The Policy of Fieldwork: Data Production in Anthropology and Qualitative Approaches

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II

The policy of fieldwork. Data production in socio-anthropology

Introduction

As mentioned in the introduction, sociology, anthropology and history share a common epistemology. Interpretative methods, hypotheses, heuristic choices, paradigms and processes of object construction are usually similar or transversal. However, these disciplines do not necessarily produce data in the same way. Though close to each other, each seems nonetheless to have its own favourite form of empirical enquiry. Archives for the historian, enquiry through questionnaires for the sociologist, "fieldwork" for the anthropologist: at first glance, these three modes of data production appear to be linked, respectively, to each of these related social sciences[1].

Granted, this is merely a question of the dominant characteristics of each discipline. And borrowing from the neighbour is quite common. Fieldwork, in particular, has acquired a prominent role in sociology. In fact, there is no fundamental difference in the way "qualitative" sociology (as it is sometimes called) [2] and anthropology produce data.

Some clarification is required here, for the expression “qualitative methods” which is quite common, especially in the United States, has a number of disadvantages, in particular that of implying that qualitative methods are not concerned with scales or figures. This is incorrect (see below what I called the “procedures of intensive survey”). Conversely, to call sociology through questionnaires “quantitative sociology” incurs the risk of implying wrongly that it involves neither value judgments nor non quantified interpretations and is based solely on statistical data. Quantitative and qualitative methods both belong, moreover, and above all, to a series of shared, fundamental intellectual operations such as the elaboration of research problematic, the use of scholarly references, and theoretical and interpretative arguments[3].

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[1] A preliminary version of this paper was published in *Enquête*, 1, 1995: 71-112.

[2] *See* Kirk and Miller, 1986 ; Schwartz and Jacob, 1979.

[3] Oppositions between the qualitative and the quantitative have sometimes led to epistemological confusions: “*The division between qualitative quantitative methods became associated - unfortunately and inappropriately - with the rival positions of positivism and interpretivism*” (Gerring and Thomas, 2005, 1). For example, Burawoy (2003, 435-438) opposes the model of positive science (represented by enquiry through questionnaire) and the model of reflexive science (represented by the case study). If, on the contrary, one accepts the epistemological unity of the social sciences, regardless of their methods (the position defended by Jean-Claude Passeron and by Jean-Michel Berthelot, which is also adopted here), positivism and hermeneutism, these warring brothers, can be regarded as scientific ideologies (or biases), functioning independently of the methods of data production, even if positivism is more a threat to quantitativists, and hermeneutism to qualitativists. But this is only one tendency; hence we have the famous examples of anthropologists with a strong positivist inclination, like Claude Lévi-Strauss, George Murdock or Alfred Radcliffe Brown.

But, nevertheless, quantitative methods and qualitative methods follow different paths produce different types of data, and draw different paintings of the reference reality. The obvious contrast between enquiry through questionnaires and enquiry through fieldwork needs to be taken into consideration [4]. They are two poles or two ideal-types (fortunately, there exist intermediary or combined forms, despite fanatics from both sides) which differ as much in their respective modes of data production and in the characteristics of their data as in their approach to the problem of representativeness. Surveys carried out through questionnaires gather limited and codable information on the basis of statistically representative samples, in an artificial context of interrogation in which answers are recorded through the mediation of paid enquirers. On the other hand, anthropological fieldwork has the ambition of being as close as possible to the natural context of the subjects involved - everyday life, conversation - in a situation of prolonged interrelations between the researcher in person and the local populations, in order to produce *in situ*, contextualized, transversal knowledge, with the aim of giving an account of the "actor's point of view", of ordinary, everyday representations, and of their indigenous significance. Statistical enquiry is mostly, fieldwork is more or less intensive and each possesses inverse advantages and disadvantages[5].

