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The Credibility of Qualitative Research

edited by
Anne Ryen

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Qualitative Sociology Review
Volume IV, Issue 3 – December 2008

Guest Editor: Anne Ryen
University of Agder, Norway

Editorial: The Credibility of Qualitative Research

This special issue of *Qualitative Sociology Review* is dedicated to the topic "The credibility of qualitative research". In June 2007 the European Science Foundation funded the ESF Exploratory Workshop "Improving The Quality of Qualitative Research" arranged at University of Agder, Kristiansand, Norway

http://www.esf.org/index.php?eID=tx_nawsecuredl&u=0&file=fileadmin/be_user/ew_docs/06-193_Report.pdf&t=1226941778&hash=17f05fba007263bcf8cfb24ea1fd86af

This was an initiative from the 2005-7 Presidency of the European Sociological Association, Research Network 20 Qualitative Research (ESA RN20)

http://www.europeansociology.org/index.php?option=com_content&task=view&id=38&Itemid=29

with a brief review and a photo from the workshop including the history and the activities of the RN20 in the ESA Newsletter here:

http://www.europeansociology.org/images/stories/ESA2007_2009/Newsletters/esa_newsletter_summer2008_high_quality_update.pdf

A more formal report "Is There a "Legitimation Crisis" in Qualitative Methods?" from the workshop was published by the then ESA RN20 President - Shalva Weil in *Forum: Qualitative Social Research FQS,* Volume 9, No. 2, Art. 6 – May 2008

http://www.qualitative-research.net/index.php/fqs/article/view/438/948

To cite the abstract for the workshop application:

> Multiple theories and contrasting methodologies in qualitative research may persuade us that credibility is irrelevant. Denzin and Lincoln, by referring to a 'legitimation crisis' which questions traditional criteria for evaluating and interpreting qualitative research (2000:17), may confirm doubts of funding agencies and quantitative researchers.
> This workshop will convene internationally recognized scholars to develop substantial arguments to satisfy external critics concerning credibility gaps and inspire a new generation of qualitative researchers. (Silverman, Weil and Ryen 2007)

Qualitative research is increasingly being employed as a suitable methodology across disciplines and professions including evaluation studies and traditional quantitative territory such as business studies, economic geography and development studies across continents. A substantial amount of such studies work as background to decisions on social change to in one way or another improve

people's living conditions. However, successful policy implications are dependent on the credibility of such qualitative research. If not, the consequences may be severe.

Our aim is to make sure the discussion about the quality of our research remains a topic across methods and practice. This QSR issue thereby is a following-up of the ongoing discussion of credibility of qualitative research claiming credibility is a most highly relevant issue. Still, in the wide array of methodological literature we do find a range of criteria to gauge successful standards, and as claimed by Christian Lüders (2007:359) they "are further developed and put into concrete terms in the various schools and methodological approaches". However, the criteria tend to remain at a rather general level, and rather conflicting when made more relevant to practice. No sets are clearly defined for the future directions, and will need to address both internal problems as well as external expectations (Also see von Kardoff 2007, on utilization of social science knowledge). Hubert Knoblauch (207:354) addresses the continental division in qualitative research. He refers to the difference between the Anglo-Saxon and the German-speaking countries where postmodernism never had the same strong impact on qualitative research in the latter compared to the former countries. This difference between continents is addressed in the ongoing ESF Programme EUROQUAL chaired by Paul Atkinson:

> Based on the fact that while qualitative research is highly visible in many fields of social-science research, it exists within many sub-specialisms, and reflects national as well as disciplinary boundaries. There is a clear need for scholars throughout Europe to share, develop and promote high-level methodological expertise. There is an equally pressing need for capacity-building within the European social sciences
>
> http://www.esf.org/activities/research-networking-programmes/social-sciences-scss/current-esf-research-networking-programmes/qualitative-research-in-the-social-sciences-in-europe-euroqual.html

In their Introduction to *Qualitative Research Practice* Clive Seale et al (2004: 1-11) address the issue of general norms and research practice where they argue against making qualitative research into a question of historical stages or moments as in Denzin and Lincoln's *Handbook of Qualitative Research* (1996), and see any general framework to guide research practice as provisional representing a "partial truth" only though differentiated from the postmodern argument. They also refer to the unhappy distinction between the external political role of methodology and the internal procedural role. In the former the task of methodology has been to legitimate our work to those asking for our results, and in the latter to guide researchers along the process of doing research. Rather, they place research practice at the centre *"…instead of forcibly applying abstract methodological rules to contingent situations, the research situation is placed in a position of dialogue with methodological rules"* (2004:7). This connects well with the contributions in this QSR issue.

We are very happy that some of the presentations from the referred ESF workshop are published in this *Qualitative Sociology Review* issue, and that other qualitative researchers as well have contributed to explore this highly relevant and important topic.

In the opening article "Triangulation and Dealing with the Realness of Qualitative Research" Krzysztof Konecki presents his reflections on working in the qualitative field with a focus on the accountability of research conclusions. He poses a most relevant question: How is the description of reality constructed in ethnographic reports? In his discussion of the meanings and interpretations of observed events, he argues they are part of a symbolic interaction between the

author and his or her audience, “the generalised other”, thus creating a particular context in which the text is received. According to Konecki this has a big impact both on what we as researchers write about and how they write. This way the audience influences the text. In his discussion of the representation of field events, he draws upon two of his previous empirical projects.

Also Marie Buscatto draws on organisational ethnography in her article “Who Allowed You to Observe? A Reflexive Overt Organizational Ethnography” where she uses a reflexive perspective to discuss how ethnographers ensure that their final results are “scientific”. More precisely, she discusses how ethnographers may employ techniques and procedures offered as guidance throughout the different research stages. In her article she draws on her own experience from doing overt research in private companies and illustrates how analysing empirical data becomes a central part of knowledge and enrich the quality of qualitative methods.

Lars-Christer Hydèn on “Narratives in Illness: A Methodological Note” invites us into the debate about narratives as text or performance with himself well positioned in the latter approach with a focus on how stories are told along with using also other communicative modalities. As opposed to many other studies on patients with Alzheimers, Hydèn analyses a narrative told by an Alzheimers patient herself. In his detailed transcriptions he shows how the telling of the story or more exact the performance between the patient and her listeners, becomes a joint and mutual performance. Telling stories then, he argues, becomes a multimodal event where the patient manages to construct her identity not only as a linguistic construct, but also embodied where the old self emerges into the teller-self, of special importance in the field of health and illness.

In “Vision and Performance. The Sociolinguistic Analysis of Genres and Its Application to Focussed Ethnographic Data” Bernt Schnettler discusses the use of audiovisual recording devices in qualitative research. Differentiating between the old focus on data collection, his concern is with the quality and transparency of data analysis. Video-data are mediated representations of reality transformed into data which makes analysing video data a most current challenge. He demonstrates the benefits as well as the limitations of sociolinguistic genre analysis. In his discussion he draws on video-taped data of a New Religious Movement’s spiritual leader and stepwise introduces data not in the video to contextualise the video fragment previously explored in detail. To decipher the meaning of certain aspects of the leader’s visionary performance, he argues a need for focussed ethnographic fieldwork supplied also by data from other methods which he makes into a general statement when using video data.

In “Wading the Field with My Key Informant: Exploring Field Relations”, Anne Ryen explores the ethnographic puzzle of prolonged field relations in qualitative research. She claims a need for bringing in the “[inter]” and the social into the analysis. She claims the problem with the classic commonsense explanations is making us into integral components of the very world we seek to describe. Rather, she argues a need for exploring (all field-) members` interpretive work and thus recognises the classic ethnomethodological differentiation between topic and resource. However, in cross-cultural research we can not take for granted that members share vocabularies, descriptions and images. This often introduces misunderstandings and even communicative breakdowns in the field. She therefore argues a need for Membership Categorisation Device analysis while also bringing the wider culture into the analysis. She illustrates with data extracts from her fieldwork in East-Africa.

This way, all articles in this edition make use of a researcher-centred view and adapt methodology to the research situation. Therefore, we hope this QSR issue will be a most relevant contribution to the ongoing debate of the credibility of qualitative research practice and how to make our research count.

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Qualitative Sociology Review
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Lodz University, Poland

Triangulation and Dealing with the Realness of Qualitative Research

Abstract

This paper provides a sociologists' reflection of a sociologist on qualitative field research. Reflections will include some methodological and epistemological considerations that are connected with field work, while building the realness of the description and conclusions, i.e. constructing the quality of qualitative research. The intellectual process of doing research will be characterized by analysis of:

1. description of investigated reality (tales of the field),
2. analytical process,
3. usage of commonsense research procedures (so called triangulation procedures), which are used in the field by the researcher and during analysis or writing a research report to adequately "re – present" researched reality.

The three above mentioned stages of representation of reality are interwoven to create one complex intellectual process, which is called "field research". The quality of qualitative research is the intellectual process where some procedures are used to create the accountability of research conclusions.

Keywords

Triangulation; Field research; Qualitative methods; Sociology; Ethnography; Writing of ethnography; Ethnographic description; Definition of reality; Realness; Tribal rules; Generalized other

The considerations in the paper are based on two assumptions. The first one is that social researchers have specific perspectives of social and psychic reality. The social world is divided into parts which can be differentiated according to a criterion and between these abstract categories ("parts of reality") exist *real* connections. Individual thoughts and feelings have also "causes"‘ or conditions that are included in the above-mentioned "parts of reality" or in the relations between them.

The second assumption is that social researchers try to show a description of social world as real and adequate. They want to prove that the described organization, organizational structure, reconstructed strategy or mission, community relations exist in reality, not only inside but also outside the description. The problem of the quality of qualitative field research is strictly connected with testifying the

description of reality, accounts of it and the "reality itself" (by triangulation) and not with the problem of validity i.e. whether the research procedures truly help to answer the research questions or whether the research measures what it was intended to measure. The quality of qualitative field research is also not connected with a problem of reliability (whether the results are consistent over time and how exact they are in their conclusions) because even revisits to the field could not solve the problem of temporal and historical changes of reality in the investigated field. The fields of ethnographic research are changing and very often are not well defined in advance (fuzzy fields); the researched phenomena could be found in many places to be observed (Nadai, Maeder 2005).

Following the trail of research and constructing reality we will proceed in the wake of hints given by K. Charmaz (2006: 127). Rather than explaining reality, social constructionists see multiple realities and therefore ask: *What* do people assume is real? *How* do they construct and act on their view of reality? Thus, knowledge and theories are situated and located in particular positions, perspectives, and experiences." Thus constructions are made by researched people and researchers themselves. The data are constructed; there is no amorphic description of reality from a constructivist perspective (see Goode 2007: 7, 14-16). Moreover, the construction is inscribed in the relation between investigated people and researchers. Even the detailed transcriptions of data and "focused ethnography" that is intensive in data gathering and analysis (Knoblauch 2005) does not resolve the problem. The question then becomes how is the description of reality constructed in ethnographic reports?

The questions of realness of the researched reality

> *Writing is also a way of "knowing" - a method of discovery and analysis.*
>
> Laurel Richardson

Realness is a process of achieving compatibility of explanations of taken for granted assumptions concerning the rationality and typicality of human action in everyday life. However, concern for realness is also present in the social sciences. There are special categories and procedures for evaluating the realness of social research, such as validity, reliability, triangulation, revisits etcetera. Usually these procedures are considered to be the tools for ensuring "realness" independent of the contexts of their usage.

Sociologists, anthropologists, social psychologists and other researchers of society who use qualitative methodologies in their studies, especially in field studies, all tend to show a particular predilection to account for realness, in other words, *validity* of research conclusions, formulated hypotheses, verified theories or observed and studied reality or/and authenticity, credibility, plausibility (Belousov 2007: 163 – 164; Silverman 2001: 232- 241; Hamerslay; 1990: 57; Van Maanen 1988: 45 - 72; Stake 1994: 241; Glaser 1978: 4; Glaser, Strauss 1967: 238 - 239; Janesick 1994: 214 - 217; Kvale 1996: 229 – 250; Zakrzewska - Manterys 1996; Konecki 1989; Konecki and Kulpińska 1996; Ożyński, 1998: 9 – 15; Marciniak 2008: 46 – 48; see also Gobo 2008: 264; 267 - 268). It might be the result of their commitment to the research in the role of "research tools", that is, acting as a device to listen, observe and record the perceived "reality". In this role they experience on a daily basis the "dialogical" nature of their research methodology, which interacts permanently with

the observed social actors and communities. Moreover, they are aware of their individual weaknesses, prejudices, personal predilections, which are often present in their everyday life. Why then would these factors not influence the research process, which is nothing more than another form of daily experience and explication of the world, with the only difference of being performed during field studies with the justification coming from the usage of scientific procedures? Very few researchers are not particulary interested in the methodological account of supposition e.g. E. Goffman (Becker 2003: 660; see also very short methodological notes in the book *Asylums* by E. Goffman 1961). This situation might be the result of their conviction concerning the difficulty of developing the relevant procedures for field studies, as well as its consequent use, and, in an unlikely event of successful description, the readers may trace the misstep in the usage of the procedure and thus accuse the researcher of causing the unnecessary disarray. The other reason might be the strong emphasis on freedom of the researcher, meaning the unconventional thinking, also in terms of methodology (Becker 2003: 660).

By qualitative field research we refer to research based mainly on participant observation as the rudimentary research technique and on the unstructured interview as a supporting technique. Such a situation implies a longer stopover for the researcher within the community and his active participation in order to discover the customs and the patterns for interpretation and interaction with the members of a given group as well as the social structure, which binds them together. The researcher builds close relations with the observed individuals. The crucial aspect, from the perspective of such studies, is the access to the life of the group and the acceptance of the researcher within the structure of the group to “share” the reality of the other, and to establish the intersubjectivity with him and to base research on the so called “intimate familiarity” (Silverman 2001: 57; Punch 1994: 84, see also: Van Maanen 1988; Hammersley, Atkinson 1995, chapter 3; Gobo 2008: 120 – 124; Prus 1996: 250-251; Blumer 1969; Kleinknecht 2007: 61 -63).

A field researcher, for example, during his studies, every day asks himself questions regarding realness of the perceived world with reference to its record and its inherent accounts which are interwoven with a “real” description of the world. How is it possible, that something I had observed yesterday is not relevant to what I saw today? Do I need to observe “the same” actions tomorrow then? What will they be like? Will these verified observations eventually lead me to a final and trustworthy version of the description and interpretation of the conduct of the inspected group? What am I suppose to do with the previous versions? Can I state retrospectively that they had been less real than the ones I observed on a last day? Shall I make some more observations, maybe in other context? Perhaps I need to encounter the representatives of other social groups in order to confirm the realness of their behaviour in its diversity? Questions like these demonstrate that a field researcher faces the permanent problem of not only adequate description of reality but also the problem of constructing in a temporal dimension (reality changes according to the passage of time) and interactive dimension (a researcher is influenced by the observed social reality whilst simultaneously influencing it).

The abovementioned questions evolved, in part, through my own experience in the field. They may become even more complicated if I take into consideration my studies of various cultures, for instance Japanese or American, where the matter of adequate representation of observed and generated reality seems to be even more complex. The researcher into European culture is in the ‘studied’ foreign culture merely “a stranger” or “newcomer” (Schutz 1944), who experiences all existing cognitive limitations adequate to the given situation.

The problem of “representation” refers to such issues as: reception of a given text (which is an interpretation itself) by a specific audience and the place of the author in the text in reference to “the other”. “Representation” then is usually the “presentation of oneself”, whether or not we want it to be so. It means that the presence of “the other” usually refers to the presence of the author in the text. The presented “other” is usually an alternative identity of the researcher (Denzin, Lincoln 1994: 503, see also: Van Maanen 1988: 45 - 72). What is more, “the other” is not merely an exterior audience, but also the audience which is the “subject of the study”. The issue of representation then refers to the problem of representation in the interactive dimension, with regard to broader social and structural conditioning (see: Strauss 1993: 172 – 186). The researcher (the narrator) while presenting him- or herself in a certain manner, at the same time represents a social group of academics. The intersubjective dimension of the scientific discourse is all the time present in the daily life of the field researcher (Prus 1996; Gobo 2008).

The analysis of rhetorical techniques used by the author displays the problems of representation, which usually refers to particular literary devices (aesthetic) which are present in scientific writings, with the dominating technique of presenting “the other” known as realistic technique (Atkinson, Hammersley 1994: 254-257 and Van Maanen 1988). It might be stated then that the presentation of “the other” is impossible without representation of reality, as well as the identity of the researcher, which is closely related, at least by the mere fact of observation.

When I observe my past research work, I doubt whether I really visited the places I describe and that I produced “real” research results (Konecki 1992a; 1992; 1996). The places were culturally unfamiliar to me, and when I look back at them they seem less real from today’s perspective (should I revisit these places?). Therefore, possibly, I wrote the methodological note to confirm my presence in these places, which serves as an observation tool, despite the fact that I stayed there as a private individual. My research reports differ in some details from what I observed in these places, a number of events and definitions did not comply with the scientific research report because some notional categories were not “saturated” enough or they occurred as idiosyncrasies and they did not fit into the theoretical framework, types (see: Glaser, Strauss 1967: 61). As a result I abandoned them. Should I have come to the conclusion that they were less real as the author’s objective research reports did not take them into consideration?

As I wrote the term “author’s research reports”, I consciously used the third person singular to emphasize that ‘HE’ – the researcher – had an insight into the social reality as opposed to ‘I’, as a private individual. My observation and interpretations then, as well as producing a research report, were governed by certain conventions, exterior from my perspective. I had in my mind (often unconsciously) the prospective readers of my reports. After all, I carried the observations for somebody else, I interpreted for someone else and I wanted to write books for somebody else, despite the fact I was doing these things mainly for myself. This is the nature of research and scientific work. Observation as well as interpretation, analysis and description, which is indeed a representation of reality, all combine to configure one intellectual process (see: Fig. 1). The meanings and interpretations of the observed events are not established once and for all by the author, but are interwoven into a symbolic reality of a particular social context of reception of the text. From the interactionist perspective it should be stated that the authors of field research reports write for a particular audience and apply in so doing they tend to apply certain assumptions regarding the attitudes, expectations and views of potential readers. Those readers are then the “generalized other” of the

author. Writing of the text together with the application of the role of the other begins at the time of observation and develops into intersubjective interchange of the ideas in the further stages of the research and writing: "While in the field, the ethnographer has an obligation to think and act with respect to the viewpoint of the generalized other. Information is gathered, observations are made, and questions are asked mindful of generalized other. It is incumbent on the ethnographer to adopt the scientific viewpoint, not as a distant party in the situation, but as a conscientious, thorough, curious, information seeking representative of the scholarly community." (Prus 1996: 253).

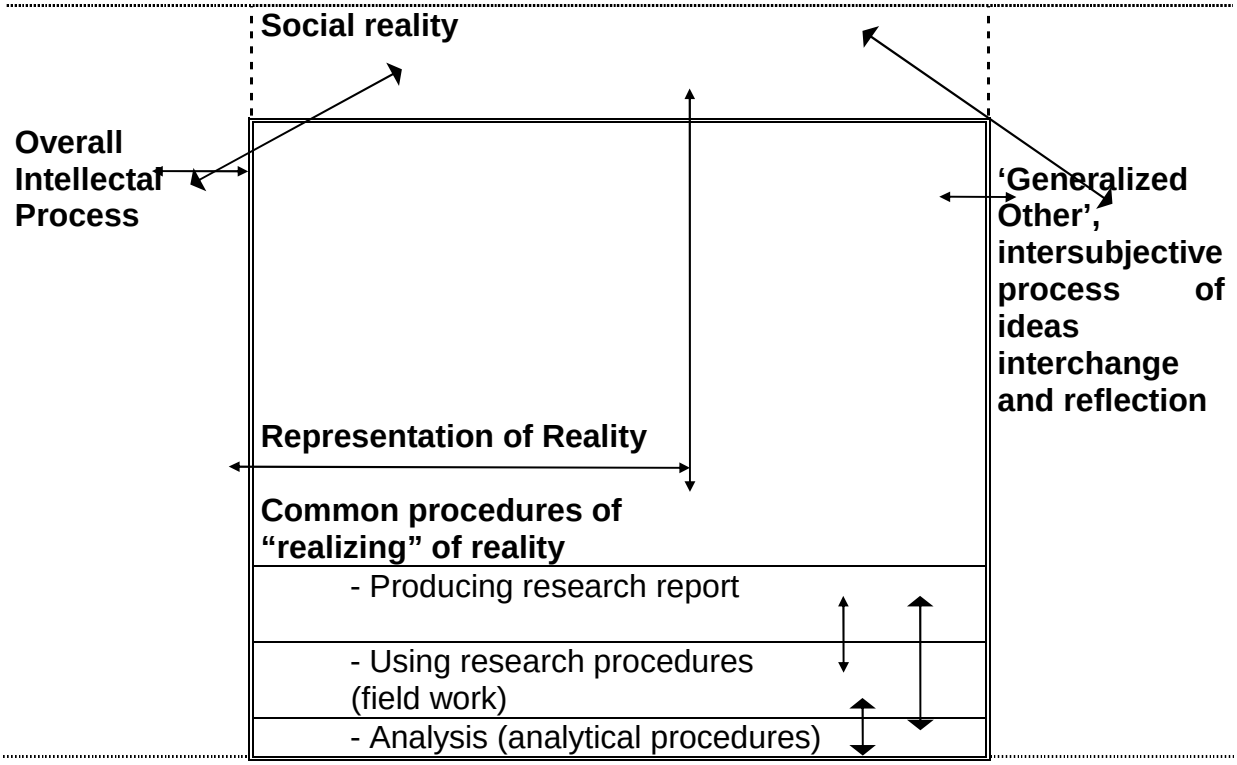


Figure 1. „Overall intellectual process" scheme in the work of a field researcher in building the quality of qualitative research.

"Generalized other", according to G. H. Mead, is a particular vision of social order and a vision of oneself shaped on the basis of "taking the role of the other", as well as recognition of the rules of mutual adjustment of roles within a wider social context. The socialization allows an individual to see oneself from a "generalized" perspective of a group and also creates an opportunity for the development of one's own social identity. What one gains is a complete set of attitudes, with which others may approach us (Mead 1932, chapter 20; see also the study of G. H. Mead's concept in: Ziolkowski 1981: 58 - 60; Krzeminski 1986: 53 ; Halas 1987). The above attitudes are represented by a group of readers of field researcher's reports. The expectations concerning the research reports, as we see them, influence the choice of what we write about and in what manner. "Generalized other", with internalized system of conventions, has then a colossal symbolic power over the representatives of social studies.

The question becomes one of who field researchers address their work to? According to Van Maanen (1988: 27-33) there exist three main types of reader of ethnographic studies. These are:

> **1.** Collegial readers, who possess the best recognition of ethnographic norms in particular research areas. They also know the standards of correct textual presentation. The important role here is given to the jargon used by the researchers. Assisted by the jargon they identify themselves with a certain club of researchers. The terminology such as "grounded theory", "impression management", "informants", "interaction order", "indexical rules", "social construction" or "interpretative procedures" place the researcher in a particular tradition, and show his/her belonging within a certain group, which excludes the uninitiated.
> **2.** Social science readers, they are the scholars who do not carry the field research but examine the research reports for information essential for their personal research interests. They evaluate the reports on the basis of the extent to which they provide the necessary information. Field researchers put low value on this group of readers, who might be extremely critical because reports fail to provide the required information.
> **3.** General readers who read ethnographic papers for entertainment, or for informative reasons before, for example, a visit to a foreign culture. In this context, the researchers become the narrators, storytellers, and their reports take the role of allegories. Writing for non-professional readers diminishes the significance of the criteria applied by the academic readers. The reports tend to show the culture rather than analyze it. Such reports are interesting from the literary perspective and free from academic jargon (see: Marriot 1999). Reliability of the description might be evaluated only by experts in a particular discipline[1].

The expectations regarding the perspective and chosen main readers of the reports determine the interpretation of observed behaviour and manner of presentation of data. Van Maanen (1988) distinguishes three main types of field research reports, based on the analysis of rudimentary standards of presentation of ethnography used by authors. He names these reports as "tales of the field" because they have certain narrative features such as: manner of presentation of narrator, metaphors, decorative expressions, text organization and style. The category "tales" implies the "representative" qualities of ethnographic texts, because, according to Van Maanen, there is no direct correspondence between the experienced reality and that presented in the text. Even if there is such a correspondence, it is no more real than the one between the observer and the observed. The first type of tale is the realist tale (compare also Gobo 2008: 290-292), which is the most frequent in the tradition of field studies, written for "collegial readers". The narration in the monographs and articles is presented in a detached manner, in the third person singular. The author is particularly concerned with an impression of authenticism in the presentation of a certain reality. It results in author's apparent absence in the text. S/he only equips himself with references by presenting their academic affiliations and

[1] A similar division was introduced by Strauss and Corbin (1990: 238-239). They distinguish the three groups of readers of qualitative monographs: 1. professional colleagues, 2. readers from various research disciplines, including practicing researchers, 3. non-professionals. Whereas Denzin (1994: 506) defines two types of readers, or "interpreters": 1. readers who possessed a certain experience in the area, and 2. "well informed experts", who are mainly field researchers. The first group searches for contextual and meaningful accounts (emic), the other concentrates on the abstract, non-contextual accounts (etic), which often refer to a particular theory. Giampietro Gobo mentioned in his book (2008: 289) about, so called, "Model Reader", that is not the actual reader but the reader type, imaginary audience, that is taken into account while choosing the stylistic options.

also presents a professional training and impersonal interest in the subject, which legitimates their access to a particular culture and its examination. The author then builds his/her authority by showing their own experience (experiential authority). The author shows: "X did this", rather than: "I saw X doing this". The realist tale annihilates the researcher and, according to the standard, the text concentrates exceptionally on what has been said, done or potentially thought by the people, who are the subject of the study. The role of the researcher is limited only to perform the description of place, duration and the strategy of research. Realist tales avoid abstract definitions and concentrate on particular descriptions of real, everyday life. Field researcher imposes a certain structure to the gathered data, which creates particular types of phenomena and characters, e.g. a typical married couple, a typical divorce, a typical member of a given culture, typical actions, typical interpretations, a typical new employee, etcetera (typical forms). The presentation of field observations need to have accounts by the members of a given culture made on the basis of the events that occurred in their lives, particularly concerning routine procedures. The researcher then presents the perspectives and interpretive practice of the members of a given community (the native's point of view). The last convention of the realist tales implies that field researcher has the right to give the last word in interpretation of the given culture. It may use common theoretical frameworks, prepared by the mighty predecessors, which provide certain rules of explanation. The other means could be the contrary strategy, according to which the researcher bases his accounts and interpretations on the interpretations provided by the members of a given community (Strauss 1987; Geertz 1973). The narrator is somehow the exponent of the perspective of the group that he passively observed (interpretive omnipotence). The ethnographer makes an assumption about the genuine perception of a reality by the field researcher. In order to achieve that goal, the researcher might use the ideologically neutral terminology, even technical or originating from the colloquial language (see: Becker 2003: 664, who analyzed the language of presentation by E. Goffman, see also: similar strategy in choosing the language in Smith, 2003; Prus, 1977; Prus and Irini,1980).

For example, Richardson (1994: 518 – 519) claims that the standards associated with the manners of writing have their historical conditioning also in academic papers. At the end of nineteenth century, realism as a mode of writing dominated both science and literature. Both writers and researchers of social studies used to search and describe "the causes of phenomena", "social consequences", "basic rules" which were the foundation of the social order. There were attempts to introduce the precise terminology, the contents of which would have an objective, unambiguous, non-contextual and non-metaphorical character, where the omniscient narrator-researcher adequately illustrates reality in the text.

