead of breaking with the participants’ knowledge, demand continuous feedback: that kind of merging of participants’ and observers’ patterns of relevance gives interpretive descriptions their inevitable “hybrid character.”

(2) Another claim, which is related to the translation effort in an intercultural contact, lies in giving voice to the voiceless or the inaudible – like an acoustic amplifier. “*Voicing*” is a strategy that suggests itself wherever strong hierarchies of languages and discourses ask for a compensatory speaker position giving voice to ethnic groups or subgroups, thus enabling social mobility for marginalized discourses. Just like the translation problem, voicelessness is a form of resistance to verbalization well known from interview research. It is a question of (self-)selection of informants: Not only those with power of speech and definition should shape the portrait of a culture, but also those who are forcibly silenced.

With regard to those oppressed muted voices, the ethnographic report can claim some access advantages, compared to the short-term relations in interviews. While e.g., the expert interview explicitly targets talkative and well articulated persons who can provide information about a field, ethnographies allow insights into the way such monological speaker positions are structured within the field so that they are open to

certain “responsible” speakers but not to others. Furthermore, long-term field stays help us recognize what specific versions are presented to cultural foreigners within the conversational format of the interview (Amann, 1997). Finally, the participants’ reticence (their taciturnity, reserve, and secretiveness) is easier to deal with by means of long-term field-relations because ethnographers, on their way to becoming insiders, don’t have to be told all those things they have already acquired as shared knowledge. Against this background, the notion of *voicing* also had an influence on the selection of fields for ethnographic research – just think of all those studies following Whyte’s *Street Corner Society* which dealt with social milieus with pronounced visual and activist (also violent), rather than discursive, modes of articulation.

While *voicing* was important for internal cultural differences, it became even more so for the field’s relation to the observer, empowering the participants’ voices over and against his or her categories. *Ethnoscience* demanded that ethnography give its informants a maximum of control over the linguistic structuring of their experience: “the problem is not to understand, but to let speak” (Knorr Cetina, 1981). It is this motif of control that is radicalized in the postmodern critique of ethnographic authority. It is not only that the field’s plurivocity ought to be saved from the dominance of some particular voices in the field; more emphatically, it ought not to be silenced by the ethnographer. Hence, the requirement was that participants have a say in the foreigners’ discourse about their very own culture. The literary forms empowering the “multiple voices” of the research subjects against the monologue of the ethnographic author are for Clifford dialogic and polyphonic texts, where the subjects themselves can enter as (co-) authors. The goal is for them to advance from “mute co-authors” to real ones. This idea of co-authoring points to the fact that *voicing* too may directly connect to the voices of the field which are merely lacking the power of self-assertion. Here verbalization is still (as in interviews) essentially a *collaborative* performance of participant and observer.

(3) There is another form of silence, however, in which this opportunity for collaboration gets lost: the *unmentionable*. Here we are dealing with those things within a culture which are normatively kept just beneath the surface of language. The restraint in one’s use of language is not only enforced in conversation (as the silence of the listener) but also assumed by convention as a virtue which demands: “hold your tongue!” Those things “you are not supposed to talk about” are verbally revealed only by special speakers, especially by children, who quite unself-consciously speak about disabilities, violence, or embarrassments.

The unmentionable does not have the form of a secret – an unequal distribution of knowledge based on strict selection of addressees – but rather that of an “open secret,” open to anyone for inspection but closed off to simple thematization. In dealing with this problem, ethnographic descriptions can no longer rely on the sheer proximity to cultural events, they start being on their own and thus make appeals to “professional distance”: Ever since Robert Park we find in ethnographic literature the recommendation to pair a gaze free of illusions with laconic realism (Kurtz, 1982). The gaze should not be turned away, but rather held steadily like the camera of a war correspondent. The rule of the thumb is: do not shy away from putting something into words but refrain from morally commenting on it. Descriptions of the unmentionable are produced at an intersection of linguistic exposure and avoidance of moralizing.

Acknowledging the unmentionable as a challenge must not always mean eliminating it by description; it may remain unspoken (for example, for research ethical reasons). But it is this very option which demands fashioning the limits of what is said *within* language, e.g., by ellipsis and allusion, unfolding, say, experiences of grief, not by putting them into words (as in “*reality-tv*”), but rather by “transferring” them onto readers, omitting just enough to leave room for their own imagination.

### Speechlessness

Once observers have overcome those resistances to verbalization that are produced in the power relations and cultural norms of the field, they are inevitably confronted by those resistances having to do with their own person. While the advocates of recordings reject subjectivity as a resource of data gathering, ethnography embraces the person of the researcher as its primary instrument for data gathering. I have already dealt with his or her forgetfulness in my discussion of recordings. However, greater importance has to be assigned to the possibility of becoming speechless, suffering from a situated “aphasia” that occurs when observer “lacks words” or when certain experiences leave them totally non-plussed.