Field enquiry	*Enquiry through questionnaires*
broad, multidimensional, non codable information	circumscribed, univocal, codable information
artisanal processing	statistical processing
natural situations or situations close to natural situations (conversation)	artificial situations (interrogation)
researcher in person	paid investigators
insertion in the milieu	sporadic interaction
information on contextualized processes and logics (without statistical representativeness)	information on decontextualized variables (statistical representativeness)
intensive, duration, returns	extensive, brevity, linearity
research trails, iteration, improvisation	verifiable precise hypotheses

Diagram 2. Field enquiry and enquiry through questionnaires

Admittedly, the ideal enquiry should combine these two approaches, and many voices are now raised in support of multi-methods research[6]. But the fact that this involves very

[4] It should be recalled that qualitative methods, which have in recent years become the object of various publications in French under this name (see Poupart and al, 1997; Mucchielli, 2004; Paillé and Mucchielli, 2005; Mulched, 2006), are not at all limited to socio-anthropological field enquiry, which is the focus of the present work (besides, the term "qualitative methods" is mainly used by practioners of other disciplines). They may be found in a variety of forms (some of which are more "applied") in political and administrative science, criminology, social psychology, the sciences of education, of communication, development, or social work. These forms are sometimes quite different from socio-anthropological fieldwork; they are sometimes experimental, are sometimes formalized; they range from group psychology to the comparative political analysis, from content analysis to the Boolean truth tables.

[5] Strauss (1987: 2) therefore notes that the force of the qualitative enquiry resides in the taking into account of contexts, whereas the force of the quantitative enquiry derives from the fact that it is multivariate and cross comparative on a large scale. For Katz (1983: 137): "*Statistical evidence of representativeness depends on restricting a depiction of qualitative richness in the experience of people studied. A similar practical trade-off confronts those who do inductive research but it forces the opposite choice* ".

[6] See Tashakkori and Teddlie, 2003; Ridde, 2008; and the review *Mixed Methods Research*.

complex and distinct competences, as well as the dynamics of university careers, makes it particularly difficult for a researcher to master these two types of savoir-faire[7].

Moreover, the multi-disciplinary teams that the rhetoric of research policies has been advocating, and not without cause, over the last for 30 years or more, are confronted with the awesome problem of the piloting of the machine (who holds scientific and institutional power?) and of elaborating the problematic (how can this be really produced jointly?). This explains why most experiments of this type have more to do with the condo (each one manages his own apartment) than with common ownership of property. At any rate, our discussion here will focus on the qualitative aspect, and on its emblematic form, socio-anthropological (often called "ethnographic") fieldwork.

While quantitative methods are the object of numerous courses and manuals, for those who do not practice fieldwork this methodology seems somewhat nebulous. Yet those who practice field enquiry do not seem to view dissipating this vagueness as an emergency. Although reference to fieldwork is becoming popular, what this usually entails are stories of personal experience or narratives of a reflective nature. Real epistemological and methodological analyses on the production of field data are often lacking. Due to this opaque or mysterious nature of data production in the field, anthropology, seen from outside, remains the most misunderstood, the most fascinating as well as the most disputed of all social sciences. Anthropology is often credited with empathy, the anthropologist lauded for his experience. On the other hand, both are often condemned for their impressionism and their subjectivity. Certain irritating and sometimes grotesque aspects of the myth surrounding fieldwork make matters worse. One example is that of the anthropologist posing as a hero by dramatizing the difficulties encountered in the field[8]. Yet field enquiry is only one of several modes of data production employed in the social sciences. Like all the others, fieldwork has its advantages and disadvantages. It possesses specific modes of methodological vigilance, and stands to gain from an explanation of the "principles" on which it works. Dissipating the "vagueness" surrounding fieldwork is definitely a problem that needs to be addressed.

Each method has a particular type of rigour, a specific means of rendering data valid or plausible. But the seriousness of field data cannot be converted into statistics, as opposed to the seriousness of data obtained through enquiry via questionnaires, which can, in part, be converted statistically. It is clear that statistical validity is not the forte of the data produced by fieldwork, which cannot be measured in quantitative terms. Nevertheless, the practice of anthropology is not merely a matter of "intuition", it involves and requires training and competence. The problem is that this competence is a matter of savoir-faire, and that the requisite training entails a process of hands-on learning in the field. In other words, field enquiry cannot be learnt in a textbook. There are no set of formal procedures to be observed as is the case, in part, of so-called "quantitative" enquiry. This is why ethnographic manuals are so unsatisfactory [9] (and the same applies to textbooks on non directive interviews).The

[7] Schatz (2007) underlines these difficulties.

[8] Fieldwork thus becomes a « *myth* " (see Schwartz, 1993: 270-271), "*an entitlement to glory*" (see Schwartz and Jacobs, 1979: 125). For a criticism of two examples of this mythification, see Olivier de Sardan, 1988 : 527-540. One can subscribe to this remark: "*The subjects of ethnographies, it should not be forgotten, are always more interesting than their authors*" (Smith, quoted in Sanjek, 1991: 610). *See* chapter V, below.