The second type of tale is confessional according to Van Maanen (1988). The characteristic feature is the personal style of narration. Such style aims at showing that despite numerous drawbacks of both the tool and the researcher, the latter is disciplined as a scholar, susceptible to epistemological matters characteristic for social sciences. The author builds his/her authority through presenting him/herself as a humane person ("I saw what X was doing"). The "confessing" researcher adds to the realist tales, the deepened descriptions of applied methods in form of separate articles, chapters or appendices to the realist tales. They also mention their personal prejudices, drawbacks, bad habits in order to build the ironic self portrait, with which the readers may identify ("See how malicious I am, full of human flaws"). The emotional reactions of the researcher, their surprise, unexpected events they participated in, all shed light on the way the researcher tried to comprehend the

observed reality (personalized authority). Moreover, the researcher expresses a fieldworker's point of view. S/he also standardizes their access to the observed group, presents themself as a person treated as "one of us", always participating in the life of community; therefore almost invisible for the 'subjects' of the study (Van Maanen 1988: 73 – 100).

The third type is the impressionistic tale. These usually describe rare or unusual situations. They openly show the researcher's personal feelings as well as their work. Impressionistic tales simultaneously present the subject and the matter of the study, and aim to find the bond between the observer and the observed. Authors of this 'type' attempt to show their own experience from the beginning to the end, and seek to involve the reader in the observation. This researcher does not imply what the reader should think and how they should analyze their experience. Rather, the text is intended to provide an inspiration for interpretation (textual identity). Impressionistic tales develop from a series of unfolding events, often in an irregular manner, which are difficult to predict. The events are presented in a loose manner, thereby providing surprises for the reader (fragmented knowledge). The characters in the tales have their own names, faces, motives, actions (characterization). The author also works to build tension or climactic highs, often followed and countered by lows. These tales require from the author the possession of certain artistic rather than scientific writing skills. The language is rich, full of implications, and cognitive and emotional elements (dramatic control; as above 101-124).

Triangulation procedures and generalized other

Two descriptions are better than one.
Gregory Bateson

The abovementioned conventions that regulate the approach to field study report writing, in my opinion, are much more far reaching than the writing context alone. The researcher is well aware of them while carrying the research. The conventions are an essential part of creating "realness" of observed and described events and interactions during the research. The expectations of the prospective reader influence the research procedures followed by the researcher, as well as created *ad hoc* procedures, which are used to build a full and credible representation of the observed reality (see many research procedures used by Jahoda, Lazarsfeld, Zeisel 1933). The procedures are intersubjectively interwoven into the daily practice of the researcher.

In order to "re-present" the reality in an adequate and "proper" manner, the researcher, still in the field, must use a number of procedures. One part of which has already been studied by the other researchers, and the other part created *in situ* or *ad hoc*, to assure the researcher in the field that the reality described in his/her record book corresponds with the reality that exists around them; adequately situating him- or herself in opposition to "the other". During the research the researcher conventionally employs triangulation methods in order to show him- or herself as an exterior observer of the observed events, even when s/he participates in them. Originally, triangulation was a heuristic tool. The basic meaning of the term refers to the method of indicating the position of points in the field by means of the configuration of triangles: the triangulistic web. All of the angles in the triangle are measured together with one or more of the sides, and by astrological methods one indicates the coordinates for some of the points and the azimuths of particular sides.

Although the term is mainly used by geodesists in order to describe the method of measuring the position of the point on the basis of two other points in space, it should not be read literally as the use of only two or three various research methods or perspectives (Janesick 1994: 215). It implies the employment of various methods, both quantitative and qualitative (see also: Huberman and Miles 1994: 438; Richardson 1994: 522; Bryman and Burgess 1994: 104 - 105, 222 – 223; Silverman 2001: 233 – 235, Hammersley, Arkinson 1995: chapter 8). Thus, the term triangulation assumes a different meaning when compared with its original use.

Denzin (1978) distinguishes four types of triangulations:

1. triangulation of data, which involves the employment of data from various sources;
2. triangulation of researchers, which implies the introduction to the research numerous evaluators and auditors;
3. theoretical triangulation meaning the employment of various theoretical perspectives to interpret a single set of data;
4. methodological triangulation, which means the employment of numerous methods to study a single problem.

According to Hammersley and Atkinson (1995, chapter 8) triangulation is used to evaluate the accuracy of a conclusion drawn from data on the basis of other kinds of data (triangulation of data). Methodological triangulation helps to verify competing interpretations and the correctness of the juxtaposition of a given term with an indicator. Triangulation then can serve as a tool to verify the validity of connections between the indicator and the term by means of other indicators. This subsequently serves a final verification of the validity of the analysis and the validity of the conclusion on the basis of collected data. In the model of triangulation increased confidence in the outcomes of the research is its intended purpose; it could be called “increased validity” model of triangulation (Moran et. al. 2006: 47). There are other meanings of the triangulation procedure used in qualitative research. Some researchers want to show complexity of social reality and multidimensional analysis of it by using triangulation. Methods can be triangulated to show “many dimensions” of the phenomenon and to help in understanding complexity of the social world under study (ibidem 2006: 48). Other meanings of triangulation are connected with the aim of “generating more knowledge about a phenomenon”. Both qualitative and quantitative methods can enrich our knowledge of the researched field. The fourth meaning of triangulation indicates that social phenomena exist on two different levels, “those of structure and agent”. Different methods help in sociological explanation of two levels of the phenomena, one macro/meso level and one micro level (ibidem 2006: 48-49). Other theoretical perspectives such as postmodernism also “provide a rationale for the use of multiple methods to capture complexity and multiple contexts of a phenomenon” (ibidem 2006: 49).

Triangulation might is not then only a tool used in positivist enquiry but will be used in field research in an effort to obtain the credibility of data and valid and objectively verifiable conclusion. To use Van Maanen’s terminology, triangulation is usually employed as a persuasive tool in realistic tales, which aims to emphasise the realness, objectivity and authenticity of presented descriptions and research conclusions.

Case one

During my studies on the socialization of new employees in the industrial company I used triangulation of data, methodological triangulation and, in part, theoretical triangulation (Konecki 1992a). I coined the term trajectory of a new employee during the participant observation when I was employed as a worker. I observed myself in this context and other employees[2]. Moreover, I based my observation on my own experience of working at university environment during the 80's. I was uncertain during my participant observation whether my experience and observations were simply the product of my interaction with workmates and my notes merely auto-biographical reflections of my interaction. Many facts I gathered during my participant observation implied that I was treated by my workmates in a special manner (as a newcomer). My education, language I used might have caused certain repugnance and the practice of degradation that I was subjected to. I was unclear of the order of the stages of trajectory that I was experiencing. I decided then to employ yet another research method (other than participant observation), that of the semi-structured interview. I felt this would allow me to examine the stages of the trajectory of a new employee discuss examples of rituals of ridicule and the so-called "worker flirting ritual". Therefore, I employed methodological triangulation. The interviews confirmed the "realness" of the previous observation and produced an in-depth insight into them. The "realness" was confirmed because the stages of the trajectory gained a typical character, the typical were also the "guardians" of the socialization, the particular interactions occurring during the trajectory also became typical. Typologisation, which I conducted on the basis of the interviews "typologised" also the trajectory itself giving it the status of the objective representation of the reality of socialization. Methodological triangulation situated me outside the researched situation. In order to strengthen the "realness" of the description I later hired a company employee (the sociologist) to conduct a covert participant observation for me, among the office employees. I did not know whether my idea of trajectory was valid and experienced by other types of employee. It was later that I learnt from the literature that what I had done was follow the procedure of methodological triangulation, which from my perspective only confirmed my intelligence as an experienced field researcher.

In the research report, which turned out to be a realistic type of tale, I (Konecki 1992a) wrote:

> The picture of the trajectory was drawn on the basis of data gathered during two participating observations and, in this section of the paper, mainly on the basis of empirical data gathered during 123 semi-structured interviews with the new employees (61 interviews), supervisors (22 interviews), and senior staff (40 interviews). Quantitative study of the data gathered during the interviews is not the most important purpose of the analysis in that part of the paper. It rather aims at keeping the order in a

[2] Trajectory involves the process of entering of the new employee into an industrial institution, when the individual adjusts to the requirements of the job and the organizational culture and he is the subject of the introductory (initiating) procedures into the work and the social relations at work and when the new employee also influences the ones who introduce him to work (Konecki 1992a: 13-17). Term 'trajectory' is used here as a metaphor showing the temporal and processual character of workers' socialization. The metaphor shows that the process of socialization has a particular purpose and its ups and downs, and also that the shape of the process may vary, also unexpectedly for the participants (the problem of scientific metaphors is further discussed by Lakoff and Johnson 1988: 70, 98-102 and other; and Richardson 1994: 519, 524-525). One may certainly use yet another metaphor instead of trajectory, for instance the organistic metaphor known as "adaptation", but then we would concentrate on other aspects of socialization of new employees.

> qualitative study of empirical data, thus it has an ordering function, sometimes supporting the qualitative analysis while forming categories and hypotheses. (p. 17)

One may observe that the objectivity and realism of my analyses is supported by the use of various types of data (triangulation of data), the use of various methods (methodological triangulation), where the numbers are particularly persuasive. Although I stated earlier that the quantitative study was not the main purpose of the analysis, I underline that it corroborates the empirical data as well as the categories. Therefore, the numerical sequentiality, which is mainly the following of the "majority rule", constructs the sequentiality of the presentation of the categories and subcategories. Being a qualitative researcher I thought at that time that I could not admit openly that I regarded quantitative data as important. They were thought necessary in order to confirm the objectivity and realness of qualitative data, from part of which I resigned as they had no quantitative significance (only one or two to be found), therefore they were less real from the perspective of the researcher. It was difficult to transform them into analytically useful typology of categories. "The majority rule", coming from the scientific "generalized other" indications, was useful while deciding on the "realness" of particular categories. Moreover, the manner of presentation of the report was appropriate to the manner of what I had done in the field, which means the report shows the realist and positivistic inclinations of the researcher.

The passive voice and third person narrator are often used in such presentation. The number of interviews and methods can have a certain persuasive power. In my writing I attempted to convince the collegial readers to accept the realness and validity of my data and conclusions. The triangulation of data increased the internal validity of data presented in the report and their explanations. In this case, I also treated the validity as a social construction that is created in communication. I took into consideration who would receive the research report and who would interpret it. The techniques of research and presentation construct the validity in communication with the readers. The social construction of validity "is decided through the argumentation of the participants in a discourse" (Kvale 1996: 245).

Similar significance is given to the methodological note regarding the research into the ritual of "worker flirting" (Konecki 1990). I coined the term "worker flirting" during the participant observation. I was not certain at that point whether my experience and personal inclinations did not provoke the phenomenon. The realness of the results and their representation seemed to me to be particularly in danger. Therefore, I decided to examine the problem by means of interview (carried by the pollsters) among the same group of employees I had observed previously. Again, I wanted to place myself outside the researched situation and far from the problem as possible. Obviously, I do not mention these doubts in my report, showing instead considerable methodological consciousness, which works to reassure the reader of the realness of the phenomenon described. The confessional tale was not my favourite type of narration.

Here is what I (Konecki 1992a) wrote in a "realistic" methodological note referring to that part of my study:

> The data gathered during the interviews broaden our knowledge on worker flirting. They provide new data for comparative analysis allowing us to verify the hypotheses or form new ones during the research. The data from the interviews have been separated here, as the way they were collected

> differs considerably (the situation of the interview) from the way of collecting data during covert participant observation. During the interviews the respondents reveal the common data, which they witness and which cannot harm them at work. Whereas in the participant observation the researcher gains data, the revelation of which the respondents would be afraid of, or some of the data which might be interesting for the researcher but which the direct participants of social life regard as obvious and might not be aware of. The comparison of those two types of data, and often the two dimensions of social life (public and informal), allows a better understanding of the conditions in which the particular categories and hypotheses exist. The data from the interviews show the knowledge, which is public property. The contrary data were obviously collected during the participating observation, where the information have also the public character, but limited to the functioning of the informal level, which was not revealed to the "strangers" (the pollsters) to the company. Nevertheless in the interviews it is allusively highlighted, either in a concealed or open manner, to the particular phenomena, which were noted during the participant observation. (pp. 122-123)

I built the authority by showing a wide empirical basis for the proposed hypotheses. The representation of reality is then shown to have an inductive and grounded basis. Thus implying that it is rather impossible for the researcher to make-up the phenomenon of "worker flirting". The data from the interviews point "allusively in a concealed manner" to the behaviours observed during the participating observation. The author authoritatively underlines that the data gathered during the interviews allow for the better understanding of the conditions of functioning of particular hypotheses. It creates the opportunity to use the comparative analysis, which is yet another method of assuring the validity of the conclusion. The realness of the description is then confirmed by the usage of triangulation of data and methodological triangulation simultaneously. "The generalised other", whose part is the reader with his standards of evaluation of the research report, reveals its presence during the field research (through methods and procedures), in order to find the final confirmation in the research report. The author can use the passive voice to annihilate somehow his/her presence in the research and to show that the research was carried by the collective author. This tendency is emphasized by the usage of personal pronoun in plural, which allows one to identify "the generalized other" with an individual researcher. The collegial readers in this case are "the generalized other" (together with the researcher), who know the definitions of the terms such as: hypothesis, pollster, respondent, public and informal dimension of social life, covert participant observation, etcetera. The researcher solved the discrepancies between the analysis of the participant observation and semi-structured interview data through their use of the procedure of triangulation of data and methods. It is somehow the explanation *aposterori*. The author claims that there exist various meanings ascribed to flirting in the workplace (ibidem 1992a: 130). Flirting might be treated by the participants as the ritual of dependence indicating the centre of discretional force in a given workplace (such interpretation was reconstructed on the basis of data from participant observation) or as a form of play which breaks the monotonous routine of work (such interpretation was reconstructed on the basis of data gathered during the interviews). During the research I was disturbed by the altered and contrary data, but still in the report which "objectifies the observed reality" the anxiety was not expressed. At the end of the report I state authoritatively: *"The researchers allowed to extend the issue of flirting and find yet another meaning (and function) of flirting,*

meaning of breaking the monotony of work and keeping the social contact" (ibidem 1992a: 130; see also Konecki 1990).

In order to underline the realness of my observation, I disclosed myself as a subject of observation in the quotations from my observational notes. There exists a certain trace of a confessional tale, but it was not employed consequently. The quotations of the studied "subjects" are supposed to invoke the persuasive force of the report, to underline the "credible" representation of reality, because we can see for the moment the "tool of observation", which uses the similar language as the observed people. It increases the realness and credibility of the description: *"I talked to a new worker (aged 21, 1.5 months of work in the transport department) about an incident on the second shift when he had been beaten up by a woman-worker (an assembler in another bay) 'What have you messed about with that woman?' I asked. He said 'Well, it went all right at the beginning, but then she went nuts ...' 'Were you pressing her too much?' I asked. 'Well, you see, I could have hit her, even killed her, but what for? ... Anyway, she was stoned too"*". (Konecki 1992a: 120; Konecki 1990). The researcher confirms that he was very close to the described events as a subject and the observation tool, that he gained good understanding of the researched field and the people achieved "intimate familiarity" (the concept by H. Blumer 1969).

Case two

Let us present here the field research conducted in a different cultural context, in Japan, where I researched the organizational culture of Japanese companies. Here is what I wrote in the introduction to the book, which is the form of the research report (Konecki 1992):

> To be in the center of events. To learn about the fact on one's own. To observe while participating. The experience is for the author of this book the source of knowledge. The author spent most of the time in Japan. He got familiar with the organizational structure, work conditions and the customs in the Japanese factory, by taking the role of a regular employee. He was a worker and an academic at the same time. Becoming "one of them" he gained a vast source of information, he could obtain the first hand knowledge. It then, along with the other research methods and tools, became the basis of this book. Perhaps it makes the book different from the hitherto writings on the subject… (ibidem: 7). The author spent a month in the Maekawa factory, all the time being among the workers. He carried the observation and the interviews as well as hundreds of conversations; he also participated in numerous meetings (kaigi) during working time.
> The other company where the participant observation (covert) was carried out was a small company Shi…
> The author also carried out research in the sub-contracted, but formally independent transport company Iyo…
> The author worked in Shi and Iyo companies as an unqualified blue-collar worker… (pp. 17-18).

The above statements serve as an introduction to the realistic tale. The narrator uses the third person and, what is more, he employs the persuasive strategy in order to show, that the author was in the centre of events and work processes within the companies, and that the research was meant to be in-depth research (in a good range of companies). There also appear certain elements of a confessional tale. The author was 'one of them', therefore despite different skin colour, education,

upbringing, he experienced the world simultaneously and in the same space, in the same manner as the observed (he possessed the "intimate familiarity"). Moreover, he employs the practice of raising his academic prestige by displaying the uniqueness and innovation of his work, which he modestly calls "different from the hitherto writings on the subject". The author generally adjusts himself to the standards of a realistic tale. Does it mean that the research was not problematic and the reality represented in the report was also certain? In fact, there were problems with the description and construction, and that these problems were rudimentary, associated with the interpretation of the definition of the situation of the researched people, for instance on the understanding of work. It transpired that the statements concerning work and the company, quite often negative, did not correspond with the activities of the Japanese workers. By negating the sense of their work, talking about lack of bond with the company, criticizing low salaries, after-hours (data from the informal interviews done during observation) the workers in the actual activities showed a certain loyalty to the company and involvement in work, a will for cooperation, fast and accurate work, and very often they worked voluntary after-hours and did not leave the companies. From the perspective of European culture, one may use the colloquial expressions such as hypocrisy in reference to the lack of compatibility that they presented. (The sociological distinction between the formal and informal organization in a Japanese company was useless in that case because of the intermingling or "overgrowth" of those two dimensions in every possible communicative situation). Nevertheless, the researcher who objectifies the researched reality cannot accept such common interpretative expressions. Therefore, the researcher objectifying his knowledge about reality (in the text the "realist" narrator) approaches the problem in more conceptual manner (Konecki 1992):

> one should refer to four notions present in Japanese culture: omote and ura as well as tatemae and honne. Similar to the comparison of Latin words recto and verso, the terms omote and ura are the contradictory concepts. Speaking about omote and ura of a particular object we mean the two sides of it. Omote-dori is the main alley whereas ura - dori is a side street. Omote-muki refers to something public, ura-muki suggests something private, closed or personal. Omote is visible, unlike ura. The meaning of behaviour varies according to whether we present it in ura or omote dimension. Such participation in these two dimensions determines the meaning of activities and statements not just the statements or activities separately.
>
> There are two important definitions for Japanese culture associated with omote and ura: tatemae and honne. Tatemae in Japanese architecture means "erecting the gable", so it refers to the external. The dictionary definitions explain tatemae as a certain type of rules and regulations regarded as natural and obvious. Tatemae then refers to the concepts created by men on the basis of consensus. The term tatemae implies the existence of the group of people in the background, who agree to the earlier stated concept.
>
> The notion of honne, in opposition to tatemae, refers to the fact, that individuals, who belong to a given group, even if they accept tatemae, they have their own motivations and opinions, which sometimes differ from tatemae. These individuals often keep the particular motivations and opinions for themselves. Tatemae (notions) appear in the omote dimension (official), whereas honne (private) is presented in the informal interaction dimension, which is ura (Doi 1986: 23-47).

> The contradiction, which occurs between the statements concerning work and the actual activities is not the matter of logical contradiction, but the question of various dimensions of presentation of oneself through the individuals. The omote and tatemae dimension, in other words, official and conventional dimension refers to work which is observable for the others and undergoing the social surveillance, thus the observable workers' activities. On the other hand ura and honne dimensions, which represent unofficial and personal motivation would be reserved for the disclosure by the individual the personal opinions only to the closest acquaintances, or complete strangers (such as pollsters), when the official social control is abolished and replaced by values such as empathy and openness…
> *The personal opinions cannot be officially revealed (for example to the supervisors at workplace) or be translated to the language of actions, because in that situation they undergo the sanctions relating to the common notions and officially rationalizations of the actions involved in typical cultural formulas (tatemae dimension). Therefore behind the verbal declarations regarding the attitude towards work there does not have to be a permanent behaviour regulators. Perhaps the regulators lie in the organizational culture of the Japanese companies, where there exist strong socio-cultural control elements of the workers' actions (pp. 59-62)*

The researcher solves the problem according to the accuracy, objectivity therefore the realness of his observations through the employment in the analysis of the "theoretical triangulation". The problem of reality is solved through the use of the procedure. The researcher borrows from cultural anthropology (which studies Japanese culture) the terminology, which helps him to find a certain standard and regularity in the observed reality. It is unthinkable for the academic reader from the circle of Western culture (the "generalized other") to leave any ambiguities and incoherencies in representation of a certain reality (the rule of coherence at the description level). There have to appear certain meta-regulators of the behaviours that allow egress out of the description of the particular context for the behaviours which for "generalized other" might seem inconsistent. Each culture is coherent to a certain extent, it cannot be chaotic, accidental – this is the field researcher's assumption or his idea of the assumptions of the prospective readers of his book. He needs to find certain typicality of behaviours and of its conditioning. If during the research he cannot find the triangulation procedure that will allow him to liberate himself from the fear of the incoherence in the observed reality, he employs the necessary analytical procedure, which still remains the *ad hoc* procedure used during analysis, which is the period of intensified lack of understanding of the described reality. "Triangulation of data" (data from interviews and polls and from the participant observation) merely confirmed incoherence of verbal declarations when compared with observed actions. It is contradictory to the general sociological assumption, which implies that the subject of an action frequently attempts to be consistent and acts according to declared and accepted motives (the rule of coherence at action level). The sociological analysis though does not allow to present the researched reality as coherent, the researcher-sociologist then uses the anthropological theoretical perspective in order to give the reality a character of coherency. Therefore, the reality refers not only to its representation. The analysis then (which is the period of intensivefear regarding the realness of the described reality, the coherence of actions and statements) is the elongation of the construction of reality, which still takes place during the research, and which is expressed by the triangulation of data (or even earlier before the field study, in the intersubjective process of shaping the "generalized other"). The use of the emic terms *tatemae* and

omote, which the researcher turns into the general and explaining definitions (etic terms), then eradicates the fear and anxiety of the researcher (or cognitive dissonance), which appear in association with the discovery of the incoherent reality, even lasting for a short period of time. The procedure of theoretical triangulation, no matter whether the researcher realizes it or not, is beneficial for his/her identity, as it rejects the common interpretation of the statement as full of hypocrisy and again puts the researcher outside the described and interpreted reality as a member of an academic community. Paradoxically, for his explanations the researcher uses the other common definitions, such as *honne* and *tatemae*, but with the theoretical sanction of cultural anthropology. The view of the incoherent reality from another perspective gives coherence to the reality, which may satisfy the academic reader, or to a certain extent, the social science reader. The non-professional reader would be least satisfied because of over-theorising of the explanations (and/or lack of the description of the extraordinary situations), but he is not in the centre of realistic attention of a bit lost narrator who follows the "coherency" rule of his generalized other from academia world.

Discussion on triangulation – scientific and common sense method

The triangulation process obviously has a deep epistemological meaning and one may analyse this by means of exploring aspects of the philosophy of science. Our analysis uses only certain motifs appearing in symbolic interactionism and ethnomethodology, which were particularly helpful for the analysis of reader's, researcher's and academic community interactions. The process of "realizing" (making real) the observed social world does not involve only, as Van Maanen (1988) believes, the manners of presenting the world in research reports. It starts at the very beginning of field research and continues during the analytic process, when the researcher "situates" him-herself in position to the observed "other", interacts in the field and tries to achieve intersubjectivity of his results *in situ* and in the academic world. The language of reports is an outcome of action, thus it does not exist separately from the interacting actors and audiences of the author and his actions in the field. These three intellectual processes intermingle with each other and form the complexity, though they may be analytically discerned (see figure 1). The usage of the procedure of "realizing" the researched social world, which is triangulation, seems to be a common practice in field research and it is to build the quality of qualitative research (see: Denzin 1978; Janesick 1994: 214 -215; Morse 1994: 224 - 225; Huberman and Miles 1994: 438; Hammersley, Arkinson 1995: chapter 8).[3] The

[3] Occasionally ethnographers attempt to receive verification of the realness of the description and conclude on the basis of ethnographic revisits in the field. It might resemble the rule of replication of research, derived from experimental sciences, at the same time the assumption, that what is repetitive is more real than something sporadic and idiosyncratic. The repetitiveness of research is supposed to give the opportunity of gaining intersubjectivity in field research, as other researchers are able to observe the same conclusions on the basis of the observation of "the same or similar" actions. It may be said, with a certain amount of arbitrariness, that "revisits" are a particular form of triangulation of researchers, who enter the field at various moments, and the theoretical triangulation, because they attempt to reconstruct the theory by using new or other theoretical inspirations in new moments of the field observation. "*An ethnographic revisits* occurs when an ethnographer undertakes participant observation, that is, studying others in their place and time, with a view to comparing his or her site with the same one studied at an earlier point in time, whether by him or herself or by someone else. This is to be distinguished from ethnographic reanalysis, which involves the interrogation of already existing ethnography without any further field work." (Burawoy 2003: 647). Revisits though are not accurate replications of research carried by the same researcher or other researchers in the past. The purpose of research revisits, according to M. Burawoy, is either the rejection of theoretical conclusion based on the research or their reconstruction.

procedure in sociology has it's beginning in the Chicago School where since the start of sociological ethnography researchers used multiple methods (Deegan 2007: 19-20), as for example life history, documents, conversations and observations. Paul Cressey used in his research on "taxi – dance halls" records from observations and records of social agencies (triangulation of data), he used also "reports from different observers upon their contacts with the same individual made possible a check upon the consistency of document obtained" (Cressey 1932/2008: XX). We can say that it was triangulation of researchers. Similarly Nels Anderson (1923/1965: XXVIII and others) collected the life histories and conducted observations by himself in his research on hobos in Chicago. He used many kinds of data from many sources and researchers: life histories, data from observation, statistics. So there were used triangulation of data, methods and researchers too.

In qualitative methodology the use of this procedure is often justified by the scientific requirements of gaining "reliability" (it is an opportunity to repeat the observed behaviours together with the explanations, which means the accuracy of the "measurement" or classification), validity (the answers to the question whether the explanations are suitable to presented quantitative descriptions of people or situations), credibility (the degree of probability of occurring of the observed phenomena), or the opportunity of generalizability (Janesick 1994: 216 - 217; Dey 1993: 253 – 261; see also: Silverman 2005: 220 - 223; Kvale 1996, chapter 13; Gobo 2008: 27 - 28). Researcher searches for information, in order to determine empirical constants (see: Garfinkel 1967: 265). Rational requirements lying at the basis of the ways of creating "reality" and "realistic" representation of social reality in research reports are not specific merely for science. Let us look closer at the concept of "triangulation". It is a method of rational objectifying of the observed reality by the emotionally and intellectually imperfect, very often lone, researcher. Researcher attempts to adjust himself to the rules of scientific rationality in order to present the observed reality objectively and intersubjectively. He tries to employ the means and aims to make them appropriate to the rules of formal logic. Moreover, he attempts to achieve semantic clarity and precision, and to adjust (agree) the definition of the situation to the existing scientific knowledge (see: Garfinkel 1967: 267-268). But while participating in the everyday life of the observed, field researchers cannot employ only these rules. One may say, that the objectification of presentations of reality occurs through the application of yet other rules, so-called "procedural rules" (as above 1967). The procedure will be rational (and the presentation objective) when the researcher evaluates the rightness of his judgments, observations and conclusions referring them to the procedural rules. Following Garfinkel (1967: 265-266) we may distinguish, according to importance for our purposes, two classes of rules regarding the correctness of common conclusions: *"cartesian rules"* and *"tribal" rules*. Cartesian rules state that the decision concerning conclusion is correct when the person acts in accordance to the rule ignoring other people's views. Tribal rules imply that the accuracy of the decision depends on whether while making a decision one takes into consideration certain social obligations. A person regards his or her decisions as right or wrong depending on the evaluation of the people with whom for various reason he or she wants to live in unity (ibidem 1967: 265). Field researchers act according to both types of rules. Tribal rules, in his case, refer not only to the people and social groups observed in the field and relations between them and the researcher, but also to "generalized other" often personalized in the academic companions.