When dealing with such elementary problems of verbalization, the participant observer’s “disciplined subjectivity” (Wolff, 1999), demanded with good reason from all qualitative research, cannot orientate itself towards the ascetic exemption from the subjective, as claimed by the recording ideal. Instead, an active involvement would be more appropriate, making fruitful use of the lower selectivity of the senses characteristic of the human “recording device.” The term “observation” carries the misleading objectivist connotation that research object and research subject are clear cut, distinct entities. Participant observation, however, is always also essentially “self-observation,” i.e., something that doesn’t necessarily involve eyes looking from a distance. Activating lower senses and inner experiences of the body opens up a heightened form of receptivity, directed towards the object by means of what one’s own body is undergoing. Psychoanalysis has an interesting model for this kind of receptivity when it tries to register the affects of the object by *mirroring* them seismographically in the mute corporeal sensations of the analyst. Analysts gather data via self-observation, allowing uncensored experience of corporal sensations, fantasies, and feelings and then (after therapeutical selection) communicating these to the patient. Psychoanalysts put into words the assumed “inner conflicts” of their interlocutors (which are often expressed only in physical symptoms), by first *inducing* verbalization problems *in themselves*.

In a similar fashion, participant observers turn silent social phenomena into their own speechlessness. Hence, they also run into the embarrassing situation of using language to track down merely intuitive knowledge or vague feelings of their own. The challenge of speechlessness for ethnographic writing lies in fathoming which perceptions can be made at all accessible to scientific communication.<sup>14</sup>

In dealing with speechlessness, ethnography offers particular spaces of verbalization, niches for the development of the essential ethnographic “discipline of subjectivity,” which is – writing. The hasty jotting down in the field is already a

<sup>14</sup> This problem finds written expression in the communicational privatism of *fieldnotes*: Since their first addressees are those who write them, they are usually illegible for others, incomprehensible and highly indexical; examples for this can be found in Sanjek (1990).

practice which distances writers from the immediacy of their own experience and distinguishes them from what is relevant to the participants, leading some ethnographers to denounce the “treacherous” appeal of writing (Lofland, 1971: 108f.). The reason for this loyalty problem lies in the fact that writing down is already a *withdrawal* from the practice one has been immersed in. Just arrived, one already retreats again into a highly self-absorbed activity.

The retreat to writing (in the field) is followed by the retreat to the scriptorium (of the office) where verbalization without competition is not only allowed, but demanded. This privacy gives room to an often socially inappropriate practice: endlessly and ruthlessly asking questions, which makes it easy to evade common sense. Furthermore, many observers cultivate the field diary as a special genre and space of writing, analogous to talking to oneself. It can contribute to the maintenance of the person as research instrument by offering textual niches where writing is used to get cumbersome moments of the field off one’s chest.<sup>15</sup> Soon, this preliminary relief from publicity is challenged by one’s colleagues, who are the first audience: a chosen group of listeners, who loosen the ethnographer’s tongue with animating questions and conceptual suggestions. The scriptorium is followed by the parley.

In each of these spaces, personal experience is socialized by forcing it into language. Nevertheless, the relationship between experience and language is of course not only “expressive.” Experiences are preformatted through various codes, they occur already structured by all those categories the available languages (i.e., of everyday life, of the field, of the discipline) allow for (semantically and grammatically). Moreover, they are edited following narrative patterns (Clifford, 1986) and the rules of communicative genres (Bergmann, 1985: 306). Those structures of language, however, do not suffice for solving the verbalization problems faced in the practice of speech. Describers, struggling with their own speechlessness are less in the situation of a narrator, retrospectively reconstructing a subject matter, and more like a *taster* who has to use categories situatively and deliver them to others. The words they are looking for are on the tip of their tongue – just like a wine one is trying to describe. Wine-connoisseurs or tasters in the food-industry (Méadel & Rabeharisoa, 2001) do not have to invent language anew, but they have to develop a vocabulary, i.e., apply words to new objects in order to make new experiences shaped by differentiating categories, and, additionally, share these new experiences with others. Words are basic analytic instruments for this.<sup>16</sup>

In a similar fashion, ethnographic observers have to transform whatever they perceive through their senses (what ears and eyes “tell us”) into speech – a completely different form of signification. This process is reinforced when verbalization takes the shape of the *written* because the urge to put something down has an interesting impact on perception. On the one hand, together with the already mentioned mnemonic state of consciousness, it supports the objectivation of the perceived. Just as novelists register their lives as subject-matter or photographers use their environment as their “subject,” ethnographers who are compelled to write view their field as empirical “material.” On the other hand, already crafted protocols

<sup>15</sup> Since the 1990s however, this genre has also been taken up by a number of published “auto-ethnographies” (Ellis & Bochner, 1997).

<sup>16</sup> Méadel and Rabeharisoa (2001) show that the development and selection of hundreds of descriptors is necessary – can orange juice taste “metallic” or “green”? – before it is possible for a “collective body” to have such corresponding experiences.

provide a template for further observations: they structure and focus whatever is observed during the next day. The categories used have already conceptually decomposed the *gestalt* of impressions and memories or the sense synesthesias. The “verbatim” of the spoken and the “literality” of the written create an analytic relationship to one’s own sense perceptions.

### The indescribable

Another problem of sociological verbalization is the *indescribable*, i.e., whatever resists the simplification of description due to complexity or polyvalence. There are two major reasons for the problem of complexity in description. The first can be located in the weak selectivity of its medium: While its “recording device” is already open to a manifold of sense perceptions, its storage medium – language – has a much lower selectivity compared to a tape-recorder or a camera. Description is not reduced to verbalization the way a camera is limited by what can be seen through the viewfinder, or the way a tape recorder is bound to the sounds of the field. The medium of recording overlaps the articulation of an event; the medium of description does not. It is only against this background that the necessity to focus – stressed so much in certain ethnography textbooks (e.g., Spradley, 1980) becomes comprehensible: descriptions have to be highly selective in their *choice of topic* precisely because their subjects are ontologically less restricted.