[9] First generation manuals were essentially "question inventories", in keeping with the classical monographic (and positivist) approach in the context of the discovery of "primitive peoples": it was a matter of being exhaustive in the systematic description of a given (exotic) culture. This was the case since 1874 with the famous and regularly updated *Notes and Queries*, a handbook which has recently been abandoned. The tardiness of field-work in France is quite obvious: the first manual dates back to 1947, based on notes taken during Mauss' course, though Mauss was never formally involved in fieldwork (Mauss, 1947). The manuals of Griaule (1957)

fact is that fieldwork is above all a matter of know-how, and makes use of intuition, improvisation and tinkering (bricolage)[10]. The "initiatory" character of fieldwork, often mentioned sarcastically by those commenting on the anthropological tradition, is not merely a myth or a ritual. It is also, above all, a matter of apprenticeship, since the apprentice learns mainly through practice. The experience of carrying out enquiries based on a ready-made questionnaire illustrates the extent to which enquirees are inhibited by an excessively narrow or directive framework. Confrontation with the countless misunderstandings between enquirer and enquiree underlines the *qui pro quos* that pepper this type of research situation. It is only after he has mastered the local codes of civility and decorum that the field researcher can, at long last, feel comfortable during a chat or impromptu conversation, which is often the most productive as far as gathering information is concerned. The researcher in the field begins by improvising awkwardly, until he becomes, little by little, capable of improvising competently. One needs to have wasted time, an awful lot of time, in the field, in order to understand that these slack times were necessary[11]. "*Learning to be a field researcher poses the same problems as learning to live in society*" (Hughes, 1996 : 279).

The paradox entailed in the following lines arises from the attempt to provide a written account of a combination of practically acquired "tricks of the trade" and the concern with scientific rigour, which can only be learnt through practice in the field, without the aid of a standard reference situation[12]. Can we take the risk of venturing into this intermediary zone between epistemology (whose enunciations, however apt, can hardly result in the acquisition of competence : what practical use can one make of Sperber's interesting texts ? [13]) and methodology (against which ethnographic practice seems to rebel : efforts at proposing "a methodological kit" soon become ridiculous) ? Abstract theoretical reflection and a cookbook

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and Maget (1962), the second being by far superior to the first, are in conformity with this category of question inventories. They are currently of little use.

For over forty years, second generation manuals in the United States have shied away from monographs, abandoned inventories, been suspicious of recipes. Their primary objective is to serve as a support for the acquisition of competence: one might mention among many others Spradley and McCurdy, 1972; Pelto and Pelto, 1978; Agar, 1986; Spradley, 1980; Silverman, 1985, as well the entire collection *Qualitative Research Methods* edited by John van Maanen ; see also the journal *Qualitative Sociology*. In France one may find a few dated efforts which are not devoid of interest, but which remain unfinished and heterogeneous, hesitating between the description of specialized enquiry techniques, useful advice, desiderata, and the presentation of areas of research (Cresswell and Godelier, 1976). Mention might, however, be made of Beaud and Weber's recent book (1998), which, in France, is the forerunner of second generation manuals, and which remains the most successful (see also Copans, 1998). As for those manuals whose aim is to make a systematic presentation of all the forms of data production in the social sciences (see Grawitz,1993 ; Quivy and Van Campenhoudt, 2006), they are often quite cursory as far as qualitative methods are concerned, and prioritize not only quantitative methods but also frames of deductive thought (hypotheses to be infirmed or confirmed). Finally, several recent francophone publications set out to reflect on the practices of fieldwork, their methodological constraints as well as various theoretical or epistemological problems they pose (see, among others, Ghasarian 2002 ; Cefaï, 2003 ; Leservoisier, 2005 ; Paillé, 2006), without forgetting the reflexions of Schwartz (1993) who developed epistemological and methodological orientations quite similar to our own, and at the same period

[10]The term « bricolage » (tinkering) is often used to qualify the methodology of anthropology and qualitative methodologies in general (Denzin and Lincoln, 1994: 2 ; see also Weinstein and Weinstein, 1991, who considers Simmel to be the prototype of the tinkering sociologist). Terms like "craft" relating to artisans are also used (*see* Epstein, 1978).