Lostness of the field researcher usually results from his state of being "a stranger" (or a newcomer) in a certain culture (see: Schutz 1944; Ziolkowski 1981:

170 - 216). Before he gains the necessary knowledge, typifications, language adequate to the culture, he feels uncertain of the "realness" of his observations (the possibility to repeat the previous observations and accuracy of explanations to the observed actions). Moreover, with him he brings different values and a "system of relevancy" to a new culture, which may have a negative influence on his interactions and the manner in which he perceives and interprets the observed reality. The gathered data are actually co-constructed together with the observed subjects *in situ*.

What does each "stranger" studying a new environment on the everyday basis do? Basically, he examines the "realness" of the perceived (often only by him) reality by means of various methods. He performs a triangulation in everyday life, which means he employs the procedure. By reducing his anxiety and doubts he searches for the confirmation for his observations in the observations of other people, he searches for written materials (also quantitative), which refer to other phenomena that he experiences. Therefore, he performs triangulation of methods and data in order to gain a complex image of reality. He searches for "accounts" other than his own, made by experts, credible informants, social control agents etcetera. He also performs a "theoretical triangulation" on the level of common knowledge, searching for various generalized points of view on the observed and experienced reality. It enables him, through making comparisons, to achieve a more general perspective (it is usually a kind of "enlightment", which we usually dread to call a "discovery") and typification of reality in its diversity. Apparent, or in the initial stage contradictory interpretations find their meta-pattern of interpretation. Every stranger does it regardless of which new subculture or culture he enters, whether it is Polish, Japanese or American culture. A common procedure of triangulation allows for an active reconstruction of reality, often unclear and threatening for her/his cognitive system. The triangulation procedure allows for double or multiple comparisons (see: Bateson 1996: 95 - 121). It often happens that combining the information from other sources not only verifies, but also gains a wider pattern of interpretation, which eventually appears to be a confirmation or negation of the "realness" of the observed facts and phenomena; that is verification, but in a logical sense (coherence rule).

Triangulation is also a necessary procedure in a number of professions and types of work, such as so-called "headhunters" (recruitment consultants). Though the procedure is not referred to as triangulation, it is widely employed. "Headhunters" while recruiting employees from other companies work on, for instance, references, education, qualifications and/or competence checking of the candidate, they examine his communication skills etcetera. They employ a variety of methods and data. They contact numerous people that the candidate encountered in the past, either at work or in private life, and also use the existing data banks in order to check and receive a "real" description of the candidate's identity. They employ qualitative methods to evaluate a candidate (opinions of various people) as well as quantitative (they check, which type of opinions prevails, that is they use the "majority rule"). They often hire other consultants (psychologists), in order to confirm their information (researcher triangulation), or they hire experts to explain, with the help of their perspective, behaviours or career path of the candidates, that is they employ theoretical triangulation (see: Konecki 1998).

The examples of use of triangulation in everyday situations and the professions that use it are numerous (e.g. jobs of police officers, attorneys, intelligence, industry intelligence, strategic or stock market investor, professional matchmaker, journalist, etc.)[4]. But this procedure is also used in our everyday life, if we want to examine the

[4] It would be interesting at this point to perform empirical field research into the common employment of

"realness" of the perceived common world.[5] A field researcher participates in the world and his/her situation in the field is not exceptional. Using the procedure of "realising" the world, s/he acts according to the rule of evaluation of the actuality of the observations, according to the assumptions of his "generalized other" that gives him/her the identity of being rational. The researcher might be wrong as to the adequacy of representation of reality employing even accurately triangulation in the scientific sense, as well as other participant of the social world, such as police officer, attorney, matchmaker, recruiter or while checking the future business partner, can be wrong in his/her/our "research". There is though one issue that the researcher might be sure of, that s/he used a "realising" procedure for the observed world, which potentially convinces the reader of his/her "representational" tale, as to the realness of the described world. Nevertheless s/he cannot state that the "real" social world is exactly as s/he observed, described and explained it. What is the most realistic is the procedure that he previously used, that is triangulation.

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triangulation procedure. It would generally aim at studying the technical aspects, range, necessary and intervening conditions of effective employment , moral evaluation concerning it, sequence of particular types of triangulation, sequence of actions in the framework of a certain triangulation procedure etc. in the various types of work in the abovementioned jobs and professions.

[5] Triangulation procedure is commonly used in everyday life unintentionally. It is common for example for matchmakers. They check thoroughly who the candidate for marriage is. The basic question is the question of their "real" identity. Without triangulation of methods, data, and often triangulation of "researcher", or theoretical triangulation, we would not be able to complete the task.

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Who Allowed You To Observe? A Reflexive Overt Organizational Ethnography

Abstract

Observing people working within organizational contexts through time creates epistemological issues, more so when doing it overtly, with top management's official agreement. Power relations as well as hierarchical structures strongly influence the way people view the observer and interact with her in organizations. Those interactions also partly depend on his personal background – sex, age, professional position and so on. Following a reflexive approach, my objective is here to better grasp how top management's agreement to the ethnographer's entry on the field may influence both the way workers from differing hierarchical levels behave with her (and thus affect her observing conditions) and how he may analyse his ethnographic notes to develop scientific sociological results.

Keywords

Ethnography; Reflexivity; Organization; Work; Epistemology

The ways in which an ethnographer is allowed to observe people through time, as well as ways she may interpret those observations, are widely dependent on reciprocal social positions negotiated between the observer and observed. Observing young men in a popular area (Mauger 1991) differs from doing one's research among French grand bourgeois and aristocrats (Pinçon and Pinçon-Charlot 1997). Those interactions are locally co-produced by actors, partly influenced by their reciprocal positions in the social hierarchy. Rather than developing a clinical talent or an empathic capacity in order to inspire trust to reluctant observed people[6], ethnographers have developed a *reflexive* perspective (e.g. Burawoy 1998, 2003; Schwartz 1993; Taylor 2002). A *reflexive* approach insists that ethnographers become aware of locally co-constructed *frameworks* (Goffman 1974), so they may

[6] Isabelle Baszanger and Nicolas Dodier have beautifully shown that through the sixties and the seventies French ethnographers have progressively transformed their approach of participant observation from a *"clinical talent linked to an empathic attitude"* to a *"reflexive process"* based on successive interactions (Baszanger and Dodier 1997) (my translation).

either play with them to improve their observing conditions, or take them into account when analysing their *collected data* (Glaser and Strauss 1968).

Observing people at work is no different. One has to find a way between *involvement* and *detachment* (Elias 1956) when entering the field as well as when analysing ethnographic notes. However, observing people working within organizational contexts - enterprises, associations, administrations or political parties - creates some specific epistemological issues, particularly when doing so overtly with top management's official agreement. Power relations as well as hierarchical structures strongly influence the ways people view an observer and interact with them in organizations, partly depending on the observer's personal background - sex, age, professional position and so on. Organizational life is produced daily through constant games between workers from differing hierarchical levels. Being present with top management's help implies that observers become part of a specific interdependent relation developed by workers. My objective in this paper is to better grasp how top management's agreement to the ethnographer's entry in the field may influence the way workers from differing hierarchical levels behave towards the ethnographer and the implications this can have for the act of observing and analysis of data gathered. This issue will be mainly addressed through my own experience as an overt ethnographer in two large French private insurance companies, *Hermes* and *Mercure*[7]. I will discuss how social relations I have co-produced with both operational and human resources top managers and first level workers have strongly shaped both the kind of observations I was allowed to make and some of the sociological intepretations that I ended up making[8].

Getting access to a closed place: ways and constraints

Organizations are closed places to which access as an ethnographer is difficult to achieve. One must either be employed as a regular worker over a long period of time, or get top management's agreement to observe as openly as possible over time. Both methods have proven useful in studying organizations. Covert observation is specifically powerful in revealing workers' ability to resist management's rules (Roy, 1952), while overt observations help reveal wider organizational rules and regulations (Burawoy 1998; Strauss 1992; Rosen 1991).

I will not discuss epistemological advantages and limits of both covert and overt positions. I will focus instead on what it may mean to get access to this closed place and be allowed free access to people *in situ*, two of the major elements that define ethnography, as stated by Atkinson and Hammersley (1995/1983):

> in its most characteristic forms it involves the ethnographer participating, overtly or covertly, in people's daily lives for an extended period of time, watching what happens, listening to what is said, asking questions – in fact, collecting whatever data are available to throw light on the issues that are the focus of the research. (p. 1)

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[7] For confidential reasons, those companies' real names will not be mentioned. This 18 months ethnographic research was led as a PhD student, from October 1996 till March 1998, at Hermes and Mercure, two major French insurance companies. I observed work activity in two similar administrative-technical departments composed of about 100 workers each and led interviews with first-level employees, middle managers and functional and operational top managers. Based on this empirical work, I defended my PhD dissertation in 2000 and published several articles in French academic Journals (e.g. Buscatto 2001; 2002).

[8] This article is based on a working paper first presented at the *American Sociological Association Congress*, San Francisco, August 13-17, 2004.

Getting such open access is indeed often difficult since most top managers experience this presence at best as an investment to be made profitable, at worse as a risk to be tightly controlled. How does one get top management's interest in one's work while being allowed to observe as freely as possible over time? Through my own experience at Hermes and Mercure, I will try to present some reflexive principles which may guide organizational ethnographers in such an attempt.

A fashionable topic, a concrete agreement

It gradually appeared that I had been accepted in this organization mainly because my research goal had been considered to be interesting by both Human Resources and operational managers. This had supposed that I transform my academic research interest – *"organizational socialisation in big private companies using a comparative approach"* [9] - into acceptable and understable managerial terms. Following Callon's expression (1986) I had to *problematise* research goal in appropriate terms for the companies I wished to study. It was meant to both get top management's interest (and thus agreement) and construct a common frame analysis with them (and thus remain accepted in the long run).

When sending letters to big organisations, I had first translated my academic research question into operational terms: *"organizational learning conditions at work".* I presented my research as *"easy to lead"* and as *"an experimental research"*. To my big surprise, three insurance companies showed interest, including Hermes and Mercure! Other letters got lost in recruiting services or were filed without even being answered.

As I found out later, once inside Hermes and Mercure, my problematisation work was successful in companies which were already sensitive to such a question: I was part of a fashion trend which had permeated Hermes and was developing within Mercure. *"Organizational learning"* had become a trendy topic in the French business litterature: books were coming out; articles were published in the professional management press; consulting groups were offering organizational learning principles. And *"organizational learning"* had become a managerial issue within the insurance business. I later discovered that organizational groups had recently been created over this topic within Hermes and among insurance Human Resources specialists (such as the Mercure Training Manager) within the Insurance professionnal Union (*"Fédération Française des Sociétés d'Assurance"*). Managerial experiences had even started to be led in some of Hermes innovative departments – *"learning by distance", "tutoring", "quality groups"* - and were coordinated by the Hermes Human Resources Department. Those companies had also developed a heavy rhetoric on workers' *"participation", "satisfaction", "motivation", "ability to change and learn"*… Translating my research objective into a management issue had caught their attention and I was first received by the Hermes Training Manager Head (the first to answer me) based on this potential reciprocal interest. I had then to be accepted as an ethnographer.

My first (and only) meeting with the Hermes Training Manager Head before entering the field consisted of demonstrating that a deep and open research could

[9] All quotations have been translated from French to English. Unless stated otherwise, I quote expressions/sentences which were either written in documents (including mine), or heard during observations or interviews.

help them to analyse organizational realities which seemed to escape them. As explained later by this first interlocutor, the topic, even if fashionable, was perceived as difficult and complex and a long-term scientific research had then been viewed as one experimental tool in this managerial process. I was then allowed to observe over a long period of time (one year and a half) in my own way, i.e. with a relative freedom of action, once access to each field was obtained. In return for this authorization, I was expected to respect confidentiality within and outside the organisation. My freedom of action was compensated by my public silence: all published articles, such as this one, use pseudonyms, which allows me to present my sociological results with no restraints. A concrete agreement was also settled: deliver oral and written restitutions to both local workers and human resources management. The same problematisation and negotiation process was repeated each time I entered a new operational field: operational managers were chosen by the Human Resources Manager for their training investment; confidentiality was required from me; I was asked for a specific written and oral presentation of my results. Compared with Hermes consulting fees standard, money given to me was defined as expense reimbursement – which it indeed was since I was financed by an academic scholarship - rather than as fees: the cost of my 18 months stay was not even equivalent to a week's consulting fees!

Well-founded fears and interests

Top managers belong to a social organization and accepting a researcher doing a survey supposes that the latter may correspond to an organizational need or resource. If not, risks are considered too high to let an observer in. We follow here Everett Hughes' (1971) idea that:

> the fears which lead people to make it difficult for investigators are often enough well-founded, more than that, they lie in the nature of social life. (p. 436)

For managers I met, risks were obvious (and often expressed as such with me when they felt more at ease). Being left alone, the ethnographer might discuss *taboo* issues, raise tricky questions or invite workers to raise unwanted problems. The ethnographer may also give a negative impression of the top manager's ability to manage when delivering sociological results or when discussing with other top managers. They may even give confidential information to competing top managers within the organization. As beautifully demonstrated by Melville Dalton (1959) in his ethnographic work on managers, management is not one unique homogeneous group but is constituted of several people defending differing interests, situated at different hierarchical levels, located in different buildings, holding several functions. They tend not to want to give other managers reasons to criticize them.

The advantage to a manager of allowing a researcher to work in his organization is often more difficult to grasp. In my case, I was allowed to do ethnographic work first because I was studying a political question – *"organizational learning"* – and in a novel way. As they sometimes told me, friendly operational top managers thought they might get some symbolic reward in looking for new ideas and enhance their image as innovative managers. Training Managers were already active in finding new ways to answer such a political issue. But the reasons one may be accepted are sometimes much less tangible. One top manager may be happy to discuss her organization with academic outsiders without necessarily paying much

interest to the researcher's final results. She may be interested in adding to her prestige due to the positive image of academics in her company. It may also happen that a top manager is an ethnographer's friend and is happy to help him in his job (Barley, 1990)! The reasons a researcher is allowed to enter the field will influence the way their position develops within the organization and the way in which they are able to negotiate their position over time. I will thus discuss how my concrete agreement with top management influenced my relationship with the managers that I have regularly met through my research.

Being trapped in difficult roles as a constant risk

As discussed by Paul Rabinow (1977) - whose difficult and forced entry in a Moroccan village highly shaped the way he interacted with villagers and the strategies he had to develop to get accepted - being allowed to lead an ethnographic project is only a first step in entering the field. Researchers have to evolve in a tricky environment since pressures, ambiguities and difficulties are always coming up in the way of the observer's goal to work as freely as possible (Schwartzman, 1993). How may one conceptualize such realities in an organizational context in order to maintain the requisite freedom of action over time and use it to develop original results?

Identifying and co-producing adapted frameworks

A very useful concept to analyse social positions co-constructed by top managers and first-level employees together with the researcher is what Goffman (1974) calls *frame analysis*: that is, the analysis of the way social experiences are organized through interaction. Any work situation implies that actors define, interpret, *frame* a situation, whatever its reality - e.g. dealing with a new file, talking to a customer or relating to an outsider. Far from being an open situation, framing is performed under certain constraints. Most experienced situations have already received shared and stable collective definitions. This is what Goffman refered to as *primary frameworks,* whether *natural* or *social*, and these cannot be easily ignored. Ignoring these frameworks risks negative consequences such as exclusion, being laughed at, or misunderstood. However, those *social frameworks* evolve through actors' daily activity. That is people ongoingly transform primary frameworks through interaction. One may also observe conflicts between frameworks when actors differ in their ways of interpreting the current situation.

Observers as well as observed workers define their relative positions through interaction, this definition being partly influenced by available organizational frameworks. Through all his behaviours, words, actions, a researcher may participate either in reproducing current frameworks or in transforming them over time. Analysing those games and experiences is what is called distancing or reflexive work. I will here discuss frameworks which were co-produced with top managers throughout my field work, during which my constant objective was to be as left alone and free as possible. These analyses were not ready-made when starting my work, but have been produced throughout my research when attempting to formalize many of my interactions with top managers.

Are you risky? Are you useful?

Even if my relative freedom of action seemed to be part of the initial agreement with top management, it was in fact constantly threatened and negotiated in my different interactions with operational and functional management. Two types of pressures repeatedly emerged in my daily experience.

On the one hand, questions and requests were put to me to provide usable data (on people, on other parts of the organisation, on research usefulness). Here is one example of many that I experienced during my research and which occurred on December 17th 1996:

> After only a few days doing fieldwork in a new administrative department, and while I was observing a middle manager for a full day, I happened to attend a monthly department meeting which gathered all middle managers, the two operational top managers and a few functional specialists. Once the meeting was finished, a social gathering, *un pot*, was organized to celebrate the event. Seeing me in the crowd, the two operational managers very soon came to speak to me. The head of the department then asked me *"What do you think of our department? How was my talk, do you think it was effective?"* I tried to politely decline to answer, saying I was just starting my observations and that, anyway, I would wait till the end of my work to give some sociological results. He insisted, saying he just wanted to know my *"first impressions"*. I said *"I was not interested in people, but in functioning rules"*... He then asked me *"since you study training, why are you attending such a meeting?"* I did then remind him that *"my topic was not training, but learning at work, organizational learning"*… which soon ended our conversation and enabled me to resume my observation of the middle manager I had planned to observe that day!

On the other hand, my presence was regularly considered to be risky. I was often (and nicely) asked questions such as: *"Who did you speak to?", "Who did you (or will you) inform of your results?", "How is confidentiality ensured?"* These were some of the questions that signaled to me the threat I posed to the individuals that made up the observed organisations. In short, I was considered to be politically risky.

Both pressures could sometimes create some very paradoxical situations. I was supposed to both inspire their trust as a serious researcher (and thus be allowed to continue my work) and limit answers to their questions in order to ensure my confidentiality clause and my ethical position (and not to be kicked out of the field)! In other words, I had to navigate between two negative frameworks which might be attributed to me and prevent me both from observing in the long run and doing it as openly as possible: the *"immature trainee"*[10], lacking seriousness and being impossible to trust; the *"professional consultant"*[11] who may use all those observations in too efficient, and maybe dangerous ways. As will be shown now, what happens to the observer can also be highly dependent upon the resources that s/he starts up with and ways in which s/he uses them to evolve within a given organizational context.

[10] My expression.
[11] My expression.

A tendency to look like an *"immature trainee"*

When top managers were informed of my former experiences both as a Training Manager in a big industrial company and as a former researcher in a famous French automobile enterprise[12], my observing conditions were quite favored. Indeed, when I had not been introduced as such, I seemed to appear as an *"immature trainee"* given my apparent youth, my position as a PhD student, my passive observing position and my informal look (these traits will be described more fully later on). And this role of immature trainee has often appeared as a constraint while doing observations and interviews among top managers – and only at that hierarchical level as will be shown later. I progressively found out that if top managers were perceiving me as a young and inexperienced researcher, they tended to either avoid spending time with me (which limited my ability to be informed), or to develop a strong wooden language throughout our discussions or interviews (regarding human resource management rules, their careers in the company or their appreciation of the company objectives and rules). Here is an example of such a phenomenon which happened on October 15th 1996:

> When starting field work, I decided to interview Human resources (HR) and Training functional managers in order to better grasp both companies HR rules and projects. Only two weeks after having started my observations at the Hermes Company Headquarters, I did interview the woman in charge of Hermes *"management and human resources processes, rules and tools"* which were used to *"manage careers, competencies, jobs, evaluations"* in the company. When I first asked her technical, as well as political, questions regarding the new *"competencies management"* systems, the ways they were implemented and/or perceived in the company, I was given general and quite tautological answers such as: *"competency management is linked to people's abilities"*, or, speaking of the links between the former and the new HR systems *"those two systems are not compatible because they do not answer to the same logic"*. Even when asked very specific questions such as *"why such an incompatibility?"* or *"how is the management of comptencies linked to the other HR tools?"*, I would not get any more specific answers. After thirty minutes of exchanges like these, I decided to share with the interviewee some of my own knowledge of such processes, refering to my former experience as a Training Manager in a company which had tried to implement similar projects. The interview then took a completely new direction. She started to be more technically specific and, interestingly, explain the human *"resistances"* that top management was confronted with within the company – *"resistances"* from Unions and from employees. Following the questions on training, salaries and career issues were then explored much more precisely and were politically contextualized. It was as if discovering that I was a (former) HR expert had led the interviewee to drop her wooden and somewhat closed language and answer my questions much more openly...

As shown with this example, when my interlocutors knew about my past experience, prior to our meeting and/or throughout our interactions, I found out that the level of discussions was quite changed. This experience had also highly motivated Hermes Training Manager to accept my offer in the first place. During the recruiting interview, I was asked specific questions on my former experiences (the

[12] I had led fieldwork in this French automobile company in 1994 and had published one article in a French academic Journal in 1996.

same questions were asked when meeting operational top managers). And this experience was regularly mentioned as the basis of those top managers' trust (and examples of other academic requests they rejected for the same reason). My knowledge of management norms of presentation had obvioulsly helped me to gain their trust. Whilst this was a blessing, engendering their trust based on my past roles was a mixed blessing as will be discussed now.

Being confused with a *"professional consultant"*

Indeed, given the way I used my personal background to enter the field and create meaningful interactions with top management, I had then to strongly fight against the potential confusion with a *"professional consultant"* which was also creating observing difficulties. As already mentioned, top managers repeatedly asked for my evaluation of their organizational efficiency, managerial abilities or workers' capacities. Once I had become aware of this potential confusion (and its potential effects on my observation strategy), I started to timidly answer that my role was to be as confidential as possible and that I was not able to give a serious opinion before the end of my research. Since it did not seem very convincing to them, I also developed a clearer strategy which might be stated as follows. I attempted to erase signs of business professionalism and build on academic resources to become labelled as a *"university expert"*[13]. My clear objective was then to avoid being characterised as a professional consultant, since it was creating strong limits to observing both top managers and workers who would have rightly felt judged and evaluated (and not simply observed) by me and may have used me in their 'power games' (which they did anyway, as will be discussed later, but without preventing me from remaining in the field).

In response to this situation I gradually developed different devices when interacting with top managers. I was often repeating the very limited cost of my research and explained it by the academic character of my work. I restated my neutrality through open behaviours: systematically refusing to comment on people or to give my opinion on organizational issues. I also provided my interlocutors with academic articles or references, always avoiding all requests to formalise a specific judgement or conclude on a given topic. Asking unusual questions to top managers while interviewing them was also a way to look more academic (as supported by comments on my *"weird questions"* at the end of some interviews). Last but not least, my clothes, ways of speaking and of presenting myself indicated, from their perspective, a lack of professionalism (too laid back, not efficient enough). This position was a perpetual construction, constantly threatened by new events, contacts or requests. And I, of course, never fulfilled this strategy and had to constantly redefine my social position in the expected way.

Consequences on the quality of results

As shown in these different examples, my initial position in the organization and my strategies to co-produce an open interaction context with top managers were linked both to my perceived personal resources – former experiences, formal status, age or professional expertise –, to my strategies for shaping perceptions one way or the other, and to the organizational context in which I was positioned. Each

[13] My expression.

ethnographer co- constructs their social position with top managers based on a constant analysis of what is going on in the ethnographic relationship. It allows the ethnographer to stay in the field as freely as possible and to be perceived in more distanced ways by workers who will then develop their own ways of managing the impression they make to the ethnographer.

More than reporting results, ensuring *"likelihood"*

As already stated, I was supposed to present oral and written reports. I will present my epistemological use of such a practice as it is rarely discussed in academic work (Michael Burawoy speaks of a *valedictory revisit* (Burawoy 2003: 674)). Once again, I was torn between two contradictory objectives. On the one hand, I was interested in receiving useful comments on my analysis through a constructive dialogue. On the other hand, I intended to present results without recommending any solutions, while recommendations were most valued by managers - and would have thus encouraged them to communicate further with me. Indeed, more than respecting a contractual agreement (and paying my observation debt), I intended to use oral reports in order to nourish my reflexive sociological work. Following François Dubet's epistemological reflections, my hypothesis here was to build *likelihood (vraisemblance*) with observed people in order to found a debate and then feed my scientific results (Dubet 1994).

The reasoning works as follows. According to the Weberian paradigm, actors have *good reasons* to do what they do. A sociological analysis formalises actors' points of view, ways of proceeding and practical choices in a very specific way, visualising interdependency relations between actors while they tend to view themselves as individual actors. In other words, through their analytical work, the researcher tries to situate individual actions in their collective reality, attempting to redefine a personal experience as an organizational phenomenon. Then, as Dubet clearly states *"sociological intepretation presents itself as a problem, between the internal sense of action and the one built up by the sociologist."* (p. 94). *Likelihood* is then supposed to solve this difficulty in that it forces the researcher to write up results which, at the same time, remain as close as possible to people's experience and present a collective analysis. People should then recognize their daily realities while acknowledging a new way to express and explain it.

But this does not mean that people agree on all aspects of the report. It implies more that even when disagreeing, their disagreement might not concern the report of their experience, but explanations developed by the researcher. And even when *likelihood* is ensured, the ways people interpret or discuss results indicate that there are many ways to experience such realities. Is this sociological analysis acceptable to them, in which terms and at which conditions? Such reactions may help support the analysis, but also bring new proofs, new perspectives. Is it unbearable, and if so why? Do actors disagree with each other and on which terms? Such a conflict or disagreement between observed people or with the sociologist, if discussed with actors, may lead either to the reformulation of results, or the development of new hypothesis. It is only through an open dialogue that researchers may use reporting back to enlighten their sociological analysis. If not used to convince others of one's analysis (membership validation), but to once more become aware of actors' ways of thinking, such an oral report may enrich the sociologist's analysis, supporting some points, transforming others, deepening some forgotten points. Once more, among

several other possibilities, I will discuss one specific and tricky example which happened in June 1998 and may help understand this perspective:

> The restitution of my other written reports mainly helped me to enrich my first organizational analysis. But one such report disappeared from the organization due to a conflict with operational top managers. Not only was this conflict never settled, but it also led me to develop a specific academic article! Conflict is indeed one of the most difficult situations to be experienced when one attempts to reach *likelihood*, *vraisemblance*, but it ended up being a very rich experience at least in terms of producing sociological results. Here is the situation. When providing those two observed operational top managers with my sociological report, they rejected it without radical transformation (the same report had been considered as very insightful by Hermes Training Manager). My main interlocutor, M. X, was very aggressive on the phone, accusing me of writing *"wrong conlusions"* and *"immature analysis".* Right after his phone call, I had a discussion with Hermes Training Manager who had already been informed and who told me to calm down (I was quite upset and not convinced at all to be in the right). She then called M. X and assured him that the report would be kept confidential, asking him to meet me to see how to transform it.
>
> A meeting was then organized with the two main operational managers of the department. Through our discussion, it appeared then that they rejected the way I explained middle managers' marginalisation from the organization – even if it was only part of the complete report. They seemed to agree with my main analysis – middle managers' exclusion and marginalisation from the organization (they often said *"it's true"*) – but wanted to explain it through *"historical"* and *"psychological"* explanations which would have legitimated their managerial choices. As explained by M. X: *"You have to put this analysis in its historical context. I have tried to work with those middle managers, to motivate them, to delegate tasks, to train them… But I soon understood they would say yes, but they would not do what was expected. (…) They wanted to change, but they could not, they were limited… (…) After two years, I bypassed them and worked with first-level employees. What you say is true, but I had no other solution. I had too many middle managers, but because of Union pressures, I could not discard them."*

While he thought middle managers were marginalised from the organization due to their psychological inability to evolve, I presented collective organizational mechanisms which reduced those people to their denigrated position and legitimated it. While we could agree on the diagnosis, we would not reach an acceptable agreement on explanations. Not only was the *likelihood* of my report ensured, but our heated discussions also enriched my analysis of this phenomenon. It progressively led me to conceptualise this phenomenon as the expression of a *social psychologisation process* which produced and legitimated this social group position within Hermes without workers being aware of it. While operational managers could protect their interests - the report was never read outside this department, I never discussed those results with other members of the organization -, I used this open conflict to nourish my academic work (Buscatto 2002)[14].