The second essential reason for indescribability derives from the temporality of social processes. Their eventfulness makes it hard for the ethnographer to come up with “the right words at the right time.” This temporality has two aspects: one is the simultaneity of social processes; the other is the already mentioned ephemerality of the social, which conditions the methodology of recording, and whose fast pace inevitably leaves language lagging behind. I have already shown that descriptions can deal with larger units of time than technical recordings. But when dealing with problems at the micro-sociological level, the fleetingness of events is the decisive factor for the deployment of technical recording media. According to Bergmann, recording primarily allows for a temporal *re-organization* of the fleeting event (1985: 318), a manipulation of its temporal structure. Audio-recordings and transcripts are “time-machines”: They enable us to preserve the temporality of an event and to modify it (Bergmann, 1985: 304). The manipulative moment resides in two kinds of interventions: *fixation* (the “stopping” of time), allowing the inspection of “the same” (constituted by this fixity) repeatedly, and a *prolongation* of time through a written unfolding of events.

On the other hand, within the capabilities of the description of ephemeral micro-processes we could name some analogies to the capacities of technical recordings. The fixation and prolongation of real-time events is again backed up in various respects by the written format used for verbalization. Oral reproduction often fails to solve the problem of the indescribable, although writing can attack it in a piecemeal fashion: The opportunity for repetition in the inspection of a recording corresponds to the compulsion for repetition in inscription. The time-consuming writing process does not simply “preserve” an experience (as a mnemonics technique); more importantly, it slows down its processing. In writing, one focuses on an event anew, thus intensifying the memory of it. Memorizing by writing is a kind of “rumination.” Lofland (1971: 104) already pointed out the advantages of deceleration in



note-taking: “While talking rather than writing saves time, it also removes one from really having to think about what has happened and from searching out analytic themes.”

A second aspect in the fixation of ephemerality is that experiences only *materialize* in writing. Notes not only *stimulate* the process of remembering, they also *discipline* that process, providing unalterable written documentations of past events, thus limiting their possible ways of reconstruction. While note taking activates short-term memory, the notes themselves correct long-term memory. That way, writing enables an experience to resist theoretical interpretations after the fact. Inscription fortifies the genre of the *fieldnote* against that of the *analytical note*; the voice of the ethnographer, still close to experience, against the sociological tone of voice she or he would take later. However, the level of resistance to theoretical interpretations in protocols is much lower than that of recordings. This is not only due to their intrinsically interpretive character, as discussed earlier, but also to the fact that they often are explicitly made for a later editing process. The materialization of ephemerality does not so much correspond to the freezing gesture of the “once and for all” but rather can be viewed as another deceleration or a “thickening” of the process.<sup>17</sup>

A third aspect of the fixing achieved in writing is further removed from the logic of recording. Recordings *intensify* the problem of ephemerality, which they are designed to solve, insofar as they are usually applied to short term field stays at the end of which the events simply “disappear” for the *sociologist on the run*. Unlike such a “hunt-for-audio” (Bergmann, 1985: 299) the sociologist in a long-term field stay can rely on the way social structures of meaning are stored in the participants’ routines of everyday life practices. Hence, there is not so much a need for breathless “hunting” as there is for patient “gathering.” Thus, the singular recording of “exactly the same” which can then be repeatedly “observed” (i.e., read as a transcript) can be substituted by the repeated observation of “something of the same kind” (of a typical scene, of an interaction-ritual etc.), which then becomes the subject of ongoing description. Instead of the meticulous analysis of material oriented at linguistic methods, descriptions need an ethological patience in situ. (Many examples for this can be found in Goffman’s work.) Here, a displacement of consequence occurs: Like the reliving of experiences in the course of inscription, the repeated observation is not an exact repetition, but rather a blurred image. Hence, the notion of an “original” disappears for two reasons: First, a good description needs a license to group situations into types as well as to fragment and recompose observational units.<sup>18</sup> Secondly, in the place of a privileged recording position, a rich description has to bet on a variation of perspective, actively following its objects around through various localities and observing them from varied angles. Ephemerality is gradually encircled rather than suddenly taken hold of.

The distance to the logic of recording increases even more when we look at the aspect of *drawing out* real time temporality. In principle, fixing time within the

<sup>17</sup> In ethnography, there are various ways of dealing with protocols that bring them closer to the status of an inviolable “original document” or to that of “literary subject matter,” which can be reworked for presentation purposes, or even rewritten. We could draw the following conceptual distinction: A subject-matter is made up of general literary themes and motifs which are given shape by individual design. Empirical materials, however, already have an idiosyncratic shape, and detailed structures of their own. They can be ordered and rearranged, but not created anew.