[11] One might consider field enquiry as a "*natural analysis*" (Schwartzman, cited by Strauss, 1987, 3), in the same way that one speaks of a "natural language" or again in the way in which social sciences are said to operate in the register of "*natural reasoning*" (Passeron, 1991). In all of these expressions "natural" is by no means opposed to "cultural" (these are obviously social constructs); its inverse is "artificial" (created by the researcher), and what if refers to are everyday interactions, ordinary practice and common sense.

[12]"*Conceivably, fieldwork situations are as numerous as societies, and as ethnologists*" (Izard, 2000: 471).

[13]Sperber, 1982.

are worlds apart. My aim is not to fill this vacuum, but to propose a few points of reference. This requires an analytical overview of the primary modes of data production peculiar to fieldwork. The perspective will then be widened in order to outline a "policy of fieldwork ", in its hectic quest for plausibility, in its efforts to unearth - despite the multiple “biases” which seep into enquiry, and through their management - some of the practical conditions of this anthropological validity, of this methodological requirement, of this " qualitative rigour ", to which we aspire.

The six types of data production in fieldwork

Field enquiry, anthropological enquiry or socio-anthropological enquiry rely, roughly, on the combination of five and sometimes six major forms of data production [14]:

(1) The more or less prolonged insertion of the enquirer in the social milieu of the enquirees (often called “participant observation”), forming the general framework of the enquiry.

In this context of participant observation, researchers usually produce fours sets of specific data:

(2) Interviews (discursive interactions deliberately provoked by the researcher)

(3) Observations (seeing and describing clearly determined social sequences)

(4) Intensive micro survey procedures (recourse to devices constructed with the aid of systematic or semi-quantitative enquiries)

(5) Written sources

Finally, although these are problematic, raise very specific problems, and are rarely used, there is a final type of data that must be mentioned:

(6) Audio-visual data

[14] Outlines of a « history » of anthropological fieldwork (and of the evolution of methodological and epistemological thought on this subject) may be found in Jongmans and Gutkind, 1967 ; Stocking, 1983 ; van Maanen, 1988 ; Sanjek, 1990 ; Cefaï, 2003.