[14] I here discuss formal reports, but reports may also be informal, face-to-face, friendly or "accidental" as I have experienced throughout my ethnographic research in the French Jazz world (Buscatto 2007a, 2007b).

Observing workers: a locally co-produced position

When first entering my operational fields, specific organizational frameworks were already available to workers to help them perceive me. It soon became clear that my position as a non-participant observer - never experienced before by those observed workers - was widely associated with that of an outside employee, sent by top management to evaluate organizational processes: internal or outside consultants. This held even when my reasons to observe appeared to them as different from those of *outsiders*.

My observation work thus consisted in transforming my original position as an outsider close to top management to a more neutral, but never fully reached, position as a young neutral researcher guaranteeing confidentiality. This work was constant, difficult to maintain, never fully controlled given the hierarchical nature of organizational life. I will show how I progressively tried to redefine my position through interaction while being conscious of the limits of this attempt when analysing my *collected data* in order not to be a victim of what was said or done in front of me.

My original observer position: *"an outsider close to top management"*[15]

As an observer, I was first viewed as a burden, as a disturbing element and sometimes even as a risk. Being unknown, of course, always creates some embarrasment when first observing. But mistrust, even fear were highly increased by the way I entered into the field: I was sent by the Human Resources department and operational management. I was then mainly perceived as an investigator interested in unveiling personal and collective strategies, daily hidden practices, supposed management weaknesses, *etc*. Workers perceived me first as a top management employee and likened me to a consultant who might use her observations to recommend changes which were not necessarily wanted (fewer workers, organizational reforms, increased productivity). This was reinforced by the fact that observed workers had never met a researcher before. Here is a clear example of fears one may create when being introduced in the field by HR and/or operational top managers:

> I spent a full day observing Jeanine[16], a first-level employee. I also interviewed her for a full hour at the end of that same day. This specific observation was led at the beginning of my fieldwork in her department, on March 22nd 1997. Jeanine had seen me about once or twice before I observed her. In the days following her observation and interview, I felt Jeanine was unconfortable seeing me there. I took the first opportunity I found to informally discuss with her, understand what was going on, make sure she was not worried because of this experience... Here is what she then told me: *"When you asked me questions, I was quite open. We all try to be. I told myself that we had to help you out in your work and that anyway things will change here. But afterwards, I got worried. I wondered what you would do with all this, if it was not going to create problems. I am worried… even if I know things will change…"* In this case, fears have appeared afterwards and did not seem to have affected my observation conditions: Jeanine was indeed quite open during the observation day and the

[15] My expression.
[16] A fake name.

> interview, I did have access to several clandestine games and private information -. But those fears were experienced fully afterwards, which gave me the opportunity to assure her once more that I would preserve confidentiality. However, this example does show that my observing position might create difficulties which needed to be dealt with. It also reveals that, as we will now see, my strategies to overcome such fears, at least while observing and leading interviews, may have been quite successful with Jeanine.

Being perceived as a *"neutral young research trainee"*[17]

Even if I had not yet been aware of this original position, workers would have helped me discover it through their regular questions about my research (*"who asks for it?", "why?", "what for?", "what will be written?", "what will they do with it?"*) and their first behaviours towards me: getting silent when I first arrived, escaping observation when possible, being uptight on first days... I then developed different strategies in order to dissociate myself from this original position. After several attempts, I tried to build what I might call today a *"neutral young research trainee"* position, partly trying to be assimilated with young trainees or temporary workers, partly constructing a new framework with observed workers. My ideal (and never reached) goal was to be perceived as a young researcher still learning at school and based in a different professional world, the academic world. Power relations and hierarchical stakes were not supposed to influence my behaviour since I did not belong here, being thus able to ensure confidentiality and neutrality to observed people – which anyway I had planned to do for obvious ethical reasons.

In order to get closer to this social position, I adopted a fitting physical and verbal appearance: wearing jeans, tee shirts and confortable shoes; being underequipped (pens, paper and handpack at best); reinforcing the impression of relative youth by the lack of make-up, jewels or glasses (observed employees, mainly women, were over 40 years old, I was then about thirty and, according to them, looked about 25); expressing all my questions or requests as a *"personal favor"*; using a simple langage; accepting personal conversations on my private life and interests… I also presented myself as a trainee, completing a PhD at the University, not mentioning (without hiding it) my former experiences, since, as the following experience shows, this generally limited my observing conditions:

> Indeed, one of my first observations of a middle manager led beginning of November 1996 had been quite a failure apparently due to him knowing my former experience as a middle manager. Indeed, when I started the daily observation of Paul[18], not only did I explain my research topic in broad terms *("organizational learning conditions"*) and ensure him that confidentiality would be preserved, but I also informed him of my former experience as a middle manager. I then thought that this might create some easiness in our relationship, as experienced with top managers before. But instead of working on his own, as I had asked him to do and as most observed workers did afterwards, this middle manager spent the day explaining what he was doing, reasons he was doing it, goals he was trying to achieve, ways he analysed his work situation… He sometimes asked me for my opinion, suggesting my former operational experience might be

[17] My expression.
[18] A fake name.

> useful to him… And even if I repeatedly asked him not to explain anything while working, not to describe his actions, not to discuss his work until our formal interview, he never stopped doing it… After this experience, I decided never to mention this past experience again to middle managers, unless asked to do so, and I never had any middle manager explaining his work throughout the day again.

I then concluded that this position as a young trainee, even if incomplete, encouraged people to relax, to conduct their business as usual, and to open up, even to help me in my work. Most people were over 40 years old, and they were also sensitive to the problems of young people with regard to unemployment and getting a permanent job. Therefore, young trainees tend to inspire more pity than fear. The numerous questions I was asked about my future, and the remarks made on my difficult job *("is it not too difficult to observe all day?"*), partly support the idea that I was partly accepted as a young trainee. I was also easily called by my surname or *"tutoyé"* without even being asked if I agreed to it. Workers were easily avalaible to answer my questions, to help me and sometimes came to me to give me some work papers for what they called *"my file"*. They would accept an interview even if time was short or they were afraid to do it, telling me afterwards they wanted to *"help"* me. I also found that after a few hours or days, workers would speak about 'private' topics in front of me (familiy issues for example), would cheat on top management, would deal with their private (and forbidden) business in my presence (calling an administration office to settle private problems for example) with no embarrassment.

However, I could not be simply assimilated as a young trainee since some of my behaviours did not fit this role: taking notes on people's behaviours, printing official papers, observing and meeting with management, and writing reports. Questions were often asked about those activities. And, that is why I also progressively constructed, through interaction, a second framework with organizational members: as a *"neutral researcher guaranteeing confidentiality"*[19]. When beginning an observation, I would explain the sociological method in metaphorical words (*"you are numbers", "I am only interested in shared behaviours"*) and the way I could ensure confidentiality *("I will use only anonymous quotations", "only shared behaviours or discourses are quoted", "I will quote only common situations, so you ca not be recognized"*)[20]. I also rapidly announced my parallel research at Hermes and at Mercure to be sure it would not be known behind my back: a merging between both companies was announced during my research and it became a threatening issue, specially at Mercure. I also tried to adopt some systematic behaviours to embody my role. Whenever asked, I would show my notes, so that people would know it only consisted in noting very specific behaviours (who says what to who, what is done, which timing…). Most people would then tell me they felt sorry for me for doing such a dull work. It even became a joke in one of the observed departments, one employee announcing aloud how I was to write their actions down (in quite an efficient way!).

I also always refused to comment on people's work and to answer questions on other observed groups, departments or companies. If people insisted, I would show them that this was the only way to protect their own confidentiality. I would also never leave my notes unattended, which meant carrying them everywhere. Over time, I became part of their organizational life as a silent and acceptable observer[21].

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[19] My expression.

[20] For obvious ethical reasons, I did respect those promises while writing written reports, even if it sometimes limited my ability to support my claims.

[21] In a overt ethnographic survey led in 2001-2002 at La Poste, the French postal service public company, I also

Not being fooled by one's own acceptance

Even when partly reached, this neutral position is never completely shared with workers and one should not be fooled by a feeling of acceptance. I was always, at least partly, viewed as an outsider close to top management and several observations could not have been interpreted rightly without taking this reality into account. In other words, part of observed behaviours and discourses are constructed in answer to this perceived position and being aware of this possibility helps better analyse observations. What is hidden or overlooked indirectly indicates what is valued or forbidden by colleagues, middle or top managers from the studied world.

Use the ethnographer to pass messages on to top management

The researcher may be used to pass messages on to top management. This use, if systematic, may deserve to be analysed in sociological ways. This happened to me often with first level employees when observing and/or doing interviews at Mercure. Management was often criticized for not giving enough incentives, for giving too much work, and for being inconsistent, as in the following example recorded on April 14th 1997:

> After having spent a full day observing Gina[22] at work, I had a full hour interview with her to discuss work, relations with colleagues, training, promotion, *etc*. Gina had been working at Mercure for the last ten years as an insurance first-level employee. Half-way the interview, I asked Gina *"how do you perceive your work unit objectives?"* She answered quite strongly *"We have to satisfy the customer. That's what they keep telling us, every week. Work and work, fast and well, that's what they repeat endlessly. It's true. Every wednesday, they have a meeting, and every thursday, they ask us to work more."* I then asked her to explain what she meant, to which she then answered *"We already work a lot (…) The problem is that we have to work, but we should not ask anything. It is not very motivating. We are seventeen and out of seventeen people, only two people get a raise. Some people did not get a raise for more than ten years…"* After the interview, while discussing with her manager, I found out that Gina had recently received a promotion and a raise (I then checked the information with her and she confirmed it). But throughout the interview, Gina never mentioned it. She had mainly complained about work being too heavy and not being rewarded rightly at Mercure...

What may have appeared as a kind of manipulation (she did not mention her own promotion while complaining about this company poor promotion policy) was in fact the expression of an organizational rule. Most first level Mercure employees, evolving in an enriched Taylorian organization, were constantly producing and reinforcing strategies of resistance (Roy, 1954) in order to work less, get better paid and work in better conditions – daily observations helped me reveal them of course. Very active union representatives were also regularly observed distributing leaflets expressing such issues, discussing with employees, helping them formalize their complaints in their work units. I even observed a Union representative choosing to

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tried to co-construct a similar framework with observed workers. But since I had then become an academic public servant at La Sorbonne, the co-constructed framework I tried to implement was not focused on my youth any longer, but heavily oriented toward my academic supposed neutral position.

[22] A fake name.

discuss one employee's new situation with a middle manager in front of me, so that I would witness his helpful work in the organization. My role had been defined in relation to this context, and becoming aware of my role enabled me to produce one more proof on their strategies of resistance which I had become a part of.

Hide relational difficulties from the observer

It also appeared that it was difficult, or even impossible, to observe what employees and/or middle managers had defined as their personal organizational life or their hidden compromises - and which John Van Maanen calls *"collective secrets"* (1979). Relational difficulties were not easily discussed as such, enmity tended to be erased from discussions and conflicts concealed. Personal difficulties, even if collectively created, were mostly hidden. I will present here two examples of such hidden difficulties which I have unveiled through careful and specific observations, leading to the production of interesting sociological results.

> The first example happened at Hermes. As already discussed, I have progressively identified a *social psychologisation process* which was producing and legitimating Hermes middle managers' marginalisation within this organization. But this finding had been quite difficult to achieve empirically, mainly because of middle managers' tendency to hide their difficulties from me and/or to express their situation in positive ways in order to *save their face*. During my first interviews with these middle managers, they were mainly stating their supposed new management responsibilities in order to describe their new roles and tasks in their work units. They described themselves as people leading their direct reports actively, leading meetings openly, creating innovative projects… While first observed, they also tried to focus on tasks which would illustrate such skills – handling a budget, preparing a direct report's evaluation, dealing with difficult insurance files… However, they could not maintain such games long. I progressively observed that their direct reports were often nagging them, were misbehaving during meetings or were bypassing them to handle tricky files... I also progressively observed them trying to escape their new role and/or failing in implementing it – incapacity to prepare a budget, to lead a meeting or to train a newcomer. I then decided to focus on their situation. When observing first-level employeees, I collected as much data as possible on their difficulties. I also followed more middle managers than previously planned and decided to (softly) confront them with those observed facts during interviews. This last strategy often led them to express their difficulties openly while explaining them as the consequence of *"psychological"* deficiencies – their *"personality"* did not fit this new managing role... Most of them then used me more and more to complain (and pass on messages to top management), get advice to handle their future meetings or careers or to try to understand what was going on at Hermes...

Hermes middle managers' strategies told me much about their shame about being marginalised, the reasons they gave for their difficulties, their desire to get out of this difficult situation, their despair to ever change, and other workers' lack of compassion. Discovering individual actors' strategies to hide some facts helped me to identify a collective phenomenon, a *social psychologisation process.* This tendency to hide some personal or collective realities may even lead to a collective

strategy developed to hide some events from the observer, as experienced at Mercure:

> My arrival at Mercure corresponded with a conflict developing between employees and middle managers, but open and collective disputes never occured in front of me. From time to time, I would perceive a tense atmosphere when arriving in a department (red cheeks, tense bodies, frowning or sulking employees, heavy silence). I also observed strong remarks between some employees and their chief. After a few unsuccessful attempts to understand what was going on, asking employees to explain those observations, I have progressively found out that a fight was just developing over the work load issue. What I then found could be summarized as follows: for two years, one or two temporary workers had been hired *per* department in order to replace employees sent on intensive training. Now that the training plan was over, temporary workers had been dismissed. But for two years, an implicit work load share had been agreed on by workers, giving temporary workers less interesting tasks (what Hughes might have called "*dirty work"*) and asking them for higher productivity norms. Regular employees had now to share this dirty work and increase their productivity norms. They wanted to get something out of it, but did not seem to think it that legitimate. They did not want an outsider, partly sent by top management, to see them while exchanging bad words on this issue. Indeed, first-level employees never explained this conflict to me. People who finally informed me were middle managers and qualified employees who did then criticise those first-level employees who were using temporary workers to do their *"dirty work"*. When I then asked first-level workers if this information was true, they would agree to it and explain it their own way, as a resisting and thus legitimate strategy.

Not being able to observe such conflicts was directly caused by collective behaviours which told me many things about the organizational dynamics and the strong separation between first level workers, middle managers and nearly promoted workers. I was then able to discover it and get to its collective dynamics only because some workers did not consider this conflict as legitimate and were ready to expose the colleagues whom they thought to be wrong to the relative 'outsider'.

Trying to look good?

Another kind of *face work* people engage in might consist in trying to work at best and to hide difficulties and resistance strategies. I decided to discuss it last since it is often presented as observations' main difficulty, while it appeared to me as a key source of knowledge.

First, when a worker tries to do their best, it tells much about what the *"best work attitude"* is supposed to be in a specific organization. Just as when reading official rules and norms, first days of observation may indicate what the official behaviour is supposed to look like, some workers naturally succeeding in applying such norms, getting promoted and valued in the organization. Being identified as an outsider close to top management may then become an advantage as long as one is aware of it:

> For instance, at Mercure, I twice observed the same middle manager, Jean-Luc[23], with a six month time lag. During my second observation, on

[23] A fake name.

> November 27th 1997, he did what he had not done during my first observation: counselling his business insurance interlocutors. He was then applying a new norm officially instated and given much importance during my absence - as I found out when reading new objectives and tasks given to middle managers at Mercure. When I asked him about this difference in his job, he seemed surprised and pretended he had always done so. He also explained at length how important it was to do so… I still do not know whether he had chosen to behave that way to look good in front of me or if he was trying to be positively valued by his hierarchy and, when confronted with it, did not want to look too eager to accept new rules – Mercure and Hermes were merging, and this new rule was clearly imported from Hermes... But thanks to this observation, I did find out that this norm was perceived by some middle managers as a new official and important norm to be implemented at work.

Secondly, and more importantly, as discussed by Becker in *Sociological Work: Method and Substance,* workers tend not to be able to play a new role over time for two main reasons. On the one hand, they relate to people who mainly continue to ask them what they are used to being asked to because work has to be done. Workers have to get their job completed, and even if they try to adopt a perfect attitude in the first hours, they have to forget it when confronted with real problems to be settled. I was often asked to help in producing clandestine behaviours, resisting or creative practices, just because this is what the work situation required observed workers to do in order to complete a normal day and not get into trouble with their hierarchy and with their colleagues. On the other hand, one observed worker may be able to practice such a game, but setting up a collective strategy is most of the time too difficult to handle. I usually became part of clandestine games which were shown to me at length (reading names, limiting work, helping settling problems)… I had no choice to return to my first passive answers because of the negative reaction they tended to provoke. I was integrated by first level employees in their games.

The observer's position is locally co-produced with workers from different hierarchical levels. It widely evolves throughout the observing time, depending on the ethnographer's personal resources and behavioural strategies, organizational context, individuals' positions within and outside the observed organization. Each daily observation is a new day in this fragile and fluid construction. Only a systematic and daily analysis, focusing on a reflexive analysis of the observer's social positions, enables the ethnographer either to work on transforming, reproducing or eliminating them, or to use collected data as sensible signs of more general rules (Glaser and Strauss, 1968). I tried to shift from my original position as an outsider close to top management to a young research trainee position through different strategies. But hopefully people resist in their own ways and their resisting strategies told me a lot about organizational dynamics.

Discussion and Conclusion

By observing people at work, one becomes part of organizational realities and participates in workers' interdependency relations since:

> The outstanding peculiarity of this method is that the observer, in greater or lesser degree, is caught up in the very web of social interaction, which he observes, analyzes, reports. (Hughes 1971: 505)

In order to both build acceptable positions with observed people, and produce valid sociological conclusions, the ethnographer constantly works on co-producing adapted social positions in the organization and on becoming aware of negotiated roles over time. This is *reflexive* work. Even if based on the use of several positive techniques, it mainly requires constant questioning of the meaning of observations, depending on the social context in which they emerge and are developed.

This is tricky work, organizations being closed, hierarchical and sometimes conflictual places. The overt ethnographer first develops convincing strategies to get accepted by top managers. Once accepted, they must constantly negotiate their social position in order to be left relatively free as an ethnographer. They must then co-produce frameworks with observed workers, in order to distance themselves from initial tricky positions - such as an observer close to top management. They also work on interpreting observed behaviours produced by observed people to hide, transform or distort discourses and practices.

Analysing empirical data becomes, then, a central source of knowlege. Discussions on methodological issues are more than a simple exchange of technical receipes, they enable researchers to enrich the quality of their sociological results, and thus work on improving the quality of qualitative methods. But reflexivity is not specific to the use of qualitative methods in ethnographic sociology. It has been present in ethnology or history for decades (Bloch 1949)[24]. More surprisingly, it has also been observed in some "hard sciences" such as particle physics, where experiments as such are defined as objects of study in order to better interpret results and to develop further experiments (Knorr-Cetina 1992). One may thus hope that reflexivity expands to all sociological research, quantitative and qualitative, since all research processes produce *artefacts* (Silverman 2007) and *social biases* (Gaxie 1990; Le Noé 2002; Peneff 1988). Those may be reduced, but never quite avoided through the simple application of positive techniques. Why not 'use' reflexivity as a way to improve the quality of quantitative, as well as qualitative, methods then?

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[24] *«Vieille sous la forme embryonnaire du récit, longtemps encombrée de fictions, plus longtemps encore attachée aux événements les plus immédiatement saisissables, elle reste, comme entreprise raisonnée d'analyse, toute jeune. »* (Bloch Marc 1997/1949: 43).

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Narratives in Illness: A Methodological Note

Abstract

As a result of the general growth in the interest in narratives different conception of what a story is and how to analyze has emerged. One especially interesting and methodological relevant difference is between the conception of narratives as textual objects and narratives as part of a storytelling event. The paper discusses the theoretical differences between these two analytical approaches to narratives. An example from my own research on Alzheimer's patients telling stories illustrate the possibilities of using a performative and micro ethnographic approach to the study of storytelling in order to understand the functions of narratives – especially in relation to identity work. If stories not only are thought of as representations of events it becomes possible to view stories and story telling as social action: social states are both established, negotiated and changed through stories. This is especially important in the field of health and illness where diseases almost always are embedded in conversations and the telling of why and how symptoms were discovered or traumas received. For many patients and persons with especially communicative disabilities story telling is a challenge, but also an opportunity to actually master, maintain and often transform their identities.

Keywords

Narrative; Methodology, Identity; Performance; Ethnography; Video analysis

The number of books and articles with word "narrative" in its title or abstract has increased explosively during the last two decades. As a consequence a narrative research field has been established with the story, in all different guises and manifestations, as the focus (Kreiswirth 1994, 2000).

The interest in narrative has historical roots going back to researchers like Sigmund Freud, William Thomas and Florian Znaniecki, and Claude Lévi-Strauss who all early on observed the centrality of the narrative form. To Freud the case story constituted a centre in his writings. Thomas and Znaniecki systematically collected stories from Polish immigrants in the US in the twentieth century. To Lévi-Strauss the myth constituted the motor of culture, determining ways of thinking and understanding the world.

One explanation for this interest in stories may be linked with a changed conception of language and language use in the social sciences during the 1960's

and 70's. To Freud and his contemporaries, language was conceived as being fully transparent making social reality directly accessible through words and stories. Through the influence of modern philosophers like Ludwig Wittgenstein it became evident that social reality is created through the use of language. The linguistic turn" in the social sciences could be noted already in the early book by Berger and Luckmann (1966), and further conversation analytic works (Atkinson and Heritage 1984) and in the more recent work interested in narratives (Bruner 1990; Andrews et al. 2004).

Another reason for the interest in narratives is the character of much of the empirical material used in social scientific research consisting of text, talk and story telling – whether collected in surveys, interviews or in observations. The fact that storytelling is found in all of these practices has also generated an interest in looking in detail at the ways in which the story and the telling of stories can have consequences for the way in which empirical materials are analyzed and theorized (see for instance Mishler's (1986) discussion about research interviews.)

As a result of the general growth in the interest in narratives there has also been a rise in interest in different conceptions of what counts as a story and how to analyze stories. This has led to different methodological conceptions and practices (for an overview see Riessman 2008). One especially interesting and methodologically relevant difference exists between the conception of narratives as textual objects and narratives as part of a storytelling event. In the former case the methodological focus is on the internal discursive structure of the narrative/object, while in the latter case it is the social organization and performance of the narrative that forms the analytic focus.

In the following I will start with a discussion of the theoretical differences between these two analytical approaches to narratives. I will then use an example from my own research in order to further discuss the methodological differences.

Narrative as Text and Performance

Many social sciences researchers have a tendency to favour a textual conception of narrative over a performative and situated one (Hydén and Brockmeier 2008). In the social sciences this often means that narratives produced in conversations are treated analytically as if they are an instance of a written, text based narrative. This means that the focus is on the discursive organization of the narrative in terms of coherence, plot and so on. It also implies that the meaning of the narrative is thought to be found inside the narrative. And, finally, that the narrative primarily is about something, it represents for instance events in the past.

Especially in interview studies the idea that narratives represent events that took place at some previous time, is quite salient. In this case, the narrative is not of interest as such, but only as a vehicle, a form for representing the past. Similarly, the narrative can be treated as a constructed expression or reflection of the interview person's emotions, identities, "meanings" or ideas.

One problem with the textual approach to narrative is that narratives told in interviews or in some other kind of interaction are analyzed in relation to norms for textual production. That is, they are analyzed as if they are instances of, for example, written autobiographical texts. The norms for written stories are in many ways different from those of spoken language (Linell 2005). The textual narrative allows a more elaborate and formal style and use of imagery, and a refined chronology of events. These norms and forms are rarely used in the telling of oral, conversational

narratives, where those aspects having to do with the necessity and importance of engaging the audience through the whole story telling event and delivering a point are much more important (Bauman 1987).

Textual narratives are generally composed in order to be read and consumed by anonymous readers in quite different settings and points in time, something that to a certain degree makes written stories de-contextualized. Whereas oral narratives are heavily dependent on the specific social situation, audience, physical setting, gestures, prosody, etcetera, that is, aspects related to the performance of the narrative. In producing a text, at least some of these contextual resources have to be transformed into textual elements.

These textual norms are also present in the transformation of the empirical material, especially in the transcription of talk into text. If narratives told in interviews are transcribed without notation for hesitations, pauses, repair, listener support, para-linguistic features, non-verbal aspects and so on, all interactional features of the performance of the narrative are left out. Consequentially everything "outside" the narrative text is left out of the analysis, which makes it difficult to analyze the telling of stories as social action.

In many cases a focus on the narrative text can of course be a feasible strategy – especially if researchers primarily use narratives as a way of accessing representations of events in the past. But a focus on the narrative as text becomes problematic if researchers instead are interested in the functions of the narrative and the storytelling, that is what is accomplished through the telling of a story. In studies using, for instance, an ethnographic or micro-ethnographic approach it is often found that story telling occurs at certain moments in the social interaction, joining tellers and listeners in concerted action, sharing experiences or memories. Story telling can then be seen as part of a family's or a group's social life, establishing, re-establishing and negotiating, relations, membership and connections (Langellier and Peterson 2004).

The focus on story telling in various social contexts has led researchers to focus on both the performance of the story and its performative aspects. That is, both the way the story is told and performed in interaction jointly with the listeners, and what is done or accomplished through the telling of the story (Bauman and Briggs 1990, Langellier 2001; Peterson and Langellier 2006; Hydén and Brockmeier 2008).

To many researchers a performative approach also makes it possible to regard language not just as a vehicle used to communicate a story. Instead of a focus "on processes within the individual or on structure within the talk of a single speaker or narrator" it instead becomes important to regard "how language is organized as a public sign system" (Goodwin 2004: 154). As a consequence the focus is much more on how stories are told in interaction using several different communicative modalities (language, para-language, gestures, eye movements, bodily positions, material and social artifacts in the immediate context, etc). This makes it important to look at the ways tellers and listeners use all available communicative resources in the narrative situation (Goodwin 2004).

Methodological aspects

Working with narratives from a performance perspective raises a different set of methodological considerations as compared with a focus on the narrative's textual organization.

First of all, it is necessary to collect data that allows for an analysis not only of the narrative as such but also the wider social and cultural context of which it is a part. Generally this means using an ethnographic or micro-ethnographic approach (Streeck and Mehus 2000).

Second it is preferable to use video recording as material if possible. The reason is that video recording allows an analysis not only of the spoken word, but also body movements, gestures, use of gaze and so on (Goodwin 2004). That is, using video recordings allows for a multi-modal analysis.

Third, a descriptive and analytical focus on a wide variety of communicative resources makes it necessary to use transcription conventions that allows for this type of analysis (cf. Goodwin 1981).

Although a performative approach to narrative analysis is much more complex and in many ways more labour intensive than just analyzing the narrative as a text, it at the same time makes it possible to discover and see new aspects of human social interaction.

In the following example from my own research I illustrate the possibilities of using a performative and micro-ethnographic approach to the study of narratives in order to understand the functions of narratives – especially in relation to identity work.

Alzheimer's Disease as a Methodological Case

The example concerns patients with Alzheimer's disease (AD). The study of Alzheimer's disease has traditionally been dominated not only by the medical discourse, but also by a – at times at least – quite pessimistic and dark view on persons suffering from AD, even among sociologists (see Ballenger, 2006, for a historical review). Much of the research on Alzheimer's patients has been methodologically dominated by experimental and clinical approaches, even in studying the narrative competencies of persons with AD. Generally few researchers have designed studies in order to be able to analyze narratives told by persons with AD in everyday settings and around self-selected topics rather than ones suggested by a researcher.