<sup>18</sup> It is just this feature which makes description nearly useless for the reconstruction of conversation sequences.

linearity of a text causes volume to grow: you have large amounts of text for small units of time. In transcripts, however, the expansion is “dictated” by the precision of the recording (and by transcription rules). In descriptions it is only encouraged by a criterion of excellence: its *richness of detail* (see Emerson et al., 1995: 68ff.). Given the standards set by transcripts, this criterion ought to be emphasized even more. The condensation of events in descriptions has to make use of the “slow-motion effect of language”: utmost exactness and conceptual dissecting of tiny details. But how does a description get its details? If we do not want to mystify description as some kind of special “art of perception” ascribed to the observer, then we have to take into account the role of writing and the recursive logic of ethnographic research once more. Descriptions mobilize functions of the research process, which in the methodology of recording are reserved for “later phases.”

I have already explained that observers’ compulsion to write has an effect on their perception. They perceive in greater detail when they have to figure out word for word what exactly is going on and when the analytic categories of already formulated protocols start to pre-configure what they perceive right now. Descriptions have an evolutionary, or progressive, character. This is why the strict separation of data gathering and analysis, which is crucial for the methodology of recording, does not make sense in their case. By way of constant writing, the “data analysis” in ethnography already pre-configures the process of perception.<sup>19</sup> Writing provides close continuity between the phases of the research process. The composition of *fieldnotes* already is symptomatic for this aspect of ethnographic research: it does not simply happen in a distinct “phase of data gathering,” but in moments where participation, observation, and writing alternate and constantly interrupt each other.

This intertwining of research functions continues until publication. Describing is a communicative act, and therefore – unlike recordings – descriptions have to take into account their *addressees*. Geertz said that descriptions ought to invoke their objects in the readers’ imaginations, take them into the heart of a scene, and involve them into “the whole vast business of the world” (1973: 18). (This is certainly more important than the rhetorical effort to convince them that the ethnographer “has been there.”) In this sense, descriptions have to provide a two-fold “translation”: from lived experience into language and from texts into imagined experiences. In the end, the repetition that settles the fate of a description happens on the side of the reader.<sup>20</sup>

Protocols have to convey an event in such a way that they not only help observers to remember, but also allow any other reader who was not present to follow them there. Whereas listeners may stop an oral narrator and request further details, the solitary writer has to do this on his or her own, by imagining an anonymous or

<sup>19</sup> This could also be put in another way: Field and text are not clearly demarcated zones; in many places they jut out in each other’s territory. Clifford (1990: 66) rightly objected to the concept of *fieldnote* in that it naturalizes a place protected from transformations within the (prospective–retrospective) temporality of the writing process.

<sup>20</sup> Here we are dealing with the “literary” problem of ethnography in a more direct sense. Geertz already was very explicit in valuing its communicative performance over its aesthetic aspects: “A good interpretation of anything – a poem, a person, a history, a ritual, an institution, a society – takes us into the heart of that of which it is the interpretation. When it does not do that, but leads us instead somewhere else – into an admiration of its own elegance, of its author’s cleverness, or of the beauties of Euclidean order – it may have its intrinsic charms; but it is something else than what the task at hand calls for” (1973: 18). To drive this point home: a sociological description which draws the readers’ attention mainly to its aesthetic means of composition has failed.

random reader. In fact, because of the lack of deictic elements, written accounts are even more dependent of context information than oral narrations. If, in the deconstruction of ethnography in literary theory, readers appeared primarily as “opponents” who had to be overcome, in the process of crafting vivid descriptions, readers are rather animators whose imagination forces writers to be very detailed. The reader is part of a *ménage à trois* which up until now has always been regarded as two separate “couples” (researcher and field, author and reader). She or he is already part of the observation process, to the extent that the addressees of a description (colleagues, teachers, students, competitors, and informants) become co-observers, always already sitting at the observer’s shoulder: “Facing the typewriter each night means engaging these ‘others’ or alter egos” (Clifford, 1990: 64).<sup>21</sup>

Emerson, Fretz and Shaw’s (1995: 41) recommendation to pour one’s fresh memories directly into the protocol instead of engaging in “what happened today” talk with a curious listener, and in that way letting the narrative energy flow right onto paper, reminds us of that old advice of Bronislaw Malinowski’s to turn completely towards the “savages” and away from the Europeans in order to get more intensely involved in the field experiences. In exactly the same way one has to turn away from listeners and towards readers and to engage entirely with the written medium. Refraining from premature oral narration can channel “indescribable” experiences into text.

### The pre-lingual

Another kind of resistance to verbalization has to do with the pre-lingual, that is, with all those layers of implicit, embodied knowledge which the participants own without being able to simply verbalize it *themselves* – *tacit knowledge*, as it is so poignantly called. It is here that the limitations of interviews prove to be most unfortunate. Of the many areas of local distribution of knowledge, they only survey knowledge “in the head,” but not knowledge in the hands or the *situated knowledge* stored in the spatial organization of material practices and objects.<sup>22</sup> This kind of knowledge resists language not for moral reasons (like the unmentionable) but because its verbalization would be eminently unpractical: It would hinder the execution of everyday activities. Participants have a command of the grammar of their activities, but they cannot and need not explicate them. We are dealing here with intrinsically non-verbal knowledge, which poses considerable (and in part insurmountable)<sup>23</sup> resistance to verbalization to the sociological observer.

<sup>21</sup> This can also be formulated from the position of the author: The notion of observation is not only misleading because it implies a too strong separation between the subject and object of research, but also because “observation,” to the extent that it generates disciplinary knowledge only through *writing* processes, stands for a second *membership*: namely in the sociological discourse. *Fieldnotes* are an interface: as a local practice, they belong to the field, as a writing practice, they belong to academic discourse.