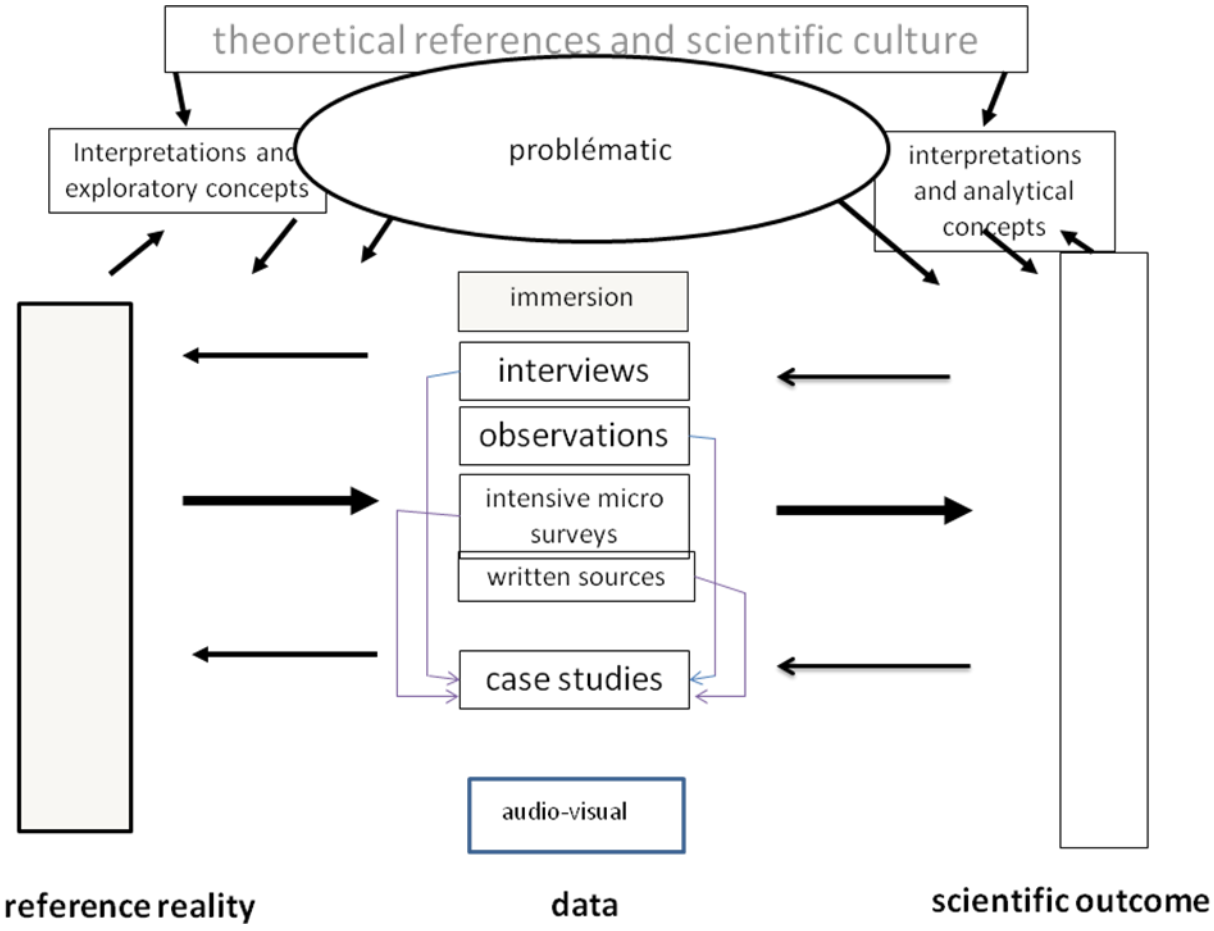

Diagram 3. Data production

1) Insertion in the milieu (participant observation)

Whether this often contested term "participant observation" is suitable or not is of little importance [15]. Its connotation is quite clear. Thanks to a prolonged stay among the people he intends to interview (and by learning the local language if he does not understand it), the anthropologist enters into physical contact with the reality he wants to study. He is thus able to observe, if not from the " inside ", in the strict sense of the word, at least at close quarters, those who experience this reality, and to engage in permanent interaction with them.

The forms of insertion in the milieu are quite varied (these will be discussed in the chapter V). Hence, it is possible to distinguish, following Gold (1958) and Junker (1960), "the complete observer", "the observer as participant", "the participant as observer" and the "complete participant" [16]. But, regardless of the role which he adopts, in a more or less deliberate way[17], the researcher will be in direct contact with the group and/or the processes which he is studying, admittedly in keeping with very variable involvments [18].

This sociological or ethnographic "immersion" constitutes simultaneously the context of data production and a very particular mode of data production. In fact, the information and knowledge acquired can, on the one hand, be systematically recorded by the researcher, who takes advantage of his presence on the spot to interview, observe, note, count… On the other hand, knowledge and information may remain abstract or latent, being, as it were, "incorporated" in the researcher's presence, through his memory, perceptions, affects, unconscious. If observations and interactions are produced and recorded in a more or less deliberate way and systematic manner, they are transformed into data, compiled and organized in a corpus. If not, they play a nonetheless important role, related to immersion

Data and corpus

A great deal of the of field enquirer's time is devoted to *interviews*, which remain the researcher's favourite mode of information production. These interviews are transformed into data through note taking or transcription, taking the very concrete shape of notebooks (and, currently, of texts recorded on a computer). A corpus of discursive data is thus elaborated.

Often, it is not quite as easy to pin down data derived from *observation*. Sometimes they fit directly into written descriptions of real-life situations, sometimes note taking is differed. Sometimes, they focus on particular social sequences, sometimes they are, in other contexts, diffuse, sporadic, indeterminate. But this type of data is also, in one way or another, ultimately transformed into written corpuses (notebooks or computer files).

Counting, measurements, diagrams, plans, intensive surveys, inventories and various other connected procedures, provide for another type of corpus, which we may term *intensive micro survey*".

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[15] It appears, quite significantly, that this term, bearing a strong anthropological connotation, was coined in 1924 by a sociologist, Lindeman, associated with the Chicago School (Kirk and Miller, 1986: 76). Other, more or less equivalent terms, including "insertion" which we have resorted to here, " immersion" (to which we ascribe a particular meaning, see below), or " involvement" (which we use in chapter V to discuss the forms of participation/adhesion linking the researcher to the social group being studied), are also used.

[16] *See.* Atkinson and Hemmersely, 1994: 248 ; Adler and Adler, 1984.

[17] In fact, these roles are often the result of circumstances, of the constraints of the enquiry, or of the habits of the researcher rather than that of a deliberate choice, contrary to frequent affirmations.