Persons suffering from brain trauma or dementias like AD all have an issue with telling and using narratives due to their cognitive and linguistic impairments. To these persons, telling a story challenges their use of their actual cognitive and linguistic abilities together with all other communicative resources they can muster. Stories told by persons with AD can be severely fragmented, parts can be repeated over and over again, certain events can be left out, and other events that never occurred or involved the teller can be included etcetera.

Accepting that telling stories is one of the most important ways of establishing and negotiating identity, having problems telling a story can of course tend to challenge a person's identity. An important question then is whether persons with AD actually can tell stories about themselves as a way of establishing and negotiating their identity in the social interaction (Hydén 2008).

Those researchers interested in how persons with AD tell stories have primarily been interested in the discursive organization of autobiographical narratives, especially the temporal and referential aspects of narratives. That is, their research has been guided by the idea of narrative as a text and hence the patients' ability to reproduce the narrative text. As a consequence the focus has been mainly on the ability of the person with AD to remember or retrieve and present memories of certain

events correctly, and to elaborate and connect them into a story. This approach tends to preclude other ways for persons with AD to use autobiographical narratives in order to sustain their identity.

One way to answer the question about the relation between storytelling and identity in AD is to find out if and how persons with AD use storytelling as a way of negotiating identity; and whether they use other communicative resources due to their linguistic and cognitive problems and limitations.

Methodologically this question challenges the established methods used in research on persons with AD and especially the research on the role of narratives. It becomes important to use methods and material that allows for a different type of analysis of narratives; namely an analysis that views stories as situated, part of an ongoing social interaction, and as a multi-modal activity.

In order to avoid the limitations in previous research an ethnographic study based on collecting naturalistic data was designed and conducted. During a period of five months video recording was took place at an elder center in Sweden serving eight residents, seven of whom were diagnosed with some form of dementia, mostly of the Alzheimer type (for further details see Örulv and Hydén 2006; Hydén and Örulv 2009).

By serendipity we were able to identify one story, “The driver’s license story”, that was told several times by the same resident, Martha. She is about 80 years old and was diagnosed with AD about five years prior to her participation in the study. The story was told on different occasions, in various contexts and with shifting audiences, both in group activities lead by staff and in spontaneous conversations between residents.

“The driver’s license story” is a story about how Martha as a young person decided to learn to drive and to get a driver’s license, and then to buy her own car. Both her husband and father questioned her ability, both to learn to drive and to save up for a car by herself, but they were both proved wrong. In one sense itis a story that portrays Martha as not only challenging the values of her generation about what women could and ought to do, but also overcoming those values, going her own way, and making a statement about herself. The story is organized around a set of reportable events and the actual story is then adapted to the different contexts and audiences and, therefore, told in different ways.

Itis a typical autobiographical story of the kind most of us tell as a way of presenting, establishing, negotiating or defending our identity in social interaction with other persons (Georgakopolou 2007; Linde 1993). Telling a story about what happened many years ago, is a way of making a connection between the “I” of then and the “I” of the present social situation. Through the storytelling the “I” of the past can cast a shadow over the present “I” and thus allow the listeners to see the teller in a new light, as a person having certain traits of the past “I” not noticed before (Bülow and Hydén 2003).

Having problems with telling stories due to Alzheimer’s disease potentially jeopardizes Martha’s identity and her identity-work. What is interesting with Martha’s telling of the driver’s license story is how she and her listeners deal with her difficulties, using ordinary linguistic structures and devices. As she has problems at times finding words and above all with the higher order linguistic organization of narratives, she creatively makes use of other communicative resources such as gestures, eye movements, touch, and so on.

The Story and its Telling

The driver's license story is organized around several sub-themes like deciding to get a driver's license, learning to drive, buying a car and so on. These sub-themes form a part of a temporally progressing story with some events happening first, and then giving place for succeeding events. This progression constitutes a higher order temporal organization of the story.

When Martha tells her story this higher order temporal organization of the narrative is problematic and even missing. That is, the temporal relation between the sub-themes is broken up and the sub-themes are told without any internal temporal organization. Further, some sub-themes are told over and over without Martha noting it or being concerned about this repetition. Martha's rendering of the sub-themes of the story in contrast, are generally well organized, and told in a similar way and generally end with an evaluation of the events.

In all these instances, the interactional organization has a similar structure: the audience is drawn in together with Martha, the teller, in appreciating the point of the story, and those parts of the story that are related to its evaluation. Evaluations are very important in storytelling, as itis one of the most prominent means a teller can use in order to convey to the listeners why the story is being told. It is also a way to position the teller in relation to the events in the story. This can be done either through the teller commenting on what happened or by the teller having some of the characters in the story deliver a comment. In the latter case the teller often quotes some person allegedly commenting on the events that took place.

In the example below Martha tells her story to her co-patient Catherine while no staff member is present. Just before the start of the example Martha has been relating to Catherine that her husband did not believe she could save money to buy a VW car. This part of the story, organized around her husband's disbelief, ends with the evaluation we see in the following example. In the transcription non-verbal aspects are added, as they are quite important in understanding what is transpiring.

Example 1

(1) **Martha:** ((*seeking eye contact*)) "oh ss <u>sure</u> I can" I said
(2) ["one can"]
(3) **Catherine:** [(xx xx)]
(4) **Martha:** "one can do <u>whatever</u> one <u>wants</u> to" [I said]
((*turning the upper part of her body towards Catherine, leaning against her and maintaining eye contact during the whole utterance. At the same time she pats Catherine on the arm with a slow and dramatic gesture, timing the bodily contact so as to further underline the word "whatever"*))
(5) **Catherine:** [yes]
(6) that's true ((*nodding*))
(7) **Martha:** yes
(8) and then one does not give <u>up</u> until one <u>is</u> there
((*marking the beginning of the line with two downward strokes with her fist and the stressed word in the end of it with a short nod accompanied by eye contact*))
(9) **Catherine:** =no
(10) **Martha:** and one <u>is about to</u> do it
((*raising her loosely clenched hands so as to underline the stressed part maintaining eye contact during the whole utterance*))
(11) **Catherine:** =yes

In the example Martha delivers her evaluation of what happened when her husband questioned her ability to save money and buy a car. Martha does this by quoting herself at the time of the event, embedding her evaluation in the story (lines 1-2, 4): "'one can do whatever one wants to' I said".

At the same time she actively engages her listener, Catherine, in the evaluation by seeking and maintaining eye contact, by turning the upper part of her body and leaning towards her, and by patting her arm while stressing one of the words (after line 4). Catherine responds to Martha's words and bodily movements by a supportive "yes that true", showing her agreement with Martha's evaluation.

Martha also actively dramatizes what happens in the story by using reported speech, that is, she quotes what someone else said. In line 1 for instance she quotes her past self saying "oh sure I can" at the time of the events in the story. By quoting herself Martha not only reports what happened, but actually enacts the utterance as a way for creating a feeling of presence and drama.

Martha further underscores this evaluation by rewording the utterance, underlining some of the words as a narrator in the speech situation and using gestures to give further strength to what she is saying (lines 8, 10). Catherine supports Martha's telling by affirming, thereby showing her support of the evaluative conclusion (lines 5-6, 9, 11).

It is apparent that this part of the story, the story evaluation, is not only linguistically and cognitively well organized. It is also enacted and also embodied in the gestures and in the qualities of Martha's voice. Some words are stressed para-linguistically and/or enhanced with gestures, and in this part of the telling the eye contact stands out as more intense and prolonged in comparison to the non-evaluative parts of the telling that surround this sequence.

The evaluative section of the story thus basically tells something about Martha as a person, both in the past and in the present. The Martha of the past, the young person challenging her husband and family, casts her reflection on the present person, the teller, an elderly resident in a care unit. By telling the story and winning support from her listeners, Martha is able to make claims about her identity and present herself not only as a woman who used to be brave and daring, but also as (still) being the very same person – hence a person with the same moral qualities.

The identity work here is inherent in the performance of the story and in the storytelling event as organized by the teller and the listeners together – and in this interplay where points are jointly established and acknowledged, indeed mutually performed. This means that both the teller and the listeners use embodiment in their joint performance of the story. Maybe this is an aspect of the storytelling activity that becomes especially salient when the person has severe linguistic and cognitive problems, as may be the case in AD. Using the body and other non-verbal communicative resources is a way of dealing with the loss of verbal fluency and the ability to create complex narrative temporal structures.

Conclusions

In conclusion I would like to suggest four general theoretical and methodological points.

First, in methodological terms the design, type of material used and the way it is analyzed is closely connected to wider theoretical issues; in this case the conception of what a narrative is. Traditionally narratives are often thought of as texts and verbal representations of events – an idea going back to the literary studies of narratives, especially narratology (see for instance Rimmon Kenan 2002). This may be a

feasible idea in some contexts, but it certainly introduces limitations in terms of the type of data one might collect and analyze.

I have tried to argue for viewing narratives as part of social action and interaction. Telling stories is *doing* something, and almost always, doing something together with someone else (the audience or listener or recipient to the story). In order to capture this wider idea of what a narrative is, it becomes important to use data gathering methods and ways of analyzing data that allows for a focus on *interaction and action*. This implies that studies of narration – both among persons with AD and all others – should strive to include not only both verbal and non-verbal aspects of talk in interaction, but also contextual aspects like the organization of the speech event and even institutional frames.

Second, I have tried to show that non-linguistic elements play a prominent part in story telling. Stories can be enacted, that is, *performed* which generally means that communicative resources like gestures, bodily contact, paralinguistic means, laughter, and not least the coordination of all these aspects in the speech event, are used. In other words, telling stories is a *multimodal event* – something that needs to be reflected in the methodological and analytic strategies used by social scientists in working with narrative analysis.

Third, this indicates that identity in relation to persons with AD, but certainly also more generally, may not primarily be a linguistic construct but very much *embodied*. That is, identity does not reside *outside* bodily movements and appearances, verbal utterances and stories, but *in* and as a *part* of all these actions. Identity can apparently be performed in many ways, of which the telling of stories is but one way and maybe not even the most important one.

Finally, I would like to suggest that the telling of narratives also has a *performative* aspect. Telling an autobiographical story is a way of changing – or at least an attempt to change – the identity of the teller. This is done by positioning the teller as a person who shares certain traits – moral or otherwise – with the character in the story. In this way the listeners have the possibility to relate not only to the teller but also the teller as a character in the story. In other words, through the story telling event the teller accomplishes a transformation of self: the old self merges with the teller-self.

If stories not only are thought of as representations of events it becomes possible to view stories and story telling as social action: social states are both established, negotiated and changed through stories. This is especially important in the field of health and illness where diseases almost always are embedded in conversations and the telling of why and how symptoms were discovered or traumas experienced. For many patients and persons with especially communicative disabilities story telling is a challenge, but also an opportunity to actually master, maintain and often transform their identities.

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Vision and Performance. The Sociolinguistic Analysis of Genres and Its Application to Focussed Ethnographic Data

Abstract

The use of audiovisual recording devices is changing the practice of qualitative research. Extensive corpus of data can be generated in (short-term) focussed fieldwork. Nevertheless, methods to analyse video data are still in an experimental stage. This article explores the benefits and limitations of applying sociolinguistic genre analysis to audio-visual data. This is illustrated with a case study, based on the videotaped »deep-trance vision« of a New Religious Movement's spiritual leader, which is one the most famous contemporary religious visionaries in Germany. The analysis aims to reconstruct the construction of this religious experience of transcendence from the perspective of its followers. We will examine three different levels of communication (a) the inner context, exploring the textual, gestural, mimical and prosodic aspects, (b) the intermediate level where the focus lies on the setting and decorum, and finally (c) the outer context, focussing on the social embedding of this form of »transcendent« communication and its filmic presentation. The article closes with a reflection on the need to combine hermeneutic analysis of case studies based on textual analysis with ethnographic field data and observation to contextualise its interpretation.

Keywords

Video-data; Genre-analysis; Ethnography; Religious experiences; New religious movements

Technical recording devices considerably change the present ways of conducting qualitative research. In particular, the availability of video cameras has exerted a deep impact on established research practices. This can impact on ethnographic work in at least two ways: On the one hand, the very *object* of research changes and on the other, it requires different – if not completely new – *methods* of analysis. Aspects of the field that passed unnoticed when using conventional forms of data generation are increasingly being rendered visible due to the use of audiovisual recordings and these ›recorded‹ aspects can be systematically scrutinised. Video-analysis enables the microscopic examination of minimal details that are unavailable with the use of reconstructive methods like fieldnotes or

interviews. Video recordings as sociological data show some particular characteristics. They are natural data insofar as they are obtained through conservation *by registration*.[25] Their relevance for ethnography is not only due to the major richness of sensual aspects that they contain (like images, sound, movement, etc.) when compared to reconstructive forms like fieldnotes, interviews or diaries. In addition, video data is apparently less influenced by the researcher's interpretations as, for instance, field notes or observational protocols. A further advantage of video-data lies in the richness of detail accessible for subsequent analysis, preferably realized collectively by a group of researchers. When compared to purely textual representations, video-recordings include another advantage. That is, due to their inherent sequentiality, they permit the preservation of the specific *chronicity*, the sequential unfolding of the recorded social action or chain of actions. Therefore, video data preserves the originality of the situation better than any form of (decontextualised) textual description. In addition, through resources of technical manipulation like slow motion, freezing a frame or rewinding, video can be accessed in *achronicity*.

Technological advance generates further consequences for the work of ethnography. Historically scholars have placed much emphasis on assuring the quality of data *collection* by insisting, for example, on the importance of first-hand experience in the field. However, it is evident that the same intensity of effort has not always been invested in assuring the quality and transparency of data *analysis*.

Nevertheless, video-data are not simple depictions of life-world occurrences, but mediated *representations* – that is reality is still transformed into data. This transformation comprehends, for example, reducing three-dimensional space into two-dimensional flat plane, eliminating perspectives and all non-acoustic and non-visual sensual qualities of the situation. In short, video combines mimetic *and* constructive elements. Although it easily preserves important aspects of a past situation and the interaction that have taken place, it would be naïve to view recorded interaction as a simple document of the situation. Video recordings as data entail some further problems for research. For example, it is easy to generate a large amount of data. But, managing a large data corpus exposes the researcher to a high degree of complexity as expressed some time ago by Südmersen (1983) when she referred to audio-recorded interview-data: The bewilderment facing the magnitude of data and the sometimes helpless search for methods to analyze them. In other words, new techniques of data collection are not a simple benefit, but confront us with a series of unresolved methodological problems. General approaches to analyse visual data (Englisch 1991; Hahn 1991; Rose 2000) are of limited use for video-analysis. And, the analysis of video-data requires more than »visual empathy« combined with a mainly descriptive »structured microanalysis« as Denzin's (2000) »principles of a critical visual analysis« suggest referring to documentary films.

The development of adequate methods for analyzing video data is a serious current challenge. Video data are gaining increasing relevance especially in qualitative research (for an overview, cf. the contributions in Knoblauch et al. 2006b). At the same time, theoretical reasoning on visuality and visual culture in general in the humanities and social sciences is flourishing.[26] In Cultural and Social Anthropology, visual data were extensively used at least since the midst of the 19th century, leading to the particular approach of *Visual Anthropology* as media

[25] For the distinction between ›conservation by registration‹ vs. ›reconstructive conservation‹ cf. Bergmann (1985).

[26] There are already some approaches for the analysis of visual data in general (Banks and Morphy 1997; Davies 1999; Emmison and Smith 2000; Heßler 2005; Pink 2001). Hence, video is playing a subordinated role, (Pink 2007) or is completely absent (Rose 2007) in visual ethnography.

supported field works (Collier 1967; Mead 1975). Although in academic Sociology one finds early uses of visual data already between 1903 and 1915 (cf. Soeffner 2006), a proper *Visual Sociology* was not established until the 1970s (de Miguel and Pinto 2002; Schändlinger 1998). Despite thorough efforts to extend the field of Visual Sociology, its influence was resisted since the 1980s by the popular project of *Cultural Studies*. Originating in Anglo-Saxon countries, Cultural Studies aspire to establish the constitution of a completely »new cultural science of images« (Holert 2000) with new »post-disciplinary« approaches labelled as *Visual Culture* (Bryson et al. 1991; Evans and Hall 1999; Mirzoeff 1999; Walker and Chaplin 1997) and *Visual Studies* (Schulz 2005). These approaches combine substantial contributions from critical social theory, media criticisms and discourse analysis, and apply them on visual instead of textual data. However, studies in Visual Culture are largely preoccupied with epistemological problems (for instance, if – or to what extent – images may betray the observer), and few researchers have dedicated themselves to the question of how video data may be conscientiously and productively used in qualitative research (cf. Goodwin 1994; Goodwin 2000; Heath 1986; Heath 1997a; Heath 1997b; Heath and Hindmarsh 2002; Jordan and Henderson 1995; Lomax and Casey 1998)

Video is used today in a wide range of research fields, as in doctor-patient interaction (Heath 1986), in visual ethnography of work and technology in the prominent approach known as *Workplace Studies* (Heath et al. 2004; Knoblauch 2000; Luff et al. 2000), studying work and interaction in high-tech working contexts like airport towers (Goodwin and Goodwin 1996), underground control rooms (Heath and Luff 1996), Information System Design (Luff, Hindmarsh, and Heath 2000) or tele-cooperation (Meier 1998). The application of video-analysis extends to research in fields like the study of contemporary religion (Bergmann et al. 1993), medical sociology (Schubert 2006), research of school interaction (Aufschnaiter and Welzel 2001), museums and galleries (Heath and vom Lehn 2004; vom Lehn 2006), social studies of science, technology and innovation (Rammert and Schubert 2006) or applied qualitative market research (Schmid 2006), to name but a few examples of the rapidly increasing research areas in which video is significantly used as a new form of data production and analysis.

In what follows, I will discuss the analysis of audio-visual data as a specific problem of contemporary ethnographic research, demonstrating the benefits and limitations of applying sociological genre analysis to video-data. Its potential will be demonstrated by using an example from a study on religious visions. The next section starts with some methodological considerations. Subsequently, I proceed to the example and close with some methodological reflections.

Genre analysis of visual data

The method of sociological genre analysis was originally developed for the study of *oral* communication (Luckmann 1985; Luckmann 1986). Communicative genres are patterns of speaking that represent predefined solutions to communicative problems which are historically and culturally specific. Their function is to deal with and to communicate intersubjective experiences of the life-world (Luckmann 1988). Genre analysis does not simply aim to describe different socially structured patterns of communication. Rather, it assumes that the communicative problems, for which predefined communicative forms exist, are of crucial importance in a certain culture. Communicative genres constitute the hard institutional core of

social life; they serve as instruments that mediate between the social structure and the individual stock of knowledge and as instruments of the communicative construction of reality (ibid: 716ff. For genre analysis cf. also Knoblauch 1996; Luckmann and Knoblauch 2004).

Genre analysis has proved to be a useful method for analysing oral face-to-face communication. But it has also been employed for technical mediated forms of communication.[27] The special methodological approach of genre analysis consists in its threefold level of analysis (Günthner and Knoblauch 1995). This seeks to understand the *internal* structural elements, the intermediate level of *interactive realization* and the *outer context* as the embedding of certain communicative forms in the wider social structure. It is this broad approach that makes genre analysis especially apt for an application to naturalistic video data (that is, video recording of naturally occurring social interaction, in the sense of Goffman). In this article, I will apply genre analysis on a video data fragment taken from a study in sociology of religion.[28]

Contemporary Visions: The case of "Uriella"

Data are taken from a focussed ethnography[29] of contemporary visionary experiences we conducted at the University of Constance.[30] The research consisted in collecting, interpreting and building a typology of contemporary premonitions of future events that are rooted in extraordinary experiences, usually called ›visions of the future‹ (Knoblauch and Schnettler 2001; Schnettler 1999; Schnettler 2004). Visions are experiences of ›greater transcendence‹[31], experienced in the inner realm of subjective consciousness. In order to acquire social relevance, these private experiences have to be communicated to others. The specific problem of visionary communication is located in the origin of the prophetic message, presented as something *animated by* the visionary *without* being him or herself the author of it. It is this ambivalent tension that constitutes the specific problem of presentation for visionary communication. In a more specific sense, visions are extraordinary experiences accompanied by extra-sensory perceptions (optical, acoustic, etc.) that are frequently interpreted within a religious frame of reference (Mohr 2000). In this sense, visions are conceived as revelations of transcendent knowledge that are being disclosed to others.

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27 See for example Keppler's (1985), genre analysis of political news magazines in television, Ayaß's (1997) study of TV sermons, Knoblauch's (1999) study of radio-phone-ins or the analysis of advertisements in television by Knoblauch & Raab (2001).

28 Video-analysis has already been used in sociology of religion, see Bergmann, Luckmann & Soeffner (1993).

29 See Knoblauch (2005) for the methodological principles of ›focussed‹ ethnography, its practice of colleting data and its differences to more conventional forms of ethnography.

30 The material is taken from the ⌜esearch project »Prophetic Visions at the turn of the Millennium«, granted by the IGPP, Freiburg (FP 68 15 10). I am especially indebted to Hubert Knoblauch, Regine Herbrik and Stefan Hohenadel, who considerably contributed to the analysis. Christian Heath and his team at Kings College London, the members of the ethnography circle Constance-St. Gall, Hans-Georg Soeffner, Jürgen Raab and other colleagues in Constance, also Matthias Kaup, Pavlina Rychterova, Bernhard Haupert and Klaus Kraimer had been exposed, some repeatedly, with video/recording and in turn gave me a lot of very helpful ideas. I thank especially Beatrice Szczepek for her support in transcribing the prosodic peculiarities of the material.

31 I follow the notion of Schutz and Luckmann that the capacity to experience transcendence is rooted in the specific intentional structure of consciousness that is constitutive for the human condition. For the distinction between ›minor‹, ›medium‹ and ›greater‹ transcendency see Schütz & Luckmann (1973) and Luckmann (1967). See Knoblauch (1998) for the distinction between the anthropological from the phenomenological transcendence.

Vision is presently a frequently used term, although it is prominent in quite a different context. ›Visions‹ are flourishing today in economics and politics. They abound in management rhetoric, business communication and political propaganda. However, these ›secular‹ visions have little in common with divine inspiration, prophecies of doom, calls for redemption or other kinds of moralizing appeals. Instead, these ›visions‹ represent quite mundane orientations and aims in contexts of pragmatic actions. Visions seem to have shifted from the sphere of religion into economic and political spheres, changing the character of a vision; initially transcendent and imposed on the experiencing person, into a mundane and intentional form of communication. Considering this, it may cause only little surprise to learn that persons invoking visionary inspiration to legitimate there religious activities are today faced with serious problems. They are widely marginalized and often believed to suffer from some kind of pathology. This reservation is not new. In our culture, since the warnings of Kant against the followers of Swedenborg, there has been strong intellectual criticism against such claims of supernatural inspiration, often interpreted as deriving from mental disorder or similar disturbances of the soul. Indeed, once they were condemned as ›ghost viewers‹. Times have thankfully changed and ritual prosecution and burning at the stake is a phenomenon of a bygone era. However, people claiming to hold some kind of direct connection to supernatural entities are still perceived with considerable suspicion and are often mocked in mass media publications. Similar reactions of hostility and rejection were suffered by the prophets in ancient Israel before the Exile, as Weber (1988 [1921]) reports. And, medieval visionaries were subject to proof about the origin of their inspiration, either believed to be ›angelic‹ or ›diabolic‹ (Dinzelbacher 1981). In this respect, little seems to have changed. But one fundamental change in contemporary views of visionaries is that they are often intended to have a primary function of entertainment.

This also happens to one of the perhaps most well known »prophets« in the German speaking countries, Erika Bertschinger-Eika, alias »Uriella«, who became prominent as a kind of anti-star in the media. Public reactions to her prophecies are less dominated by warnings to prevent from a dangerous leader-lady of a strange religious sect, but by a way of strange appreciation mixed with amused fascination.[32] The trance messages of Uriella serve as a »loudspeaker« of Jesus Christ and entail an interesting problem for communication analysis. That is, how and by which means can an experience that is primarily enclosed in the inner stream of consciousness be communicated intersubjectively and thereby provide access to others? How does Uriella render plausible her claim that the messages she propagates are being communicated *through* and not *by* her? How does she interactionally produce the presence of an invisible entity that is using her as an instrument or a »tool« of communication?

As we will see, this specific problem of presentation – or, to use an expression of Alfred Schütz (1962[1945]): the »paradox of communication« – is resolved in the performance. The analysis that follows will show that those elements of the presentation which account for the theatrical humour to outsiders are precisely the elements that build up the plausibility of Uriella's audition for the members of her community. The expression performance has to be understood as the physical presentation to a co-present audience, to which audiovisual analysis is the method of first choice. In analysing this performance we will distinguish between different levels:

[32] Swiss expert on sects Franz Schlenk points out that Uriella »attracts primarily for her entertainment value, she seemingly amuses and raises roaring laughter. Most people may have problems to take her seriously« (Schlenk 1999).

the framing elements as setting and decorum, four elements of the presentation itself, namely gesture, mimic, text and prosody, focussing on their respective forms and the way in which they are combined in the presentation. This detailed analysis not only allows reconstructing the symbolic stock and the identification of its respective origins, but also gives an answer to the question of how the presentation of a principally intersubjective and inaccessible inner dimension of experience is rendered credible through specific forms of presentation. This inductive analytic approach permits the reconstruction of the reality claim of the visionary performance and the resulting effects of it, seemingly absurd or at least problematic for an outsider.

The analysis, therefore, aims at reconstructing a specific external form of religious experience, that is: a vision. This is a new approach insofar as research in the field of religious experiences mainly draws on surveys (Yamane and Polzer 1994) or narrative data (Yamane 2000). Besides the fragment of video data under scrutiny, the analysis that follows is based on several visits to the field in May and July of 1999, in-depth interviews with Uriella and her husband Icordo. Interviews lasted for three hours and these combined with extensive documentary materials about the community »Order FIAT LUX«.

Analysis

Fragment 1: Onset of Uriella's ›Full-Trance Vision‹



Sec.	1	2	3	4	5	6	7	8	9	10	11	12
Gesture and Mimic	Uriella's eyes are closes (take 1), she inbreathes very deeply and slightly moves her head to one side, before abruptly turning her head to the sky (take 2). This abrupt movement of her head is synchronized with a demonstration gesture, moving her arms with hanging hands in a theatrical manner upwards and closing them, beginning to separate them again over her head (take 3), and then extending them at maximum. She now knits her lips together and her closed eyelids contract (take 3). For a short moment, an ecstatic expression shines up in her face. She moves slightly up her whole body, her mouth opens, but is being closed again without a word leaving her lips. She then begins to sink together a bit; the closed eyelids are now slightly turned downwards. Her whole head begins to drop a little to the front											
Text	.hhhhhhh <inhalating>			(7.0)							<<t> meine *my*	
Montage				[title masking:] „Uriella empfängt in Volltrance eine Originalbotschaft von JESUS CHRISTUS“ *Uriella receives in full trance an original message from JESUS CHRIST*								

Image 1: transcript of the onset of Uriella's visionary performance

Following the principle of an upgrading contextualization, the analysis starts with the core element – that is: the (recording of) the visionary performance. Subsequently, I will introduce elements of the wider context of the field in which this performance is embedded. The fact that we are not analyzing the situation but its recording will be ignored for the moment in an attitude of »artificial (methodological) naïveté« (Hitzler 1991), but I will return to that aspect at the end of this article.

Inner Structure: Text, Gesture, Mimic and Prosody

Looking from the order FIAT LUX members' perspective, the starting analytic focus aims to explore what is the central point or legitimizing moment for Uriella's claim to be a direct »loudspeaker of Christ« and how is this realised. The following fragment dates from 1995 and is a video recording of one of Uriella's »full-trance messages«.[33] Of special interest is the onset of this message, as it marks the beginning of the transcendence communication and entails what seems to be an important change in ›footing‹ (Goffman 1981). For this reason, we will analyze these first decisive seconds of the performance in detail and treat the following trajectory in less detail (see figure 1).