<sup>22</sup> This *knowing how* (Ryle) has been the target of ethnomethodological “*studies of work*” (Garfinkel, 1986), which explicitly take professional “*skills*” as their object; for example, the embodied knowledge in human-machine-interaction.

<sup>23</sup> This does not mean it is “lost.” The part of the ethnographic experience which remains “*tacit*” – a background knowledge of somatic traces and intuitively understood connections – plays a central role in orienting theoretical decisions about topics, hypotheses, and concepts.

Ethnographic attempts to pull pre-lingual knowledge into the realm of language are based mainly on two operations. The first is marked by the concept of “*participation*,” i.e., practical membership in events. In regard to the pre-lingual, the rationality of participation lies in immersing oneself in everyday practices but then (instead of bothering other people with questions about it) doing something very alien to practice: *verbalizing tacit knowledge*. Being “on site” in a novice role opens up access to those occasions in which “explanations” are also given to other learners (for example, to children or apprentices). A procedure is explained to them by *demonstrating it on an object*, accompanied by a few words. Furthermore, the local observer can attempt to acquire certain *skills*, in order to gain participation competence. Garfinkel’s invitation to “*become the phenomenon!*” was an instruction to plunge into the deep layers of practical knowledge and go into symbiosis with the object.<sup>24</sup> The sociological participant’s knowledge acquired in this way, however, would become immediately habitual if it were not simultaneously documented in writing.

The second operation for verbalizing pre-lingual knowledge consists in the *retreats* from the field which are made possible through writing. I have already named some of the opportunities for explication this allows for. When dealing with the pre-lingual, the following abilities need to be added: *Reading* one’s own writing (unlike listening to oneself speak) allows for a temporal distance from one’s own utterances. Writing allows for interaction with oneself and a further role change from participant (insider) to author (observer) and reader (commentator). The later this reading takes place and the more the *fieldnotes* have already fallen into oblivion the more, so Emerson et al. (1995: 145), they can be read as though they had been written by someone else: Ethnographers become their own informants, willing to be asked questions; they turn into textual objects, whose interactions in the field can be observed and commented on.

Thus approaching the field without reserve offers opportunities to explicate the pre-lingual, but those opportunities can only be fully exploited in a very decisive distancing move: they grow in an alienation process between author and notes. The criterion for achieved verbalization in the case of the pre-lingual lies in a successful transfer of practical knowledge into empirical knowledge: a precise description allows participants to “discover” their own experiences insofar as they clearly recognize what they only “half knew” before – namely not only knowing how to do something, but knowing how they actually do it.

### Things taken for granted

Another kind of resistance to verbalization related to pre-lingual knowledge has its source in that which “goes without saying.” Things taken for granted are “not worth mentioning.” On the one hand, their banality does not require words (this is the challenge of a sociology of everyday life). On the other hand, a lot is left out from verbalization because it is already symbolized outside language. Hence, one central component of what is taken for granted is the obvious, the self-evidence of the

<sup>24</sup> It is obvious that there are fields of social practice where such participation competences can be acquired either only rudimentarily or at the cost of leaving the discipline (see the discussion in Lynch, 1993: 273–275). In other fields however, becoming a co-worker, that is, a strong hybridization of roles, can be an indispensable condition for getting access.

visual. This is a treacherous problem for sociological verbalization, for no one needs to waste any words on what “already tells its own tale.” Just think of the pantomimic dimension of the social: those everyday “visualization methods” of the body. For a sociology so used to thinking in terms of language, these come across as “non-verbal” behavior, dubbed “*silent language*” by Hall (1959). While visual perceptions, especially, put a strong demand for verbalization on the describer, the visual, i.e., that which expresses itself visually, on the other hand, does not invite that. With its symbolism, which allows for quick understanding without words, it is almost an invitation to verbal laziness.<sup>25</sup>

The most powerful resource for dealing with things taken for granted and with the obvious ethnographically, is the unfamiliarity of the observer. That this is a resource for sociological knowledge is one of the oldest insights of the classical sociology of the stranger (i.e., of Simmel, 1908 & Schütz, 1964): In principle, verbalizations can be released more freely when at a distance from the culturally obvious. Accordingly, ethnographic textbooks recommend immediately explicating first impressions and exploiting them to a maximum, before the basic features of the field become normalized in one’s perception, and thus disappear again into one’s own pre-lingual tacit knowledge.

Ethnographers’ initial position as strangers in the field, which enables certain insights, can be methodically renewed during their stay and further used as a resource by varying the normal distance to the object, thus actively “alienating it” (Amann & Hirschauer, 1997). This already happens in the course of the manipulation of temporality, employed in managing the indescribable. To use an optical metaphor, descriptions – just like transcripts – should offer microscopic “close-ups.” If descriptions’ rendering of details were merely “realistic” they would not exceed the participants’ knowledge. Only a “hyperrealistic” change in scale enables us to see the all too familiar in a new light. Thus, description and transcription are not copies that can be looked *at* again and again (substituting the original), but rather different lenses one needs to “look *through*” in order to gain new ways of seeing.<sup>26</sup>