In this fragment, we are looking directly into the visionary's face, observing not only her mouth articulating the »divine words« but also her facial expressions and the movements of her arms. Observe which methods of expression are employed to transmit the visionary message, delivered in free monologue to a co-present audience. The sequence, which in total lasts for around 10 minutes, starts with an extremely long pause, in which the visionary remains completely silent and no words are articulated. Nevertheless, this long interruption is very dense in gesture and mimic. Uriella's eyes are closed (figure 1, take 1), she produces deep inbreaths and slightly moves her head to one side, before abruptly turning her head to the sky (take 2). This abrupt movement of her head is synchronized with a demonstration gesture, moving her arms with hanging hands in a theatrical manner upwards and closing them, beginning to separate them again over her head (take 3), and then extending them to their maximum. She now knits her lips together and her closed eyelids contract (take 3). For a short moment, an ecstatic expression shines up in her face. She moves slightly up her whole body, her mouth opens, but is being closed again without a word leaving her lips. She then begins to sink together a bit; the closed eyelids are now slightly turned downwards. Her whole head begins to drop a little to the front. In total, 11 seconds elapse in a room characterised by complete silence, before she finally starts to speak (take 5).

This long lasting silence of profound significance does not only mark the interruption between normal and transcendent communication. Uriella uses additional bodily signs to signal to her audience that she now is »tuned in«: Therefore, she combines at least three forms: (1) She takes a deep in-breath, which is a classical physical symbol of enthusiastic inspiration, where the inhaling of air represents the spiritual force penetrating and taking possession of the medium. (2) She adopts a

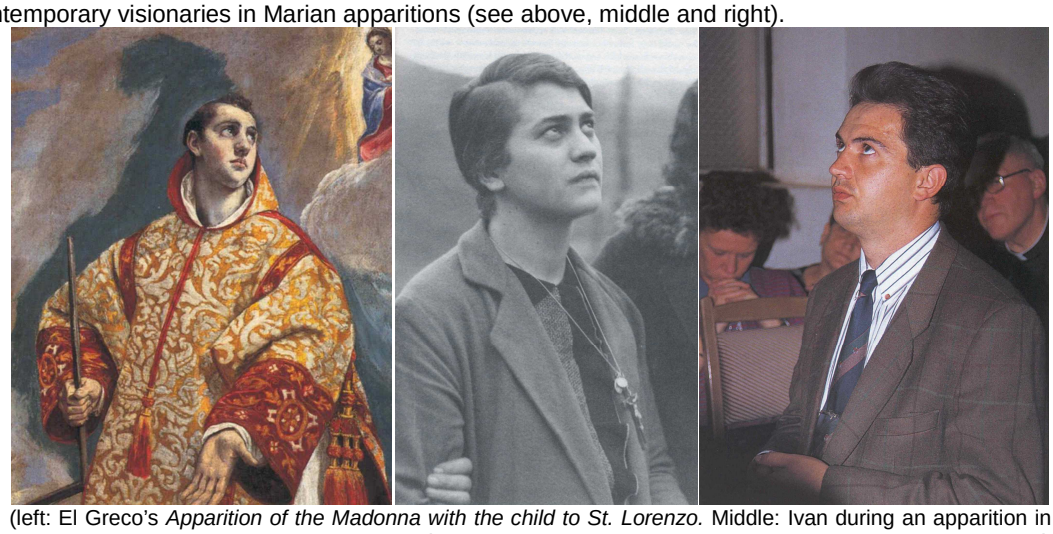
[33] Since the foundation of her community, Uriella had around 600 auditions, communicated in trance to her followers, mainly from Jesus Christ, in some cases from Maria.

head posture commonly seen with visionaries, with the slight variation of closed eyes[34] and a short ecstatic expression flickering over her face. Finally, (3) her arms extend like antennas, and serve to display her as a cosmic receiver of a divine message. It is only after this complex sequence of para-linguistic action, when she finally starts speaking. She begins with a formal greeting (»my beloved children«), interrupted by a clearing of her throat (line 3).[35]

1 U: .hhhhhhh
2 (7.0)
3 ‹‹t› meine ((räusper)) ge´lIEbten ↑KINder. (6.0)
my beloved children
4 ↓S:::ELikh:::::,› (2.0)
blessed
5 sind die ‹‹all+f› ↑H:ONGernden.› (2.0)
are the hungry
6 ‹‹f+h› denn sie werden ge`s:Ättigt^{h}› (-) ‹‹t› WERden.› (1.0)
for they satisfied will be
7 ‹‹f+h› !↑`S:ELig^{e},!=
blessed
8 sind die:: barm: ↑↑H:ERZ:igen. (3.5)
are the merciful
9 .h denn ↑sIE werden barm↑↑HERZich:`kei::t erf:Ahren dÜrfen.› (5.0)
for they will merci be shown
10 ‹‹t› sElik^{h} › (1.5) ‹‹f+h› sind ´JEne::,
blessed are those
11 dIE ´reinen ´↑↑HERZEns sInd^{e} -› (3.0)
the pure in heart
12 denn sie ´wEr`den: (2.5) ↓GOTT ‹‹flüstern+t+f› schAUen^{e}.› (1.0)
for they will God see

Following the salutation (at line 3), three verses from the Sermon on the Mountain (Mt. 5, 6–8) open the visionary speech (see also the full transcript at the

[34] One can find this typical head posture documented in paintings of saints as visionaries, for example in El Greco's *Apparition of the Madonna with the child to St. Lorenzo* (ca. 1777–1580, see above, left) or pictures of contemporary visionaries in Marian apparitions (see above, middle and right).



(left: El Greco's *Apparition of the Madonna with the child to St. Lorenzo.* Middle: Ivan during an apparition in Medjugorje, 15.9.1992, right: Evarista Galdós in vision at Ezkioga, early 1932. Photo by Raymond de Rignè (taken from Cristian 1996).

[35] This transcription follows the GAT-conventions (Selting et al. 1998). See also the selection of transcript symbols at the end of this article.

end of this article). The following vocalization shows on the four different levels – that is: text, mimic, gesture and prosody – these structural characteristics:

The lexicon of the talk includes a series of terms and expressions drawn from a Christian canon, as for example the beatitudes (line 4–12), »God« (line 12, 74, 84), »reincarnation in flesh« (line 28), »Kingdom of Heaven« (line 48), »Hallelujah, Hosianna« (Line 70, 71), »mysticism« (line 85), »the beyond« (line 87), »prayer« (line 108), »penitent sinner« (line 75) and the reference to Trinity (line 115–120). But Uriella also uses terms that clearly do no stem from a Christian tradition, like »divine sparkle« (line 25), »wheel of time« (line 32), »ancestral law of karma« (line 33), »cosmic clock« (line 127), »sublimation of being« (line 51). Although pertaining to different registers, both mark her speech clearly as religious. This special mixture also makes comprehensible the accusation of heresy that Uriella is confronted with from parts of the official Churches. Elements of the Christian tradition are mixed up with those of non-Christian ones and linked to technical metaphors: »therefore everyone has to *repair* that what he has broken« (line 54–56), »the best *glue* to *mend* this broken spots« (line 54–56) »because he has *constructed* his doctrine on this imperishable love« (line 78–79), »the wheel of time is *connected* to this ancestral law of Karma« (line 44-45), »my content of truth that I have *built-in* to my doctrine« (line 100–1001).[36]

Analyzing the textual elements shows how highly sophisticated the composition of the visionary talk is: Uriella (or Jesus) draws on a series of rhetorical techniques to deliver the message. For example, she produces *triple lists*, e.g. in the beatitudes (line 4–12), »thanking, praising, and glorifying« (line 47), »he is…, he is…, he moves, he is (line 18–26); *rhetorical questions* »where, you may ask yourselves« (line 13-14), »if only the people would know« (line 46 ff., line 52ff); and, a series of *causal conjunctions* that knit together quite heterogeneous dogmatic elements in a conclusive relation to each other: »ergo« (line 29), »therefore« (line 37), »then« (line57), »since« (line 66) »for that reason« (line 81), »because« (line 121). She also frequently uses *categorical formulae* like: »God can only be experienced through mysticism« (line 84–85). The text is also loaded with *metaphors* like »being rewarded with the most splendid coat of love« (line 78), »prayer as the most powerful spiritual weapon« (line 110), »sword of love« (line 68, 106), sparkling in the darkness« (line 93–94), »key to the Kingdom of Heaven« (line 47–48). There are also typical *topoi* like »the Holy Ghost that blows everywhere« (line 119-120), the »collapsing house of cards« (line 100–101); and, two motives from the apocalypses: the »gathering of the last third of mankind« (line 122) and the »purification of the Earth« (line 125) in the final part of the message.

But it is not only the density of the textual composition that accounts for the effect of the visionary communication. Above all, it is the way in which it is enacted what creates the visionary character of that communication. The performance acquires its vigour through the simultaneous orchestration of gesture, mimicry, and prosody. The performance is accompanied by expressive gestures, partly used declamatorily to accentuate the text, for example in the moment when Uriella says »Hosianna, Hallelujah« (line 70–71), she throws up her hands, and when ending her speech she stands up, extends her arms and in so doing works to emphasize her words with her gesture (line 112).

The most striking peculiarity of Uriella's performance in this fragment is the *vocal character* of her talk, which differs from normal speech in several ways. Unlike

[36] It would perhaps be more precise to call it a constructivist metaphoric, because repairing, mending, constructing and building resembles the construction process of the Uriellas' new Doxa that seems to underlie her eclectic belief system.

in glossolalia, another form of ›inspired speech‹, she articulates words and sentences in a language understandable to those listening. But there is a noticeably special prosodic pattern that differs from speaking in tongues, which in contrast is composed by strange, non-identifiable words, but follows the contour and melody of the speaker's normal daily language.[37] As the »organ of Christ« Uriella's voice sounds bass, nearly masculine and especially at the onset of her speech sounds sonorous and hollow, and has an almost ghost-like quality. Uriella speaks with the Adam's-apple lowered (the opposite of falsetto), a characteristic of news speakers' and actors' voices, which indicates the performative efforts she makes. She produces a hypercorrect pronunciation, speaking with much aspiration. The rhythm of her visionary speech also differs from that of normal talk, as she drawls and extends the words, takes extended pauses and reduplicates final consonants; aspects which when combined produces her as deciphering and reading an invisible text that is viewed only by her inner eyes. This intonation resembles that of children having recently learnt to read, when they spell out their first lines without being fully cognisant of its meaning. The repetition of final syllables (e.g. in line 7: ‹‹f+h› !↑`S:ELig^{e},!= [blessed]) is reminiscent of a kind of echo imitation. The strange rhythm with atypical pauses, elongations, stretching and change of tempo frames her talk in conjunction with the frequently forced mimic she displays as a kind of effort similar to that of consecutive language interpreters.

Uriella also undermines the preconfigured emphasis of the text. She falls low at very untypical points, a further prosodic characteristic we often find in the articulation of news speakers or in speakers that can not be contradicted in an asymmetric communicative arrangement. This melody of her voice is characterized by a very low basic tonality, from which she rises suddenly in very emphatic moments (see line 93: ‹‹len› ´dIE::se ´↑LEUCHtEn^{e}.› (1.0) [this refulgance]). Generally, her speech stands out for a significantly louder volume, nearly reaching that of screaming at certain moments. This intensity is not justified by the local ecology (a relatively small, closed room, all spectators remain absolutely silent, and, in addition, she is equipped with a microphone). Rather, it is another feature of the performance strengthening the metaphorical might and the power of her bodily possession. As a whole, the performance seems to express vividly how difficult, strenuous and exhausting it is to operate as a mouthpiece of God.[38] That the communication's source emanates from a different realm than that of normal daily life is, as we have seen, marked clearly in and through the internal structural elements of this ›trance vision‹.

The level of situated realization: setting and decorum

It is not only the talk in itself that includes *liturgical elements* framing the speech as a religious message (e.g. opening address »my beloved children« in line 3 or the final blessing, line 122ff). This speech is also embedded in a fixed course of action, a liturgy of a periodically celebrated religious service: the community's monthly held holy service that takes place every first Sunday in their ›sanctuary‹ near Ibach, a small village in the Black Forest. This service is preceded by abstinence during several days, in which Uriella gets »spiritual injections«. On the day of the service,

[37] As Stark points out: »Glossolalia has not the sense, but more or less the form of the popular language around« (Stark 1987). But see also the work of Goodman who, in contrast, has observed stable sound patterns independent from culture and therefore argues for a different, neurophysiologic explanation of glossolalia (Goodman 1972).

[38] Cf. for the display of suffering of Christ as a part of the visionary charisma the chapter on »Visionary Imitatio Christi« in Benz (1969).

Uriella – who also practises as a spiritual healer – receives an audience of followers that consult her for spiritual or health advice. Thereafter the congregation gathers. After three quarters of an hour of spiritual music Uriella enters the room and salutes her community, and this is followed by a piece of harp music, the Lord's Prayer, and a twenty minute exercise of »the Sending of the Light« (FIAT LUX's regular prayer). Next the »Laudate Dominum« is played whereupon the visionary performance begins. During her presentation, Uriella is seated in front of the audience on a slightly elevated throne-like chair, from which she stands up at the end of her performance. After finishing the 10–15 minute presentation, a Gregorian chorus sounds from a record.

In contrast to prophetic messages, which tend to be enunciated spontaneously and are directed to a reluctant and heterogeneous audience (Weber 1988 [1921]), the arrangement and the social composition of this auditory rather resembles the form of a traditional community of Sunday church-goers, though, in this case, restricted by a special exclusiveness. The content of the visionary message points directly to the community itself and its internal affairs; it contains spiritual tutoring, edification and direct and practical instructions on how to cope with particular problems of the daily life, rather than being directed to a broad public of non-believers or renegades, which are threatened with the Judgment of God and which are persuaded to radical conversion.[39] The *decorum*, as well, follows the same pattern. A strict clothing norm unites FIAT LUX's adherents, which demonstrates their membership to a specific religious community and visibly segregates them from non-believers. They all wear white cloth, and cultivate a unique aesthetic that can not hide its roots in traditional popular Marian adoration, even though it has a slightly more modern character. To give an example, the white skirt that Uriella wears in the performance is reminiscent of a wedding dress, which can be read as an allusion to the bride-of-Christ metaphor; her sparkling diadem is also reminiscent of holy first communion attire and an angel costume from a nativity play. The colour white symbolism is repeated in the clothing of the order's members and the order's cars. Cloth, furniture and the whole housing equipment that serves for living, working and religious service of the community are carefully selected and display a consistent style, which is repeated in the architecture of the housing. The sanctuary of FIAT LUX in the Black Forest is surrounded with impeccable white Madonna statues in the centre of a green scissor-cut lawn and a small lagoon with floral arrangements. These characteristics work to further distinguish their community from that of ›others‹, a practice common to other sects across the world.

External context: the medium

We will only shortly refer to the reflexive level of the video data. To this point, we have taken the video recording as a kind of window to the situation, a looking glass giving direct access to the visionary performance, without considering the sort of data.[40] The above analyzed sequence is actually part of a semi-professional »documentary video« produced and edited by the Order FIAT LUX. It was first published in 1995 and lasts about 90 minutes. On its cover, this film is presented as »giving an insight into the community of Fiat Lux«. Its plot follows the scheme of the six works of mercy (cf. Mt. 25, 35–40: I was hungry…, I was thirsty…, I was

[39] See the publications of the visionary messages in the community's own journal »Der reinste Urquell« (»The most pure ancestral source«), formerly called »The hot wire« (»Der heiße Draht«).

[40] Or, as Raab and Tänzler (2006) call it: the second and third analytic level: that of camera action and editing. For the distinction between different sorts of data cf. also Knoblauch (2004).

imprisoned). The scene containing the trance visions is the longest thematic unit of the complete film. Throughout the whole scene, the camera position nearly does not change during the entire 10 minutes. It varies only slightly when zooming into Uriella's face during her vision and it changes to a medium long shot at the end during the moment when Uriella gives the final blessing. There are a total of 22 scenes cut into this take of Uriella's face, showing the co-present audience from different angles, all demonstrating various forms of devotion, prayer, absorption, and – towards the end – also ritualized interaction.





Sec.	72	72	73	74	75	76	77	78	79	80	81	82
Gesture and Mimic												
Text	Wo *Where*			(4.0)							werdet ihr Euch fragen *will you ask yourself*	
Montage				Cut					Cut			

Image 3: the co-present audience at the visionary performance

These cuts have a clear function, they show the shifting of experience by which we – the video spectators – are able to view how those co-present react to the performance (see image 3). The cut-ins, therefore, operate like a guide on how the audience ought to interpret the recorded situation. In this sense, the editing of the raw recordings does not primarily show its supposed manipulation. It rather documents how the editors of this material react to the specific problem and anticipate the subsequent difficulties people viewing the screen may have when attempting to decipher the meaning of the video. The inserted subtitle »Uriella receives a full-trance-message from Jesus Christ« (see above image 1, shot 4) serves the same purpose: invisible to the co-present audience, it instructs the media-audience about the character of the ongoing action. It is a clearly identifiably trace of how the editors try to deal with the split-audience problem.

To sum up the results, the detailed analysis of this short fragment reveals a broad array of interpretative aspects, thus creating a need for a dense interpretation of this case of visionary proclamation. In addition, the fragment illuminates some of the community's specificities which serve a key function. The microanalysis demonstrates the concerted performative efforts that are employed to convey Uriella's visionary messages. We did not only discover single dramaturgical elements referring to the text, the gesture, the prosody of the visionary communication. Rather, the analysis also leads to an encompassing interpretation of a specific form of community building in this new religious movement. Fiat Lux turns out to be a community showing quite modern facets, present not only in their experience-centred religiosity. It is also visible in their ritual of aesthetization and, finally, in the use of

modern technological equipment for the propagation of the visions. The visionary message is not restricted to the co-present audience but transmitted simultaneously via telephone to those members of the community living in the other two centres in Switzerland and Austria. They are also tape-recorded and transcribed in preparation for publication in the order's bulletin »The Hot Wire«, and, in some cases, published in press releases.[41] Moreover, Uriella's frequent appearances on the television and that of her husband Icordo clearly demonstrate that the claimed detachment from the mundane is obviously not the correct characterization for this religious community, in spite of a strictly followed ascetic way of life and the firm regulation of its member's conduct.[42] Rather, one could speak of a quite market-oriented form of religiosity, which is grounded in the following observations: (1) There is a kind of *double mediatization*, in which a traditional medium's messages are disseminated to a second order via mass media techniques (the video); (2) the open *syncretism* of the community, combining elements of tradition Christian belief with non-Christian elements of other religions and that of a popular scientific belief systems; (3) this accounts for a quite manifold and flexible religious order that works together with the service of spiritual healing; (4) a corporate identity fostered by ›corporate design‹, present in the specific sophisticated symbolism of costume, ornamentation and jewellery which when taken together, may be a persuasive and compelling religious proposition to some followers. Its appeal may, however, be restricted to a particular population segment. Surely, FIAT LUX's special dogmatic and aesthetic pattern is not primarily attractive to the average middle classes, as the analysis may have indicated.

Conclusion: Video analysis and ethnography

Coming back to methodology, I will conclude with some observations that can be drawn from this example. This analysis illustrates how visual data collected for ›focussed ethnography‹ can be usefully scrutinised by using the approach of genre analysis. This approach has been successfully applied by others (Ayaß 1997; Keppler 1985; Knoblauch and Raab 2001) and has proved to be especially apt for analysing ritual performances, as the studies of Kotthoff on Georgian toast performances (1995), lamentations (1999) and grieving (2001) have shown.

Naturally, as already mentioned above, when referring to the character of video as a quasi-natural document, the recording does not encompass all sensual aspects of the performance. Freezing action and interaction into video data actually transforms the original situation into analyzable data, a process in which various aspects pertaining to the performance are lost, e.g. the spatial, olfactory and tactile dimensions. Therefore, analyzing the data is not an exhausting method for an analysis of performance.

What I have shown in this article represents a slight variation of the usual way that genre analysis proceeds, as it focuses on primarily *recurrent* structural aspects of communication, whereas I have centred my analysis on a single data fragment, which may be criticised by orthodox methodologists. Nevertheless, I augmented the single case analysis with insights from ethnographic data and documentary materials that served to contextualize the fragment of video data that was analysed in detail. In the course of analysis, I have introduced in a stepwise fashion broader contextual

[41] Broad interest caused the press release of August, 8th, 1998 that contained a prophecy of doom for august (cf. http://www.relinfo.ch/ofl/august98.html, visited Jan, 9, 2001)

[42] See the orders' regulations as formulated in the document »Mirror Image of the ORDER FIAT LUX« (undated).

information by drawing on data that is not evident in the video fragment. In fact, we would not have been able to decipher the meaning of certain aspects of Uriella's visionary performance without our field observations and the information gathered in interviews; neither would we know how this episode is embedded in the community's daily life and the centrality of its role for them. Therefore, I would argue that not only for the purposes of this singular case but in general, the analysis of video data needs to be augmented by focussed ethnographic fieldwork and other forms of social research (such as interviews, documents, and even survey data). Restricting the analysis exclusively to the video data and its interpretation carries with it the danger of drawing conclusions that contradict the situation and thereby running into the methodological problem of missing the ›postulate of adequacy‹ (Schütz 2004[1953]).

What, in my view, still remains open is the question of how single case analysis following the hermeneutical interpretation combines with ethnographic field research. Without going into details, I would argue that the methodological debate still lacks a solution for the challenge of how to combine a hermeneutical approach with linguistic-structural approaches in interpretative sociology. One of the most relevant problems in video-analysis is the problem of selection and the search for methodological principles that would adequately account for the reasons an analyst selects certain fragments to scrutinize in detail over others. I suggest that the hermeneutical practice of producing an exhaustive amount of context-free reading versions and its stepwise consecutive exclusion from within the material needs to be combined with a more corpus-oriented analytical approach as practiced in conversation or genre analysis. In addition, it is essential to take into account the broader distal contexts in which data fragments are actually produced and embedded. Consequently, ethnography is essential for research in any area of modern, pluralized societies with wide-ranging and highly fragmented areas of local culture and situated knowledge. Even the most erudite interpreter does only have limited insight into the rich and manifold segments of contemporary social worlds. Therefore, ethnography is indispensable for qualitative research.

One last point has to be considered. Along with the greater availability and use of digital technology, the analysis of video data is increasingly gaining importance in qualitative sociology. Due to the popularization of video equipment, the practice of videography may spread even in people's everyday life. As a result, this produces a growing corpus of visual recordings, recordings that can be analyzed as auto-ethnographic data. This is especially important for an insight in fields with difficult or restricted access, for which these recordings gain special importance in ethnographies as in the case at hand, where participation is only allowed for members of the community after long times of spiritual exercise and strict ascetic purification, entailing the renunciation of alcohol, coffee and cigarettes during weeks beforehand. Nevertheless, I would not suggest the employed style of analyzing data is apt for a general approach. Surely, it has to be tested in further studies, where its advantages and limitations of generalization are explored. Without doubt, in ethnography, every field has its peculiarities and therefore may need an approach other than video.

Notwithstanding, video recordings will change the way in which field work is conducted for many researchers. The practice of videography is expanding throughout many areas of qualitative research (Knoblauch 2006). Different data types have to be distinguished in video-analysis (Knoblauch et al. 2006a), each entailing its own methodological challenges. Beside other types, video data produced by members itself, like the one analyse above, is of special interest for sociological interpretation. It may open up areas of restricted or difficult access for research.

Moreover, it contains the members' self representations. Therefore its analysis allows studying the forms by which member construct and represent their own reality. Admittedly, even video is not able to produce genuine copies of that practice in all its details; though that is not the aim at all. Rather, these recordings provide insights selected by the members themselves. Hence, auto-ethnographic video data is a privileged resource which can provide access to the mediated ›ethnotheory‹ of members, whose reconstruction is one of the ongoing tasks of focussed ethnography.