Furthermore, there are ways of making the familiar appear strange which are specific to description. They consist either in exposing one’s language of description to the field’s influences or withdrawing from it. The latter points to the use of metaphor, which describes or rather “transcribes” phenomena of one field using the language of another. Metaphors ask for “imaginative perception,” compensating for the immediateness of co-presence, of being-there, with an explicit observation from “elsewhere.” If ethnographers as participant observers have become completely

<sup>25</sup> It was “non-verbal behavior” that in the history of visual sociology suggested itself as the “natural” object for the use of the camera (Scherer, Banse, & Wallbott, 2001). Like audio recording in regard to ephemerality, the camera cannot be outrun by description. On the other hand, it finds in the obvious its biggest problem. The camera intensifies the problem of visual self-evidence, just because it is an “optical type” oriented to the visual and hence relying on other people seeing “the same thing.” The fact that this doesn’t happen becomes immediately evident as soon as the need arises to *speak* sociologically about what has been shown. On the testing of strategies of using the camera in an alienating way to “watch people watching,” see Amann and Mohn (1998).

<sup>26</sup> Here the comparison to photography suggests itself: this recording technology too manipulates the temporal structure of social processes. It turns them into stills and through this harsh way of decontextualization it allows for new forms of observation.

immersed in their objects, as describers they can expose them to incongruous perspectives which “intentionally miss” their alleged self-identity.<sup>27</sup> This means facing the obviousness of what is taken for granted by bypassing the *denotative* sense attached to cultural objects and opening oneself up to the ambiguous *connotations*, by which cultural objects evade the harshness of conceptual language.

The other option, which is the opposite of metaphor, lies in occupying the participants’ language: a stylistic mimesis of the field, taking participants’ accounts literally, thus shifting the meaning of what is said. Recordings and their notion of quotation guarantees mutual exclusivity of voices (of the field and of sociology); description intentionally allows mixing up participants’ categories and the voice of the author, making both audible in a sociological discourse. This technique leads to an exaggeration of the idiosyncratic properties of the field. It is comparable to the way caricature “disfigures to recognizability,” “multiplying” facial features with themselves. For example, a hyper-realistic (re)presentation can be achieved if one writes an ethnography of blindness using “haptic prose” (Langer, 2001) or presents an ethnography of surgical practice by portraying it as a subjectless interaction of bodies (Hirschauer, 1991).

### Muteness

The last example mentioned points to a form of silence which reaches “deeper” than any of the resistances to verbalization discussed so far. For some of them the gap from language seemed comparatively small. We could still view the description of facial expression, for example, as a translation of one code into another, a move that happens within a human universe of signs. This premise crumbles, however, when we are dealing with what is unable to speak, or the mute.

The requirements for descriptions of “silent discourse” (Tilley, 2001) of objects of material culture like spatial settings, (architecture, sitting arrangements, etc.), garments, artifacts, etc. are comparatively trivial. The challenge gets more complex when dealing with non-talking entities which are self-animated objects that can be considered agents, such as infants, fetuses, unconscious people, animals, bodies, and machines that are emitting signs: sounds, symptoms, signals. Sociology should not disregard those entities any longer, since after all, humans do establish contact with them either verbally or non-verbally; be they gods, ghosts, pets, plants or fetuses. As long as we are dealing with observable activities, for instance, with interactions between fathers and their infants, masters and dogs, humans and machines, the verbalization task in question can still be considered one of “description of behavior.” However, here we already have a problem on our hands: whose terminology should be used for description, when only one side is speaking? Description gets even more difficult when an interaction protocol is strongly fragmented by the muteness of one of the participants – cut in half like a telephone conversation where only one side is audible, as in the case of a pregnant woman communicating with her unborn child or of people in prayer waiting for responses from their deceased or from gods.

It is not by accident that such description problems at the “borders of the social world” (Luckmann, 1980) arise primarily in the sociology of the body (Lindemann,

<sup>27</sup> This is the serious aspect of the otherwise satirical classic “Body Ritual among the Nacirema,” portraying North-American bathroom habits in terms of religious ritual (Miner, 1956).

2005) and in ethnographic research in science studies. Various approaches in the social studies of science and technology have demonstrated that on the level of *tacit knowledge* not only bodies, but also artifacts have to be included into sociological analysis. They too embody knowledge and hence belong to the constituents of social worlds (Preda, 2000). Especially *Actor Network Theory* (ANT) has made this a strong point. Against other approaches in the sociology of knowledge, ANT argues that ethnographies especially concerned with the reality of the natural sciences should not sociologically push aside the ontology of their objects. As much as the notion of *voicing* in anthropology was directed in a self-reflexive twist also against the anthropologist (and his or her euro-centrism), in science studies it has brought about a critique of anthropocentrism and a symmetry postulate for the relationship between disciplines. Sociological descriptions (too) should give words to natural objects by installing an array of “advocates” (Callon, 1986): If we allow the natural sciences to make their own relevances be heard and stand their ground against sociological discourse, this also means communicating *their* voicing of things (Latour 1993: 42).

From a theoretical perspective, ANT provides an example for the kind of innovations which can be achieved through a radicalized ethnography engaging with its object in a maximally permissive way. For that purpose, it has to invent a descriptive language which dispenses with the idea that agency is a property exclusive to humans. At the same time, this approach points to methodological problems concerning the inclusion of the mute in sociological discourse. Artifacts or organisms do not “formulate” (in the terms of ethnomethodology) their own contributions to a practice (Preda, 2000: 288). They do not indicate what they are doing when they are doing it. Methodologically speaking, this creates a gap for sociological verbalization. It requires complex translation chains in which not only scientists as advocates, but also their laboratories as unique articulation spaces for natural facts are indispensable. Only here can micro-organisms communicate with humans by way of leaving traces in esoteric recording devices: electric impulses, radioactive “tracers,” etc. Therefore scientists use many non-linguistic forms of signification in their own communication in which pictures (technical photographs) are much more important than texts. Ethnographic observers, however, in trying to further the representation of the mute inhabitants of the planet, put themselves in a paradoxical situation. On the one hand, they act (and in the rhetoric of ANT very emphatically so) as *advocates* (of antecedent advocates); on the other hand, they articulate themselves as *advocates* within the linguistic framework of the medium that dominates their discipline and that at the same time excludes all kinds of self-representation by “*non-humans*.”

The limits of what can be said

To summarize my discussion up to this point: I started out with the statement that sociological description has to verbalize aspects of the social for which participants’ formulations often seem insufficient. Ethnographic verbalization is able to surpass participants’ propositional knowledge through a number of resources, the more general ones being time and writing, and the more specific ones being the use of various cultural techniques of verbalization: establishing special spaces for verbalization, rumination over experience in writing, the ongoing interruption of perception

through the writing process, changes of scale and detail, making familiar objects appear other, and alienation from one's own linguistic products, etc. These resources are opened up to ethnographers through participation in the sociological discourse in which they function as "writing tools." Opening up forms of silence always also implies a break with the membership in the field: betraying loyalties to the unmentionable, overcoming intense experiences that render us speechless, breaking with participation competence, which keeps knowledge "*tacit*," and formulating what is taken for granted and thus in need of no words.

My review of such verbalization problems implies a paradoxical point. On the one hand, discovering what an important role language has played in ethnography opens up the non-verbal dimension of social reality: without the fixation on verbal data, our image of the social changes. On the other hand, all claims to the knowledge of this dimension are confronted with having to take one's own linguistic constructions seriously as a task and as a problem. At "the end of the linguistic turn" (Knoblauch, 2000), we are confronted with the demand for a serious engagement with our own language.

Without descriptive constructions, the mentioned verbalization problems cannot be solved because recourse to a pre-existing discourse (the participants' or the discourse of sociology) is not enough; nor does practice, through its implicit self-characterizations, "tell" us exactly how it "needs" to be described: often enough its self-explications remain fragmentary, cryptic, polyvalent, multi-vocal, contradictory, and hence simply *not descriptive*. A gap always remains and that requires a great leap for a science which largely exists in the form of text, but whose objects are not textual. At the same time, the descriptive closure of this gap is never achieved without cracks: In the same way in which recording obstructs the fleetingness of the social event (Bergmann, 1985: 317), the verbalization of the silent does not come without a basic transformation of the object. The social does not exist in language on its own. *This* (and not the adding of an observer's interpretation) is the decisive factor why descriptions cannot be doubles.

The verbalization problem suggests a basic quality criterion for sociological description, namely a greater pervasion of language within social reality, i.e., shifting the borders of articulation. Description is linguistic "land reclamation"; its task lies in extension of the world (Knorr Cetina, 1989: 94) for sociological knowledge. At some point, such a transformation of the social into discourse will reach an insurmountable limit of verbalization. Together with the verbal explication of the social, knowledge about that which cannot be said – the ineffable – also grows. These limits have two reasons. The first is the empirical means available for pushing language forward into all nooks and crannies of the silent: What can be put into words sociologically without laboratory equipment or without a couch and a body undergoing long-term psychoanalysis? The second reason lies in the limitation of the medium itself (which I also accepted in this article): Language reaches the limits of what can be said, and beyond those limits phenomena can only be articulated by means of other communication media. Ethnographic writing oscillates between those two often quoted remarks by Wittgenstein: "*The limits of my language mean the limits of my world*" (1974: 115). And: "What can be said at all can be said clearly, and what we cannot talk about we must pass over in silence" (1974: 3).

The first remark has to be taken as a challenge for description: Ethnography has to fathom those limits, finding out what it is that cannot be talked about. The second



remark however, can be read as a demand to change the medium at a point which has yet to be determined.<sup>28</sup>

## Conclusion

I have dealt with a particular problem of ethnographic methodology, wholly concentrating on a specific type of data needed for the verbalization of the “silent.” To conclude, I would like to answer two questions that have been neglected by narrowing down the problem the way I did: If we compare them to recordings, how can the great liberties of description be controlled? (1) Where does the special research strategic value of description for sociological knowledge lie, if not in documentation? (2).

(1) Obviously the “constructive moment” in descriptions is greater than that in recording. They are “homemade,” which is why Geertz quite openly spoke of their “fictional” character (1973: 15). Furthermore, we have found that the hybridization of the “data” with the interpretations of the observer does not allow for any means within the text whereby the reader could exercise any control. This weakness in the reliability of description, however, is relativized in the context of ethnography through three factors. First, it is because – depending on the field – there are also other data types, complementary to description, (recordings and self-descriptions elicited through interviews, among them) which get processed and presented. Hence, most ethnographies can be thought of as a collaborative verbalization of the social through participants and observers.