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Annex: Transcript of Uriella's "Full-Trance Vision"

1 U: .hhhhhhh
2 (7.0)
3 ‹‹t› meine ((räusper)) ge´lIEbten ↑KINder. (6.0)
my beloved children
4 ↓S:::ELikh:::::,› (2.0)
blessed
5 sind die ‹‹all+f› ↑H:ONGernden.› (2.0)
are the hungry
6 ‹‹f+h› denn sie werden ge`s:Ättigt^{h}› (-) ‹‹t› WERden.› (1.0)
for they satisfied will be
7 ‹‹f+h› !↑`S:ELig^{e},!=
blessed
8 sind die:: barm: ↑↑H:ERZ:igen. (3.5)
are the merciful
9 .h denn ↑sIE werden barm↑↑HERZich:`kei::t erf:Ahren dÜrfen.› (5.0)
for they will merci be shown
10 ‹‹t› sElik^{h} › (1.5) ‹‹f+h› sind ´JEne::,
blessed are those
11 dIE ´reinen ´↑↑HERZEns sInd^{e} -› (3.0)
the pure in heart
12 denn sie ´wEr`den: (2.5) ↓GOTT ‹‹flüstern+t+f› schAUen^{e}.› (1.0)
for they will God see
13 .h !↑WO!. (4.0)
where
14 ‹‹f+h› werdet Ihr euch `FRAGen, (4.5)
may you yourself ask
15 er ↑↑Ist ja ↑↑ÜberA:LL. (2.5)
he is everywhere
16 in dieser geWALTigen^{e} (.) natU:r (.) und krEatUr.› (5.0)
in this tremendous nature and creature
17 in (d)er unENDlichkeit^{t} des:: (.) grEnzenlosen Alls::. (3.0)
in the infinity of the boundless universe
18 E:R .hh – (2.5)
he
19 ist dÄ:r SCHLA:G .hh – (1.0)
is the beat
20 eurer HERZen. (2.0)
of your hearts
21 er ist der GLANZ eurer ‹‹all› AUgen.›› (4.5)
he is the splendour of your eyes
22 er bewEgt eure ↑GLIEDer. (2.5)
he moves your limbs
23 ‹‹f+h› E:R IST^{h}; (1.0)
he is
24 ↑DA:S L:ICH:::T^{h}.
the light
25 .h da:s::: (2.5) als gOttesFUNKe; (1.0)
that as divine spark
26 In EUch (1.0) BR:ENNT.›
in you burns
27 .h ‹‹f+h› ↑wÜssten die MENschen; (1.5)
if only know the people
28 .h Um^{e} ´die ´wiedergeburt im `FLEIsche, (1.5)
about the reincarnation in flesh
29 Ergo; (.)
ergo
30 ↑nIcht `nur im GEIste, (1.0)
not only in spirit
31 dAnn^{e} würde vieles auf ´die::sem^{e} (.) glO`´bus ↑ANders AUssehn.›

then would much in this globe different look

32 `das ´RAD der ZEIT. (0.5)
the wheel of time

33 ist an mein ´URgesetz des KARmas gebUnden. (1.0)
is to my ancestral law of karma bound

34 in ´↑IHM^{e} r:uht^{h}; (2.0)
in it rests

35 ↑UR: `sAche, (2.5)
cause

36 und ↑WIRkung. (3.0)
and effect

37 ´↑DEMzu folge. (1.5)
as a result

38 muss ´↑EINjedes;
must everything

39 ´↑DAS was Es; (2.0)
that what it

40 ´↑ZERbricht; (2.0)
breaks

41 wIEder (.) ´↑SELBST r:epa↑rIEren. (1.5)
again (it)self repair

42 und der ´↑BESTe lEIm. (1.5)
and the best glue

43 um ´dIE:se ´schAdSTELLen; (3.0)
to this damaged spot

44 zu ↑FLICKen. (1.0)
to mend

45 ´trÄgt den ´nAmen LIEbe. (1.5)
carries the name love

46 ´↑wEnn:: `doch die ↑MENschen wüssten. (1.5)
if only the people would know

47 dass im ´DANKen ´LOben und ´PREIsen ein schlÜssel; (.)
that in thanking praising and glorifying a key

48 zum ´hImmel´REICH liegt^{h}. (2.0)
to the Kingdom of Heaven lies

49 DANN^{e} , .h (1.0)
then

50 würde es ´↑AUch noch ´SCHNELLer vorAngehn;=
would it also much faster go ahead

51 mit der ver´EDelung ihres SEINS. (2.0)
with the sublimation of their being

52 wenn doch die ´↑↑MENSCHen ↑wÜssten. (1.0)
if only the people would know

53 DASS::: - (.)
that

54 im em´↑↑PFANG`en, (1.5)
in receiving

55 ↑und GEben. (.)
and giving

56 .h ein ´↑U:Rgesetz gOttes ruht. (2.0)
an ancestral law of God lies

57 DANN^{e},(2.5)
then

58 ´wÄre das ANTlitz. (1.5)
would be the visage

59 ´VON^{e} MI:R; (-)
of me

60 ↑NICHT `mehr ´so dur:chfUr:cht; (2.0)
no more so furrowed

61 von ´FALten des `KUMMers und der ´sOrgen. (2.0)

by wrinkles of sorrow and of worries

62 .h die ´mEnschen haben das `bÖse zu einem ↑AB GOTT geSCHAFFen:;
the people have the Evil to an idol created

63 dEn `sIE: (-) ver´GÖTTern. (.)
that they adore

64 mit[h] vergÄnglichen[e] WERTen[e].
with perishable values

65 die für ↑GOTT ↑↑nie zÄhlen;=
which for God never count

66 nenn er ↑HAT seine lEhre.
for he has his doctrine

67 au:f dieser unverGÄNGlichen L:IEbe AUfgebaut[h].
on this imperishably love build on

68 .h daher ist ↑`SIE die `krOne der ↑schÖpfung;
therefore is it the creation's crowning glory

69 und nicht der ME:NSCH:; (.)
and not man

70 .h ´DE:R mich `jA:: nach dem ! ↑↑↑HALLELUJAH::!;
who me afte the Halleluja

71 !HO::SIANNA!; (.)
Hosianna

72 ans ´krEUz ge´nA:gelt !HAT[h]!. (1.0)
to the cross has nailed

73 ´↑WUNderbAr. (.)
wonderful

74 möge doch ´AUch für euch die ge´WISSheit sein. (1.5)
may be also for you the certainty

75 dass jEder ´rEUige SÜNder. (1.5)
that every penitent sinner

76 ´VO:N GOTT. (.)
by God

77 aufs ´REICHST- (1.5)
to the most plentiful

78 ja mit dem ´schÖnsten `mAntel beSCHENKT wird. (1.0)
yeh, with the most beautiful coat will be donated

79 die `WAHre ´HEImat; (1.0)
the true homeland

80 be´fIndet sich ´DRÜben. (.)
is situated yonder

81 dAher hAbe ich AUCH; (2.0)
therefore have I also

82 den ↑R:ICHtern die über mich zu entscheiden hatten zugerUfen. (1.0)
the judged which had to decide over me shouted at

83 ´I::CH BIN NICHT› ‹‹l› von dieser wElt[h]. (2.0)
I am not of this world

84 GOTT.›
God

85 ‹‹f+h› kann nur ↑In der MY`stik (.) ‹‹t› erlEbt werden.› (2.0)
can only trough mysticism be experienced

86 ‹‹f+h› OH:NE: ↓MITler.
without intermediary

87 zwIschen[e] dem JE:Nseits und DIESseits. (.)
between this world and the other

88 hätte ´mEIn WAHRHEITSgehAlt.
could have been the content of truth

89 .h den ich in ´MEINer LEHre EINgeBAUT hAbe;
that I have incorporated into my doctrine

90 .h !↑NIE!.
never

91 unter die ´mEnschen ge`brAcht werden KÖNNen. (1.0)

under the man could be brought
92 ´je`DOCH;
however
93 ‹‹len› ´dIE::se ´↑LEUCHtEn[e].› (1.0)
this refulgance
94 In:[e] der FINsternis dieser MENSCHheit und WELT. (.)
in the obscurity of this mankind and world
95 werden von den !↑UN!wIssenden. (-)
will be by the ignorants
96 be´KÄMPFT; (.)
combatted
97 ja am lIEbsten möchte man sie auf dem schEIterhaufen verBRENNen; (.)
yeh, they would prefer them at the stake to burn
98 .h ´wEll DA:S; (.)
because that
99 was über ihre LIPPen:[e:] QUILLT[h];
what over their lips swells
100 .hh das gAnze KARTengebäude; (1.0)
the whole house of cards
101 ´där ´mEnschen[e] ´zUm[e] ´↑STÜR:zen BRINGT[h]. (4.0)
of the people to collapse brings
102 die ´pOlariTÄT – (.)
the polarity
103 die auf eurer ´Erde HERRSCHT; (-)
that reigns in your Earth
104 trägt (.) ↑DIE:Sen[e] gEgenpOl.
carries this conterpole
105 und er kann NUR – (.)
and he can only
106 mit MEINem schwErt der lIEbe; (-)
by my sword of love
107 IN BANN (.) ge´hAlten werden; (2.0)
in check being kept
108 ‹‹t› das ge› ‹‹f+h›´BE:T – (.)
prayer
109 ist die STÄRKste. (2.5)
ist the most powerful
110 ´GEISTige `WAFFe die es gIbt.› (5.0)
spiritual weapon that exists
111 ((Stuhlrücken))
((moving chairs))
112 ‹‹t› mit ´AUS ge breite´ten Armen. (1.0)
with extended arms
113 ver´ABschiede ich mich von EUch:; (1.5)
say goodbye I to you
114 ´Im ´NA:me:n; (-)
in the name
115 ´und in der ALLmacht des Ewigen gOttVAters. (3.0)
and in the almightiness of the Eternal Father
116 in der lIEbe seines eingebO::renen sOh::nes jE::sus CHRISTus:: - (1.5)
in the love of his native son Jesus Christ
117 dEr: zU EUch ↑SPRICH:T.› (.)
who to you talks
118 ‹‹h› Un:d in: der KRAFT; (.)
and in the power
119 des HEI::Ligen gEIstes:::. (-)
of the Holy Ghost
120 die ↑Ü::Berall wEht[h:::::].› (3.0)
who everyway blows
121 ‹‹t› WEIL.› (-)
because

122 das lEtzte drIttel der MENSCHheit$^{h:::}$;(1.0)
the last third of mankind
123 zus:Ammenge´BRACHT werden muss::. (-)
together must be brought
124 ‹‹t› um (.) in› ↑BÄL:de; (-)
so in brevity
125 ‹‹t› die› ↑REINigung EInlEIten zu kÖnnen. (2.5)
the purgation starting can
126 ‹‹t› alle› ↑VO:Rbereitungen sind getrOffen. (4.0)
all preparations are made
127 ‹‹t› die› (.)↑kOsmische UH:R; (.)
the cosmic clock
128 steht ‹‹h› nUr noch hUnderstäl: seKUNden;›
stands only hunderth of seconds
129 vor der ´`ZAH:L (.) Z:WÖLF.
before the number twelve
130 ‹‹singen›› amen›
amen
131 G: ‹‹singen› amen›
Amen

Selected transcription symbols

(-), (--), (---)	short pause, one dash representing 0.25 sec. approx.
(2.0)	pause (in seconds)
.h, .hh, .hhh	exhaling, according to its duration
gesättigt^{h}	strongly aspired final consonant
:,::,:::	extended, according to its duration
?	high rising intonation
,	medium rising intonation
;	medium falling intonation
.	low falling intonation
akZENT	primary or main accentuation
ak!ZENT!	extra strong accentuation
↑	high onset
↓	low onset
<<h> >	low register
<<f>>	forte, loud
<<all>>	allegro, fast
<<acc>>	accelerando, accelerating

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Wading the Field with My Key Informant: Exploring Field Relations

Abstract

Entering and staying on in the field or rather avoiding being kicked out are the two classic ethnographic challenges. The rather positivist nature of textbook guidance on dos and don'ts in fieldwork in general and in delicate issues in particular (for researchers` dilemmas in the field see Ryen 2002), tend to recommend a gentle, middle-class (rather female) interactional style. This gaze suffers from being both researcher-focused (cf.Fine 1994 on "Othering") and based on problematic pre-fixed identities nailing us to the role pair as researcher and key informant. As the introductory extract illustrates, it takes patience also to have an ethnographer "hanging around". This article deals with the credibility of qualitative research when accounting for or exploring how we do staying in the cross-cultural field and it asks how can we credibly explore the stamina that takes us further? If we accept fieldwork as social interaction, we need to bring the social (or the "inter") of it into the exploration of our puzzle. Membership categorisation device (MCD) offers to take us closer to understanding and piecing together our puzzle, but to better get at the events taking place in field interaction there is a need also to introduce the wider cultural context and the images available (or not) to members. In this way I recognise the ethnomethodological differentiation between topic and resource, but argue that when understandings and images are not necessarily culturally shared and collective, we also need to make problematic how members deal with the unavailability of shared images. In the conclusion I argue that the artful side of the local interpretive work in the field is closely entangled with whatever meanings or images are available for construction (in line with Gubrium and Holstein 1997:121). In cross-cultural contexts more than in others, this is particularly delicate because in such contexts images and experiences often do not connect and may lead to complications or even breakdown in communication (Ryen 2002). Mending or repair thus becomes another crucial phenomenon, itself complex, in the evolving field relations. The analysis thus pinpoints the artistry of members` local collaborative efforts accentuated when constrained by images or descriptions that do not connect across cultures. This makes stamina a joint effort, though itself an intricate, emergent phenomenon. Next I will briefly introduce a couple of classic works on working with key informants followed by a brief presentation of the analytic approaches to be applied to my data from East-Africa. Before concluding, I will comment on "wading the field" as reflected in the close exploration of the cross-cultural extracts.

Keywords

Key informant; Qualitative research; Membership categorisation device; Credibility; Cross-cultural research; East-Africa

Extract 1:
1 Anne : and she was here in September.
2 Mahid: yeah. When she left, it happened after that.
3 Anne : She said ´she did it. Why does she say ´she did it, you say ´you did it?
4 Mahid: Ok, ´she did it, all right? Are you happy?

(Uganda 2004)

Informants have been seen as important not only to get access to activities not directly observable, but also in validating conclusions based on the ethnographer's own observations (Hammersley and Atkinson 1996:152; Ryen 2002:17, see Gerald Duane Berreman`s classic work *Behind Many Masks* 1962 on changing key informant). However, by time criticism pointed to the objectifying and colonising gaze of the Western anthropological tradition (Kincheloe and McLaren 1994:152) with relevance both to fieldwork and field relations (Fonow and Cook 1991; Clough 1992, Marcus and Fisher 1986; Clifford and Marcus 1986). The criticism advocated the informant's voice both in the field (experience) and in the report (representations of experience, Richardsson 1991)

This debate was linked with reflexivity concerned with the researcher's self-subjectivity, radical reflexivity or self-consciousness. However, as rightly put by Gubrium and Holstein this focus on the researcher's self was also closely affiliated with narcissism and posed a risk to overthrow qualitative inquiry's need to go beyond the analyst (Gubrium and Holstein 1997: 112). And, we could add another, - ironically to invoke a neo-colonial "othering" by leaving the researched behind in the field. Rather, we need a place for both members which I argue to be essential for both practical and analytic reasons when exploring ethnographic field relations across cultures.

The history of the informant: "the other" becoming a member

Guidance in research is always epistemologically anchored (Ryen 2008a). This also applies to portraits of informants throughout history. The encounter between the Westerners as travellers, missionaries, colonialists or researchers and those of the foreign habitats slowly came to influence perceptions of "the other" (Vidich and Lyman 1994:23-59). As to "my" (African and Asian) field in East-Africa, relevant illustrations would be the colonial ethnographic era with explorers` reports such as by Henry Morton Stanley and others. As argued by Anne Hugon (1993: 122[43]) hardly any of these paid any tribute to the effort and local knowledge of their local African companions who went on their expeditions. As to "othering" Asians, Rozina Visram`s book (2002) on Asians in Britain throughout the last centuries or John Campbell's chapter (1999) on the making of the Asian settlement in urban Tanzania, illustrates this well.

In research William Foot Whyte`s (1943/1981) *Street Corner Society* and Paul Rabinow`s (1977) *Reflections on fieldwork in Morocco* represent two classic ethnographic works that invite us inside both to the substantive side of their fieldwork as well as to their epistemological reflections on this activity though in different ways. Successively such reflections lead to the methodological self-consciousness that eventually has given us the analytic choices that made us consider our own

[43] Alternatives could be the travelogues from British adventurers on hunting and fishing expeditions in Norway 18-19th centuries like Lees and Clutterbuck (2001 originally published 1882)

representational practice (Gubrium and Holstein 1997:110) hence the analytic choices to explore our own puzzle.

Reports from the field: The informant as insider

The first editions of Whyte`s classic book mirrors the change in the growing methodological reflections of the time. The early edition is a classic naturalistic report from the field focusing on life on the corner based on Doc`s inside reports to Whyte across the four year period when he stayed with Doc, Sam and the other boys[44]. Especially Doc`s reports from the street gangs made Whyte slowly come to grasp the life on the corner (Whyte 1981) or "there":

> The life of the corner boy proceeds along regular and narrowly circumscribed channels. As Doc once said to me:
>
> 'Fellows around here don't know what to do except within a radius of three hundred yards. That's the truth, Bill. They come home from work, hang on the corner...Most of them stick to one corner. It's rarely that a fellow will change his corner...'
>
> …The group structure is a product of these interactions (p. 265)

However, in the later edition of his book, Whyte shares his story "On the evolution of Street Corner Society" (Whyte, 1981: Appendix A 279ff) with the readers. He now offers methodological comments on the complexity of the ethnographic professional-private divide, and how being immersed in the data impacts on the process of analyses. The patterns he comes to see gradually emerge out of this very method (Whyte, 1981:279): "…If on the other hand, the researcher is living for an extended period in the community he is studying, his personal life is inextricably mixed with his research…"

We cannot conclude that methodological reflections originally were missing, but rather that not until later were these made explicit and published allocating Doc a more vital position in his analyses and at times even a naturalistic co-analyst[45]. However, the methodological section does signify an important move towards the methodological self-consciousness referred to above, and reflected and announced the analytic tension and choices ahead. Paul Rabinow takes this further.

Externality as a moving ratio: informing as an intersubjective construction

In his Reflections on Fieldwork in Morocco (1977) we meet Ali, Malik and his other Moroccan companions in Sefrou, a town in North Africa. Rabinow, famous for his work with Foucault[46], joins in the methodological debate, but from a postmodernist perspective.[47] Thus, his portrait from the field is different from Whyte`s.

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[44] Another parallel example would be Elliot Liebow and his African-American key informant Tally Jackson from the inner city of Washington, D.C. in his book *Tally's Corner* well described by Gubrium and Holstein (1997).

[45] Critics still maintained that despite these reflections Doc remained somewhat on the margin both in Whyte's later project-related activities and to the economic outcomes from his work (Richardson, 1992, also argued to be ethically problematic, see Ryen 2004. It also shows the potential dilemmas involved in balanced relations).

[46] at Berkeley (http://www.answers.com/topic/paul-rabinow) (retrieved Oct.10, 2008)

[47] This era also set off more experiencing texts like Ruth Behar (1993) and her doubled-voiced text with "Esperanza" in her book *Translated Woman* (1993) where she transcends the anthropological form of the life history (Olesen 1994:167) and challenges what is argued to be an old colonialist approach.

Rabinow`s reflexivity makes his informant into a mediator somewhere in between an insider-outsider with no final "there" as opposed to Whyte`s corner with Doc more or less as a passive reporter. To tell something to someone who is unfamiliar with your culture rather demands that he or she needs to operate somewhere in between the in- and outside, or more precisely be on the move. The "facts" we are searching are themselves constructions. The key informant is actively recognising and handling the impossible of simply passing on "true" stories about social realities of this world unfamiliar to the researcher. (S)he thereby becomes an active participant constructing the field rather than just telling it "as it is". The challenge is with how the information can be passed on to the researcher, "…an outsider who is by definition external to his [the anthropologist's] usual life-world…This externality, however, is a moving ratio. It is external both for the anthropologist (it is not his own life-world) and for the informants, who gradually learn to inform….This informing, however, goes on not in a laboratory but in interpersonal interaction. It is intersubjective, between subjects…" (Rabinow 1977:153-4).

Consequently, Rabinow allocates a vital place for breakdowns in the field or "interruptions and eruptions" (1977:154) as opposed to the worries in classic textbooks. These "ruptures of communication" become core aspects of this kind of inquiry by representing turning points and they start new cycles from which a new depth in their communication can develop. This marks an abrupt break with the traditional naïve worries in the search for harmony. Rather it is through these events that more come to be incorporated and more can be taken for granted and shared. It is this moving ratio which is the change and the informing:

> Fieldwork, then is a process of intersubjective construction of liminal modes of communication …the subjects involved do not share a common set of assumptions, experiences and traditions. Their construction is a public process…It is the dialectic between these poles, ever repeated, never quite the same, which constitutes fieldwork. (Rabinow 1977:155)

Now, if reality is social, "the other" is slowly recognised as another subject and the researcher cannot claim monopoly on beholding the key to the correct version.[48]

The insider-outsider dilemma: The informant and researcher as members

Initial fascination, monetary rewards, status, assumed potentials of networking etc. could all work as immediate explanations at least to informants` stamina, and the quest for data as the cardinal explanation as to the researcher. However, these explanatory devices are all commonsense explanations from everyday life used as resources to explicate what is going on. According to ethnomethodology (Garfinkel 1967; Sacks 1992) the problem with relying on members` own explanations is that it makes us an integral component of the very world we seek to describe (Zimmerman and Pollner 1970). Rather we need to analyse members` (the key informant included) interpretive work on assigning meaning to the reality they make come into being.

[48] This debate came out in many "streams". Feminists were concerned with power as gendered or with the traditional focus on men's experiences (Reinharz and Chase, 2004), others with colonialism and the imperialistic whip either within continents as with the Black Americans or the Red Indians or across continents (Vidich and Lyman 1994), and still others pointed to the basis of categorisations or "othering" as contingent or dependent on geographical, social or ethnic context (Nagel 2003:39). Illustrated with Asians, this would mean that in East-Africa, his or her ethnicity would most likely refer to skin colour, whereas in India to religion, language or region (Latin 1986).

The classic way out would be to interview people about this puzzle, but the problem with interviews according to Sacks, is that this method of asking native informants questions, "…means that they're studying the categories that Members use…they are not investigating their categories by attempting to find them in the activities in which they're employed" (Sacks 1992) [49]. Interviews generate categories rather than seeing how categories are employed because in practice, talk is recipient-designed in the sense that we construct our talk according to how it will be heard. This means we make it possible for the hearer to read our talk in a particular way, and framed by the place or arena in which this activity or event takes place (Drew and Heritage 1992)[50].

This then refers to the ethnomethodological differentiation between topic and resource or simply put as between the hows and the whats. Rather than searching members` explanations or more precisely what they say, the focus is on how members make this phenomenon come into being. This makes talk into an event, an activity. The object of ethnomethodological inquiry then is what Gubrium and Holstein refer to as "worlding" or "reality construction practices" (Gubrium and Holstein 1997:39). This does not imply that the existence of reality is contested, rather we "bracket" our belief in it as put by Schutz (1970:58) by making members` constituting practices our main focus in order to analyse how they themselves produce recognizable forms that are treated as real. We make their ongoing achievement of the social phenomena they are talking about, into the very topics of our research by treating their explanations as indigenous understandings to be studied, not used as resources (Gubrium and Holstein 1997:42).

In their critical comments to this approach, Gubrium and Holstein argue that we also need to look into another side of their constitutive local work; the content of lived experience or the whats (ibidem 1997:107). That is, we also need "to document ethnomethodologically how members` interpretive activities are conditioned by what they experience as the circumstantial realities of their lives" (ibidem 1997:120). Simply, we need to look at what makes their descriptions sensible or not, or rather what conditions these shared descriptions. This brings us to the intricate matter of culturally shared vocabularies, understandings and descriptions or the interactional problems arising out of a lack of such connections as in interactional breakdowns. If descriptions are cultural, this might be meticulously intricate in cross-cultural contexts.

To analyse the local interpretive work I will employ Harvey Sacks` Membership Categorisation Device (MCD) analysis (Sacks 1992; Silverman 1998; Baker 2004a and 2004b) described below, However, I will also look into the issue of available cultural images and the constitutive work in situations where such images not easily connect (Gubrium and Holstein 1997; Atkinson and Coffey 2004).

[49] According to Sacks this calls for naturally occurring data (see Potter 2002), though interview talk can also be approached as an accountable event in itself (see Baker 2004a:790 and Watson and Weinberg 1982 in Baker 2004:792). Hester and Eglin (1997) argue along these lines seeing interviews as displays of culture in action.

[50] The boundary between ethnography and ethnomethodology has been blurred (see Pollner and Emerson 2001) and the genres have been mixed with f.ex. Gubrium and Holstein 1997, and Gale Miller 1991, representing a focus on local culture and with Dough Maynard's rather the micro-social discourse (1989).

Membership Categorisation Device

The central research question for Harvey Sacks is how societal members "see" particular activities and therefore also offer a way of describing "methods persons use for doing social life" (Sacks 1984). As referred above, this makes language into social events rather then just a passive medium for transferring external meanings or experiences (cf. Whyte versus Rabinow). According to Sacks, analyses of such talk-in-interaction have shown patterned regularities, and his "apparatus" or machinery represents one way of analysing such talk, illustrating how "participants orient and respond to each other in an orderly, recognizable way" (Gubrium and Holstein 1997:55, for Goffman`s criticism of this machinery see Silverman 1998: 65-6). This machinery is not the actual categories that members use (like "culture" or other sociological variables), but rather what allows the phenomenon whatever it is, to be done. Members actively construct social reality, and the researcher's job is to describe how this reality is being done. Importantly, members collaboratively make social order happen in their unfolding sequences of talk.

This is the background to Sacks` membership categorisation device[51] (MCD) regulated by a collection of categories and some rules of application (Silverman, 1998:chapter 5, Ryen and Silverman, 2000). The importance is the search for how the persons involved make use of the resources for membership categorisation. We therefore, according to Carolyn Baker (2004a:174), "need to locate the central categories (of people, persons or things) that underpin the talk". We then need to look for "the activities associated with each of the categories in order to find out the attributions that are made for each of the categories". Attributions may be explicitly pronounced or just hinted at[52] "indicating the subtlety and delicacy of much implicit categorisation membership work". We then need to sort out "connections members produce" between categories and attributions "to find the courses of social action implied: descriptions of how categories of actors do, could or should behave" (Baker 2004a:174). This way we can describe how people come to hear or do whatever they do because each of the pairs of MCDs implies common expectations about what sort of activity are appropriate.

Sacks` apparatus is a collection of more general principles behind the everyday, mundane practices people unconsciously employ and take for granted when they talk, and that they often cannot account for if asked to explain, hence the criticism of interviews to make people explain or tell us about "reality" or how it is done. Let us now turn to data.

Wading in the field: Accomplishing field relations

The classic use of native key informants is to ask questions to make them tell us about whatever we want to find out as in the opening sequence from a day in the field. However, ethnomethodology`s focus on the constitutive practice calls for exploring the MCDs in our ongoing doing of field relations.

However, in cross-cultural collaboration we tend to face more challenges since the ordinary taken-for-granted no longer can be taken for granted which may clutter up communication and lead to misunderstandings as well as eruptions.

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[51] This analysis originated in Sacks` works (1972a, 1972b), and was developed by among others Stephen Hester and Peter Eglin (1997, Silverman 1998. Also see Schegloff 2007).

[52] As Silverman (1998:75) reminds us, this way we may be accused of "prejudice" without being explicit. This shows the powerful side of invoking category-based explanations as we know it from the media about f.ex. racism, gender issues etc.

Consequently, we also need to look into doing being disconnected or erupted or the problematic sides of ethnographic work that call for mending.

Doing classic ethnography: The researcher - key informant

Asking questions as shown in the initial sequence (extracts 1) is one of the activities involved in ethnography[53]. In the next Mahid is telling me about how they once in one of his businesses came to hire a professional hotelier. This is shortly after he was recruited to my project:

Extract 2.

1 A: so why did you hire the person?
2 M: I hired the person because I needed a professional hotelier here
3 A: ehe
4 M. ok, none of us were professional hoteliers, ok. And hotel is a job where the kitchen has to be looked
5 after which is very important, the bar has to be looked after, the clients` interests have to be looked
6 after. I had no time for that eh nobody wanted to come forward and I had employed [my cousin] she
7 was working here. Just because she did not have any hotel experience
8 A: mhm
9 M: ok, UNDP came up and said, No, we want to hire a professional…

(Kenya 2002)

This is a non-remarkable sequence of talk about (the budgetary implications – not shown) of employing a professional hotelier. Our interest is with the constitutive aspects that also make my intercepted "ehe" and "mhm" into active responses that work to prompt more information. In this particular extract we jointly invoke the emergent standard relational pair (SRP) researcher and key informant traditionally portrayed as the only legitimate roles in the field rendering an impression that alternatives are ethically or in other ways problematic (Ryen 2004, 2008a). Rather, the flexible and constitutive character of doing field relations makes us see the constitutive or the active doing being in the field assembling the social world.

Scrutinising interactional activities: Business stories as moral restoration work

Not all talk is as smooth as the above. Extracts 1 and 2 are part of a longer communication that developed into talk about benefits and allowances for his staff including the division of work between himself, the owner, and his manager. However, their versions on fringe benefits not only differed, but also opposed each other (for fringe benefits, see Knudsen and Ryen 2005). This topic seemed to touch delicate ground (see extract 1, line 4) accentuated by observations that I also presented to Mahid. Talk about finances then in different ways came to display some of the activities involved in communication. Describing things means "pragmatic selections" from a range of possibilities which makes even "simple" describing into a social and moral activity (Baker 2004:164; Schegloff 1988; Jayyusi 1991). The

[53] Thanks to Mahid for collaborating with me and for accepting me to join in across a number of occasions. He is informed about my different publications including this one. We agree that there are different ways we can orient ourselves to this world (cf. Gubrium and Holstein, 1997) and have also agreed to try to settle any disagreements that do arise including accepting that we do disagree.

extracts below show how the meaning of an action is shaped by the sequence of previous actions from which it emerges whether this sequence is the immediately preceding talk (most common) or simply preceding at some time and thus creating the context for the next person's talk (Heritage 2004:223).

The business owner-researcher complexity in the exterior of pay-packet policy

When explanations do not meet the classic advice is to check and counter check data. But, if talk or members` explanations are context-shaped they can be seen as produced and designed for different interactional purposes. The problem arises when the ethnographer puts responses together (comparing statements from the same interviewee or from different interviewees on the same issue as in this case) treating them as context-free. In my project, Mahid tended to hear such exercises as allegations and they therefore tended to invite trouble. Pursuing such inconsistencies has given way for severe conflicts on the verge of breakdowns which can be seen as violating the language game and thereby cluttering up the speaker's credibility (as in extract 1).

Earlier in our talk where extracts 1 and 2 belong, I introduced some questions on the full wage-packet for his manager or general manager (cf. the inconsistency as to title). Also during our communication he was changing his reports on her salary-level and did not seem to connect with my more nuanced questions on costs associated with her perks. Doing the total cost estimates and adding them to her salary, her monthly compensation package was considerable and far beyond what he as business owner claimed. This seemed to be an unfamiliar way of estimating total salary or the wage packet and could invoke discomfort by displaying the discrepancy between assumed and factual compensation level as well as her privileged position. Potentially, the situation also collided with images of rank and gender and our assumptions of "people doing such monetary estimates" (cf. his position as boss compared to me as an outsider). If we then proceed to Mahid`s response he now narrates two stories. One is on his demand that his staff do marketing equals $ 1000 a month "to cover their salary" and the other is on the professional hotelier that was recruited by UNDP demanding that my key informant's company pay him $ 2000 with UNDP compensating $ 500 of this. This then elegantly puts him back into the owner position and the activities associated with this category. It even makes his manager's total pay-packet relatively moderate.

Rather than looking into the explanations themselves, the accounts can better be seen as designed to convey the speaker's motivations and morality attached to the complex system of a company in this particular region (extract 2, line 9). The stories produce a moral order as well as a setting of practical reasoning and action. They also show the subtlety of much implicit membership categorisation work woven into implicit justifications and evaluations in the course of the telling. There is a parallel in the next sequence.

The delicacy of restoring the key informant attributions

Here is another illustration of active managing dilemmas. In an early stage of the project my key informant faced economic problems. Though I had been explicit that my project was not to give him any economic costs, actual monetary problems is a delicate issue especially in a project about success and cannot easily be made explicit. However, in extract 3 Mahid elegantly does so joined by me (like in lines 1-7

and 9-18) with reference to some delicate monetary incidents like borrowing money or "forgetting"[54] his money back home.

Extract 3:

1 M: Well, sometimes I am fucking, you know, counting pennies. Right

2 A: Well, I know that. I think you have said most of your assets are in (1.0)
3 M: Yeah, cash flow is always a problem. You need so much cash flow, and it generates, and it goes, and it
4 generates, and it goes, you know[
5 A: [and you go to London and you run out of money[

6 M: [and I have to
7 borrow money from you (3.0)
8 A: It was so funny (laughing)
9 M: No, even when leaving [place], I just crossed, we were just about entering the city, I just panicked,
10 sorry, I just panicked[
11 A: [ja
12 M: (3.0) eh, I left all the money at home[
13 A: [ja, you could just have told us[
14 M: [No, I just panicked you
15 know. Oh, fucks, you know. You probably realized I pulled out my wallets and you know
16 A: you did, I was sitting in the car.
17 M: Oh, you know, this is very common thing with me (1.0) I forget money
18 A: yeah, I know from Uganda[
19 M: [ja (4.9) at times I have so much surplus on me, I don't even know (2.0)
20 A: Oh, no problem, just let me know (2.0) laughing
21 M: whenever you need, let me know….I want you to call me
22 A: I will…

(Kenya 2004)

By my rather humorous responses "Oh no problem, just let me know" (line 20) or "I will" (line 22) I accept his recipient-designed descriptions to restore the category as a successful businessman (lines 19 and 21)(cf. the project is on Asian economic success) and I refrain from asking more detailed, problematic questions. I was well aware that the situation had still not been fully restored based on carefully examined observations by comparing institutional contexts and audiences.

In this way extract 3 can be heard as a moral account in which he skilfully invites to restore or re-establish categories and descriptions also paving way for staying with my project. Importantly, this epistemological approach to inconsistencies in data also supports a smoother way of handling field relations and motivated by the differentiation between analytic purposes and private generosity. While the latter is informed by empathy, the former is based on professional skills in research practice, though at times this ethnomethodological indifference can be a painful exercise in practice.

As put by Bergman (2004:34) in his comments on Garfinkel and Sacks (despite the differences): "Their work shows that the construction of social reality can be observed in the communicative processes and situational practices of everyday life; they draw attention to the fact that research must analyse its social objects within the timescale in which life takes place; they demonstrate the enormous gain that can be made for sociology in considering the apparently insignificant details; and they encourage mistrust towards common-sense interpretations and towards the scientific categories that scientists all too gladly use in handling data". Analysing how our discourse attends to requirements of the setting then "has important implications for the way we understand what happens between us in the very talk and how we view the data which are subsequently produced" to cite Woffitt and Widdicombe (2006:34).

[54] The quotation marks display how I myself came to hear his story.

This also relates to coping with images that do not connect across our cultural borders, a moment that often generates frustration and eruptions. The context of talk makes comparison with an external truth irrelevant. This also refers to how the speaker creates a context for the next speaker's talk. If I rather had pursued the inconsistencies in data, I would have created a context for conflict which was more frequent during the early stages of our project. However, the slowness of the ethnographic process invites a building up of a shared stock of collective experiences. Later these help to inform the researcher of local descriptions and inferences initially unfamiliar to the ethnographer. This is an argument for accepting eruptions (though more problematic if they lead to end the project) because when thoroughly analysed they are part of the informing process that slowly takes us into new territory or informing cycles in our field.

I will now illustrate how this feeds into the artful interpretative work of participants.

Membership categorisation as cultural competence

My fieldwork involves visiting factories, offices and board rooms, but also attending lunch- and dinner meetings, spending hours driving in the car, meeting up with colleagues and business partners in old and new networks in offices as well as in bars (Ryen, 2008a and b). The classic advice to talk with research subjects in their settings is motivated by the concern for accuracy as opposed to the talk-in-action perspective's concern for how the local institutional setting frames interaction. In my fieldwork borders may become blurred by the category-associated activities associated with certain settings. On the other hand, experiencing the descriptions available teach us how we as speakers may do and hear descriptions to produce plausible versions. This way "wading in the field" becomes an argument for acquiring cultural competence. By using our analytic skills we will slowly be able to join in the artful methods used for producing a recognizable and orderly social world in that particular culture. Let me illustrate.

Friend – friend

The membership categorisation devise approach makes visible the very production of field relations as fluid and flexible as well as conflicting and complex. Though not always explicitly mentioned such categories may be implied through the activities associated with them.

For quite some time my key informant suffered from a health problem. Apart from offering calm time for more talk, it also made us do more non-business talk. Health (issues) is a description that invokes particular responses which are excellent for constituting care and friendship. After a couple of years it was my time in hospital and Mahid reciprocated my concern:

Extract 4:

"R u ok pls tel me"
Mahid`s text message, January 31, 2007
(my mobile was off the day I was operated)

Extract 5:

"Did u have a comfortable nite u worry me u rest and get wel soon"
(Mahid`s text message, February 4, 2007)

By this time we had reached a point in our relationship where we could also crack jokes or pass on irony to each other. At times our paired friend-friend relational category got challenged. Explicitly trying to mend an eruption I apologised for my anger which generated these responses:

Extract 6:

I am glad. I must kneel down. I told u to listen to the great man.
(Text message from Mahid 26.01.06)

Followed up next day by a new message:

Extract 7:

Have a good day full of fun.
(Text message from Mahid 27.01.06)

Other times Mahid does the mending like last year when he forwarded a "pre-packed" Christmas greeting on my mobile. These are not my favourites and I let him know and asked for a better option. He then forwarded another pre-packed message, this time a Christmas-related joke. I couldn't but smile and did compliment him on the great improvement. "At least," he responded, "I made u laugh".

The action orientation of talk makes us better understand data. However, seeing talk as designed to achieve particular interactional ends means we recognise the actions designed for the particular audience. In cross-cultural contexts we may accept the action orientation of utterances, but still have incomplete or partial understanding of the activities, which can generate breakdowns and misunderstandings. Interestingly, communication via mobiles (cf. extracts 4-7) connects the real and virtual fields by making FTF and CMC[55] communication into more or less everyday activities in the sense that new technology does not necessarily represent a full break with old practices. In their discussion of transnational spaces Robert Stake and Fazal Rizvi (2008) have interestingly explored the use of mobile phones in the Asian communities to uphold communication across long distances (cf. their migration histories). This connects the "here" and "there" and simply provides another option for communication across or within ethnic and cultural groups. Consequently, we adapt to this cultural space (hence in my own culture the lower rate phone cards are available in immigrant shops only).

The social aspect of gender works well to illustrate the complexities or cultural specificities of cross-cultural fieldwork and calls for bringing the wider culture in to get better at local descriptions and inferences; both conveyed explicitly and in more subtle ways.

The man-woman category elegantly illustrates this point by accentuating cultural competence as a facilitator to members` artful interpretative ongoing work.

[55] The meaning of the abbreviations: Face-To-Face communication and Computer-Mediated-Communication.

Man-woman

Any observation of a man and (a somewhat younger) woman may invoke an image of a pair of some kind. However, also such a description may be more complex than initially assumed. Consequently, I will now also introduce local images that in their own ways add to descriptions and to how we negotiate, accomplish and play with variations of a relational pair.

At times, incidents arise that mirror the stressful aspects of cross-cultural projects like here from one of our fieldworks in Kenya (2007). We were standing by the bar in the evening with me somewhat further away by the table. Before giving the African barman his orders Mahid is shouting over to me.

Extract 8:

1. M: What do you want to drink?
2. A: (3.0)
3. M: can you get your bloody head to work (aggressive)
4. A: A Safari and a Sprite, thanks.
   (I turn away, but can hear him talking nicely to the African bar man. Mahid is coming over.)
5. A: What did I hear you say? (with resentment)
6. M: What do you mean? (polite)
7. A: If I ever again hear you address me that way, I will simply leave! Not even once will I hear that again!
   (very determined)
8 M: I don't understand (1.0) what are you talking about (mild, I hear an apology)

He had been rather morose for some time. This had made me walk a few steps ahead of him to the restaurant to avoid the complicated silences with him neither talking nor responding to me. Silences thus become actions, and in this case as "inappropriate silences" (Silverman 1998: 10) signalling that he is not properly listening or does not recognise me as speaker. On the other hand, entering the restaurant clearly ahead of him made it obvious we did not communicate very well supported by the total silence by the table. Hence, while helping myself at the buffet, the African chef who had observed us for two days, asked me in a low voice though with a smile on his face:

Extract 8:

1 Chef: How is the old man doing?
2 Anne: Ah, you know, by time he will mellow down (smiling).
3 Chef: (smiling)
4 Anne: (bringing my plate back to our table)

Telling the story in extract 7 (made anonymous) to some men and women (separately) from the South, they all relate it to the ambiguity of gender and ethnicity calling for invoking a hierarchy in front of an audience, the African barman. Nagel (2003:52) in her discussion of performance and performativity of sexuality and gender roles argues that the unconscious, performative gender roles tend to get noticed only when a rule is violated. This connects well with Sacks on the mundane practices that often pass unnoticed. The black/coloured-white ethnosexual frontier may still remain as a controversial intersection and possibly accentuated when framed by the institutional bar setting. The wider context with the legacy of the British East-African colonial policy including a tripartite racial rank system with Asians squeezed in between the dominating white Europeans and the less privileged black Africans, may in a subtle and uncomfortable way have displayed the meaning of

whiteness (for ranking based on ethnicity in the East-African region linked up with colonial times, see Kristiansen and Ryen 2002).

However, the potential inferences inherent in this observation white woman-brown man may in certain contexts put masculinity at stake. This makes power enter fieldwork and calls for debating if or when to call off the ethnomethodological so-called indifference or moral non-judgement. No doubt such experiences do work well to teach us of cultural specific issues, but in their more extreme variants the power aspect may impact on our fieldwork in ways that may become uncomfortable or even put the researcher at risk. As ethnographers we should indeed explore such experiences or data, but also be free at times to put on the breaks to interfere with the referred ethnomethodological indifference. Just as power-plays and breakdowns can be informative and necessary for the informing process as put by Rabinow, they can also in various ways disadvantage our projects. This marks a difference between the classic ethnomethodologist sitting in his (the old classics are all men) office waiting for the tapes of naturally occurring data compared to the ethnographer her or himself going to the field. Still, not only power issues, but also negotiating them offers great ethnographic data.[56]

However, after Mahid had a few hours of sleep, we were back to stories, jokes and business talk. He claimed he suffered from leg pains. This identification reformulation, another health issue, worked well to bury the hatchet. His new description constituted a new relational pair as sick-healthy calling for the other member's (my) empathy. We both accepted this and refrained from exploring the incident any further.

This illustrates how Sacks` apparatus can help us get at the commonsense activities implied in talk, but also the necessity to bring in the wider culture to describe the active interpretive work members do whether smooth or problematic. The gender-ethnicity intersection invites a number of possible categories for making sense of the observation of us two together. The problem with descriptions addressed to possible audiences is, as we know, that we can be held responsible for both descriptions and for the inferences that can be drawn from them. This is commonsense knowledge, and Mahid and I have over time become explicit on this problem especially when we enter certain institutional settings.

We both know that our mixed "pairing" at times is seen as a more private relationship, a rather frequent stereotype in the region.

At a barbeque once, I received a gracious compliment from an Indian man standing next to us that made Mahid pass on an ironic statement about women. The Indian man elegantly handled his rough talk by citing an Indian poem. He turned out to be the boss of the Indian UN base. Later in this lazy afternoon he sent one of his younger staff over to our table to invite us for an evening drink in the camp. Mahid, our friend the local businessman and I happily accepted this joyful invitation after some rather hectic days together. Before we left for the UN base, I found it appropriate to sort out the gender-relational issue with Mahid when still on our own. As the only white person in our team (we were a mixed team of black, brown and white), my minority status in the region definitely would stand out:

[56] In this particular project we have explicitly agreed to allow breakdowns, but have also agreed on a responsibility for mending them cf. end note 12.

Extract 9:
1 A : Who am I?
2 M : What do you mean?
3 A : Who am I to the Indians? How am I to present myself? A researcher?
4 M : You can say "researcher", but they will never believe that. They will think you are with me.
5 A : What does that mean? (smiling) What should I say?
6 M : You could tell them we are married.

(Congo 2004)

We smilingly agreed this was a viable option, and having potentially been married for many years we could drop the flirting business. So, for the evening we decided to act as a married couple and I thereby joined in with the performance co-constructing the SRP husband-wife. Belonging to a low-contextual language group (the Germanic, Ryen 2002) I am no trained actor so my poor play probably violated the quality of our performance. Back in the car Mahid told me about his plan to help me out well prepared for the anticipated trouble ahead of me, and we both burst into laughter followed up by jokes and more stories. This way both our initial negotiation as well as the performance itself worked to underpin the standard relational pair as man-woman.

Much later I realised I had forgotten to inform the third person in our team about our practical joke. He made no comments, not then or later. I heard the absence of comments from the other members that evening as indicating they saw our story as contextually appropriate, though not necessarily accurate. This way Mahid elegantly invited us into a contextually adequate play, but one that no one mistook for "real". Interestingly, I noted how elegantly the audience played their roles. This represented the first in a line of local performances with Mahid or when alone and has made me improve and recognize other local performances. Slowly they have also become part of my own repertoire when convenient[57]. In humorous and at times in complex and aggravating ways, my experiences from field practice have slowly come to inform me about the local culture well enough to be accepted in as a collaborative player (for other unfamiliar illustrations on "truth" see Ryen, 2008b about a phone call and 2008c about cancelling a journal). Not by interviewing, but by doing being us across different places and settings have I have come to learn how these in different ways have framed our experiences including the different descriptions and inferences made or called for. It is via these activities that Rainbow's referred externality becomes a moving ratio, but they are all collaborative.

Consequences of colour will vary across time, place and audience, but these constellations constitute the images that feed descriptions. Years in East-Africa has as referred above, taught me the experiences associated with the British racial system encountered by Mahid`s age-group in particular. As put by Nader "The relative importance of this ethnic boundary to different ethnic constituencies and audiences illustrates how structure (recognized ethnic categories) and power (whose opinion matters) work together to map ethnicity" (2003:43).

However, gender is a complex label also in the field. At times Mahid has taken on responsibility as the security guard to protect me from other men as in this bar-incident where we stood next to another Asian businessman. The two men are talking together in their own language. Mahid turns around and tells me in a low voice:

[57] The challenge is not to confuse what counts as legitimate performances across cultures. Once after coming back home from fieldwork, I unintentionally came to mix up or rather to forget to switch back to my own culture to our children's worry. This clearly called for an explanation adequate in my own culture, and trust was re-established.

Extract 10

1 M: He asked if you were available.
2 A: Jesus, what did you tell him?
3 M: I told him you're an old girlfriend of mine and that he should leave you alone (smiling).
4 A: Thanks, you bastard (both laughing)

This variation of the man-woman category can work in different ways. Sacks` consistency rule calls for describing us as a unit. Duplicative organisation however, helps us see this "man" and "woman" as belonging to the same unit which means that other members would find it odd if we didn't see it this way. This mundane practice is described in Sacks` rule called the hearer's maxim for duplicative organisation (Silverman 1998:81). When the MCD "family" is duplicatively organised, the co-incumbents also make up a pair of positions with standardised mutual rights and obligations towards each other as in any standard relational pair just as described above (this everyday practice is described in that part of Sacks` apparatus called Collection R as opposed to Collection K which deals with some troubled person e.g. client and an expert of some kind allowed to offer advice). Whatever inferences the "man" and "woman" observation does invoke, depends on the images we carry with us of what rights and obligations each party of such standardised relational pairs has. This is contextually determined so descriptions and inferences may differ in a polygamous region compared to a non non-polygamous region, or in a culture with wives, mistresses, concubines and lovers compared to a culture where extra-marital relations are formally condemned like a strict Christian or Muslim community. A bar represents an institutional setting that itself makes certain descriptions more likely than other descriptions because participants' discourse attends to requirements of the setting like the courtroom, television news interviews, religious service or even survey interviews (Drew and Heritage 1992).

This does not mean that "culture" immediately works to explain such incidences, but rather that the cultural collective images constitute a reservoir of shared images people may choose from to render descriptions that make members make sense of such events and observations. In the sequence Mahid subscribes to his friends` (im)moral schema ("availability") by his "old girlfriend of mine". In this way they themselves constitute another relational pair – old buddy-old buddy. As such his description of me as his "former" girlfriend does give him certain rights towards his friend that this friend obviously will have to respect. Consequently, I am happily left alone. This is another example of Mahid`s elegant use of mundane practices in use which slowly inform me about available images called for in particular settings or contexts. Our joint experiences enlarge our collective pool from which we later can choose from or not in later local contexts.

The thorny way to interpretive artistry: muddling through the cross-cultural field

Again, ethnographers agree that accessing and staying in the field are the two hurdles we all need to pass. After passing the first, they also agree that the second needs constant reworking though how is contested and rather unclear. In the cross-cultural field this is even more delicate.

A wide range of external explanations offers to explain best ways. However, their mundane character weakens their explanatory power. By analysing detailed so-called natural occurring data from the field, I have tried to explore the puzzle of

lasting field relations. This has taken us a long way by displaying talk as designed for targeted recipients and audiences- talk as action.

Whose field- what context?

Still, when researcher and key informant interact across cultures, we cannot take for granted that they share collective experiences and images that inform their talk-in-interaction (Ryen 2007). In a number of instances both members will evidently draw upon descriptions and inferences that are cultural and context specific and that inform the descriptions and the inferences they actively invoke and draw upon without always being connected. In the cross-cultural field communicative eruptions and breakdowns are more likely than in inter-cultural or inter-ethnic research. However, since the researcher is the initiator and has looked up the key informant, (s)he has approached the other's territory or field. Consequently the responsibility to explore is with the ethnographer and to slowly make it into "our" field. Still, with interview studies or evaluation projects across cultures, the time schedule unfortunately does not always allow for adequate time. Because of a lack of cultural knowledge also in the funding agency it is likely that nobody will notice because they all draw on their own collective cultural images in making sense of the reported experiences. This does not necessarily overlap with sense-making in the very field. This is a criticism raised in the discussion of western projects` potential for (or lack of) social change in the South.

The asset of slow time: the road to cultural awareness

The very local context then needs to be supplemented by a wider cultural gaze. Paradoxically, the mundane character makes interviews about the cultural specificities problematic because as members we often cannot account for whatever we are doing despite the everyday character of whatever this is. This is a strong argument for the necessity of fieldwork combined with thorough work on analyses of detailed notes from the very same field. This way researcher and key informant slowly build up a new and shared stock of experiences. These are then fed into the images we bring with us into our artful interpretive activities that are conditioned by whatever we experience as circumstantial real whether it is or not. By time this informing process, though at times a rather painful process for both members, contributes to connecting across the cultural (often blurred) borders (business meeting or just having a beer in the bar? Researcher or old girlfriend?). Then, eventually, we can collaboratively share the available taken-for-granted descriptions in that very particular culture. It is this that widens the possibilities for joint creative and artful interpretations. If (individually) artful, but not shared, our roads may follow each other, but never cross because our images do not connect. The slowness of this work - typical of ethnography that takes time and hard work - is what conditions the artfulness of the interactional interpretative play that we by time can engage in. Cultural awareness then, is closely associated with skills both analytic (typical of the researcher) and mundane (practice done by any member in everyday life).

Conclusions

Not until we reach this cultural awareness, do we successfully become another member that can be invited as a more fully competent member into the local performances across the varied audiences and institutional frames that our fieldwork invites us into. We will then enlarge our performative competences and shared repertoires that invite us to a prolonged collaboration between field members.

Eruptions will still occur. But, now we are better equipped by being able to employ descriptions and inferences that have become shared and collective, or to actively refrain from them. Important, as the initial interest for the strange outsider slowly evaporates, it is exchanged for another: the competent participant. The "hardworking slowness" thus has invited us to become more competent players. We are now neither fully inside nor outside, but the public informing process has invited us to join the play. This invites us to remove the major obstacles to prolonged fieldwork and caters for staying in the field across contexts - from offices to bars, with friends or business partners, across ethnicities and gender. This way our analytic approach is a viable road to credible research.

This complex "wading" invites the cultural awareness needed for widening collaborative members` shared repertoire of experiences and descriptions across cultures. Just as with the research interview, our fieldwork feeds on the intersubjective process which constitutes the very ethnographic cross-cultural work itself though never quite the same, ever on the move. Connecting on this track, the collaborative nature may take the researcher and key informant further. As put by Baker (2004a:175): "The artful production of plausible versions using recognizable categorization devices is a profoundly important form of cultural competence. What we hear and attend to…are members` methods for putting together a world that is recognizably familiar, orderly and moral." And importantly, "we are interested in how aspects of the accounts are put together irrespective of their truth-value" (Baker 2004b:785).

By making the hows of interaction and the whats of the cross-cultural research practice our topic, we now hopefully have a powerful and credible illustration of doing being or staying in the ethnographic cross-cultural field.

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Author-Supplied Abstracts & Keywords

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Triangulation and Dealing with the Realness of Qualitative Research

This paper provides a sociologists' reflection of a sociologist on qualitative field research. Reflections will include some methodological and epistemological considerations that are connected with field work, while building the realness of the description and conclusions, i.e. constructing the quality of qualitative research. The intellectual process of doing research will be characterized by analysis of: description of investigated reality (tales of the field); analytical process; usage of commonsense research procedures (so called triangulation procedures), which are used in the field by the researcher and during analysis or writing a research report to adequately "re – present" researched reality.
The three above mentioned stages of representation of reality are interwoven to create one complex intellectual process, which is called "field research". The quality of qualitative research is the intellectual process where some procedures are used to create the accountability of research conclusions.

Keywords: Triangulation; Field research; Qualitative methods; Sociology; Ethnography; Writing of ethnography; Ethnographic description; Definition of reality; Realness; Tribal rules; Generalized other

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Who Allowed You to Observe? A Reflexive Overt Organizational Ethnography

Observing people working within organizational contexts through time creates epistemological issues, more so when doing it overtly, with top management's official agreement. Power relations as well as hierarchical structures strongly influence the way people view the observer and interact with her in organizations. Those interactions also partly depend on his personal background – sex, age, professional position and so on. Following a reflexive approach, my objective is here to better grasp how top management's agreement to the ethnographer's entry on the field may influence both the way workers from differing hierarchical levels behave with her (and thus affect her observing conditions) and how he may analyse his ethnographic notes to develop scientific sociological results.

Keywords: Ethnography; Reflexivity; Organization; Work; Epistemology

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Narratives in Illness: A Methodological Note

As a result of the general growth in the interest in narratives different conception of what a story is and how to analyze has emerged. One especially interesting and methodological relevant difference is between the conception of narratives as textual objects and narratives as part of a storytelling event. The paper discusses the theoretical differences between these two analytical approaches to narratives. An example from my own research on Alzheimer's patients telling stories illustrate the possibilities of using a performative and micro ethnographic approach to the study of storytelling in order to understand the functions of narratives – especially in relation to identity work. If stories not only are thought of as representations of events it becomes possible to view stories and story telling as social action: social states are both established, negotiated and changed through stories. This is especially important in the field of health and illness where diseases almost always are embedded in conversations and the telling of why and how symptoms were discovered or traumas received. For many patients and persons with especially communicative disabilities story telling is a challenge, but also an opportunity to actually master, maintain and often transform their identities.

Keywords: Narrative; Methodology; Identity; Performance; Ethnography; Video analysis

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Vision and Performance. The Sociolinguistic Analysis of Genres and Its Application to Focussed Ethnographic Data

The use of audiovisual recording devices is changing the practice of qualitative research. Extensive corpus of data can be generated in (short-term) focussed fieldwork. Nevertheless, methods to analyse video data are still in an experimental stage. This article explores the benefits and limitations of applying sociolinguistic genre analysis to audio-visual data. This is illustrated with a case study, based on the videotaped »deep-trance vision« of a New Religious Movement's spiritual leader, which is one the most famous contemporary religious visionaries in Germany. The analysis aims to reconstruct the construction of this religious experience of transcendence from the perspective of its followers. We will examine three different levels of communication (a) the inner context, exploring the textual, gestural, mimical and prosodic aspects, (b) the intermediate level where the focus lies on the setting and decorum, and finally (c) the outer context, focussing on the social embedding of this form of »transcendent« communication and its filmic presentation. The article closes with a reflection on the need to combine hermeneutic analysis of case studies based on textual analysis with ethnographic field data and observation to contextualise its interpretation.

Keywords: Video-data; Genre-analysis; Ethnography; Religious experiences; New religious movements

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Wading the Field with My Key Informant: Exploring Field Relations

Entering and staying on in the field or rather avoiding being kicked out are the two classic ethnographic challenges. The rather positivist nature of textbook guidance on dos and don'ts in fieldwork in general and in delicate issues in particular (for researchers` dilemmas in the field see Ryen, 2002), tend to recommend a gentle, middle-class (rather female) interactional style. This gaze suffers from being both researcher-focused (cf.Fine, 1994 on "Othering") and based on problematic pre-fixed identities nailing us to the role pair as researcher and key informant. As the introductory extract illustrates, it takes patience also to have an ethnographer "hanging around".
This article deals with the credibility of qualitative research when accounting for or exploring how we do staying in the cross-cultural field and it asks how can we credibly explore the stamina that takes us further?
If we accept fieldwork as social interaction, we need to bring the social (or the "inter") of it into the exploration of our puzzle. Membership categorisation device (MCD) offers to take us closer to understanding and piecing together our puzzle, but to better get at the events taking place in field interaction there is a need also to introduce the wider cultural context and the images available (or not) to members. In this way I recognise the ethnomethodological differentiation between topic and resource, but argue that when understandings and images are not necessarily culturally shared and collective, we also need to make problematic how members deal with the unavailability of shared images.
In the conclusion I argue that the artful side of the local interpretive work in the field is closely entangled with whatever meanings or images are available for construction (in line with Gubrium and Holstein, 1997:121). In cross-cultural contexts more than in others, this is particularly delicate because in such contexts images and experiences often do not connect and may lead to complications or even breakdown in communication (Ryen 2002). Mending or repair thus becomes another crucial phenomenon, itself complex, in the evolving field relations. The analysis thus pinpoints the artistry of members` local collaborative efforts accentuated when constrained by images or descriptions that do not connect across cultures. This makes stamina a joint effort, though itself an intricate, emergent phenomenon.
Next I will briefly introduce a couple of classic works on working with key informants followed by a brief presentation of the analytic approaches to be applied to my data from East-Africa. Before concluding, I will comment on "wading the field" as reflected in the close exploration of the cross-cultural extracts.

Keywords: Key informant; Qualitative research; Membership categorisation device, credibility; Cross-cultural research; East-Africa.

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