Secondly, such a co-articulation consists not only in the integration of varying data types, but is rooted in the whole ethnographic research process itself. In ethnography, the resistance of the empirical, which has to be allowed by any science based on experience, is not merely incorporated into *data*, as in the research strategies primarily relying on recordings. Rather, the emphasis that the concept of experience receives in ethnography stands for giving the field (during a long-term close contact) a maximum of opportunities to “inscribe” itself into researchers and authors. That is why their interpretations are so much more strongly bound to the research situation and the control exercised by the *participants* in it (Amann & Hirschauer, 1997: 32). Approaches that primarily analyze documents, on the other hand, abruptly free themselves from this control, replacing it with their own disciplinary context. Hence the ethnographer’s primary recording devices are not merely sense organs, but rapport: The social events are “recorded” through the medium of the research relationship. A description is “thick” because the voice of its author is so impenetrably multi-lingual that it may happen that “other people make use of his mouth” (Simo, 1991: 138).

<sup>28</sup> Outside the social sciences, language is of course transgressed in almost all the arts, especially in music; literature too has a “zone of the ineffable” (Fuchs, 1989: 163). Within sociology, the possibilities are more limited, but nevertheless, interesting: in “ethnodramatic” performances of research results (Mienczakowski, 2001) or in attempts to use *visual media* not only for the recording of “data,” but also as a semiotic extension of sociological communication, and as an analytically articulate form of showing (Amann & Mohn, 1998). If such a visual form of sociological communication were to become established, this might have two opposite effects for our topic: It would make the competence in description shrivel even more, *and* it would, once again, highlight the possibilities that are special to description.

This control through the field, however, does not change the central outcome of the debate on the crisis of ethnographic representation, namely that description, regardless of occasional “ventriloquy” (Geertz) in the end falls within the responsibility of the author. Since it is not a transcript, description has to end with a *signature*. The claim to authorship implied in this gesture is on the one hand – considering the contributions of the research objects to the text – always way too strong; on the other hand, (compared with the genesis of other data types) it can never be strong enough.

Thirdly, in the sense of the *ménage à trois* mentioned above, the controlling reader as “co-author of texts”, whose reading completes the text, is needed. Some authors are rather skeptical concerning the amount of control the reader can exert. They say that the ethnographer has only the text to persuade, readers only the text to control (Reichertz, 1992: 334) since they cannot access the object themselves. In my opinion, this view does not hold for *sociological* ethnography for two reasons: Firstly, for many this reading will not constitute a “first contact” with the culture presented. They might already have some information through the written self-representations of a field, through journalistic coverage, or even through their own experience. Secondly, some of the “objects” of the study might be among the readers. One of the premises of classical ethnography was that members of oral cultures never had the opportunity to study the works written about them. In contrast, all sociological and current anthropological ethnographies carry a new risk: they are subject to generalized “*respondent invalidation*” in which “*the native talks back.*” At the same time, it is irrelevant whether there are few or many such readers. Even if most readers do have only the text to go by, still, the author cannot be *certain* of that. Not so much that what “is the case” delimits the constructions of the ethnographer, but that someone with good reason *might say* that *something else* is the case.

(2) However, as soon as readers of ethnographies have extra-textual access to their object, the problem of professional control does not seem so central any more. Much more severe is another problem, which is the opposite of all the challenges of documentation – be it forgetfulness, ephemerality, or the vanishing of oral cultures. The problem is not that something is “still there” but about to disappear, but rather that something is always “*already there*”: that is, the many forms of easily available self-representations of the object – observable as public habitus, legible in written accounts, consumable in journalistic writing and in talk-shows. What has caught up late but ever more forcefully with cultural anthropology, is the notion that its work might be superfluous in the face of the self-representations of the natives. This insight ought to be a natural starting point for sociological ethnography. The problem of concern for ethnographic writing is not the disappearance of its object, but rather its own redundancy.

An ethnography which limits itself to eloquent reports of other people’s exotic lives and talk becomes gossipy as soon as those people start to publicly articulate their self-reflexive knowledge themselves. Here, the accusation frequently brought forth against ethnography in general is on target: if a sociological reportage finds its research value primarily in the novelty of its object, it merely produces a copy of reality. If every description is an addition, omission, accentuation, and presentation, and if all these things are considered “distortion” according to the truth criteria of the recording, then it has to be said that many ethnographies suffer from a *lack of distortion*. After the symbiosis with the object they lack a phase of individualization – those de-contextualization

processes in which a perspective is created, a vision of the field that differs starkly from the way members can see it.

Readers have a right to claim that a sociological description “make a difference.” In order for that to happen, however, it is important that its constructive moment be developed, rather than kept under control. Descriptions have to *make use* of their liberties. But this also means that they will be judged according to their *analytical* achievements. I noted several times that descriptions do not hold their own against theoretical “prejudices.” On the other hand, descriptions, compared with recordings, have a much higher potential for theoretical innovations: Where language becomes the central instrument for data production, *concept formation* becomes the center of empiricism, since from the very beginning it is in line with the whole struggle for verbalization with which description is dealing. It is from “soft” data that ethnographies obtain their theoretical impulses. If these impulses are taken up and developed further, the solution to the redundancy problem might be found in practicing ethnography, much against its traditionally naturalistic self-definition, as *theoretical social research*.

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