[18] "*The role of participant observer may be either formal or informal, concealed or revealed ; the observer may spend a great deal or very little time in the research situation ; the participant observer role may be an integral part of the social structure or largely peripheral to it*" (Schwartz and Schwartz, 1955: 343).

Written documents derived from fieldwork and *audio-visual materials* produced or collected by the researcher make up the last two types of corpuses.

These five types of corpus (discursive, observational, intensive micro survey, documentary and audio-visual) will be examined below in greater detail.

The common characteristic of all of the data thus produced is that they arise from interactions between the researcher and the social milieus he studies, and that they take the objectified shape of written or numerically organized traces which can be analyzed and stripped ex post. Each corpus is formed on the basis of procedures which combines improvisation, on the one hand, with coherence and systematic methodology, on the other. Improvisation is inherent in fieldwork.

But elaborating a corpus entails an attempt at organization of data production and a process of reflection on their choice. Consequently, it is usually conscious about why one is interviewing such and such an interlocutor and on such and such a subject, and one usually knows where to classify the interview. In the same way, the observation of a given ritual or meeting is generally part of a rational selection.

It is quite obvious that data, as defined here, does not refer to "fragments of reality" gathered and conserved in their original state by the researcher (a positivist illusion). They are not mere figments of the researcher's imagination or sensibility (a subjectivist illusion). Data represent the transformation into objectified traces, of "fragments of reality", as perceived and selected by the researcher[19].

But their intelligibility supposes the existence of a "ready-made" conceptual language of description. Passeron underlines this through a reference to Bachelard: the "*epistemological vector*" goes from the rational to the real and not vice versa [20]. Without preliminary questions, without intellectually pre-programmed curiosity, without an initial problematic, the researcher cannot produce significant data. Pure and naive observation is an illusion, and listening has always an intention. It is a well known fact that the observations a researcher makes are structured by the object of his research, by his language, his problematic, his training and his personality. Notwithstanding, the "empirical aims" of the anthropologist should not be underestimated. The researcher's desire for knowledge and his research training are capable of overriding, at least partially, his prejudices and feelings (otherwise empirical social sciences would be completely impossible)[21]. Owing to observation, an initial problematic may be modified, displaced, or widened. Fieldwork is not simply the colouring of a ready-made drawing. It means subjecting preconceived curiosity to the test of reality. The competence of the field researcher consists in his ability to observe the unexpected (although the overriding tendency is usually the discovery of the expected) and in his ability to produce data which impose a modification of his own hypotheses. Field enquiry must attempt to contradict the bambara proverb from Mali according to which: "*A stranger sees only what he already knows" [22]*

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[19] Goffman uses the term "strip" to designate the "*fragments of reality*" which interest the analyst (Goffman, 1991). Jean-Michel Berthelot differentiates « events » (what really happens), i.e. what we call here "fragments of reality", from "data" and "facts" as used in the researcher's enunciations (stabilized objects of analysis and explanation): Berthelot, 2001: 490-491. Gérard Lenclud, in a parallel with the historical document, groups data under the term « ethnographic documents »: « *the ethnographic document is reality transformed into sign by ethnologic heuristics* » (Lenclud, 2000: 475).

[20] Passeron, 1994: 73-74.

[21] "*If there are indeed problems in ethnographic description, they will not be solved by less detailed fieldwork and writing*" (Parkin, 1990: 182).

[22] Quoted in Fassin, 1990 : 97

Immersion

But the everyday life (be it of a social, professional, familial, associative or religious nature) in which the researcher participates, in one way or another, is not only a site from which he derives sequences in order to produce corpuses and fill notebooks.

The researcher is in fact continuously engaged in several interactions. Far from being a mere eyewitness, he is continuously engaged in simple or complex verbal and non-verbal social relationships: conversations, chats, games, etiquette, solicitations, etc. The anthropologist evolves in the realm of ordinary communication, "*he adopts the forms of ordinary dialogue*" (Althabe, 1990: 126), he encounters local actors in everyday situations, in the world of their "*natural attitude*", as noted by Schutz (1987). But many of the everyday conversations and acts in which the anthropologist participates concern directly or indirectly his professional curiosity, i.e. his research theme.

Hence, many local discursive interactions in which the researcher is barely or not all engaged, i.e. ambient conversations, take place in his presence. The researcher is a voyeur as well as an eavesdropper. The dialogues people have among themselves are as valuable as those that he has with them[23], and the everyday behaviours of local actors can be as relevant as the rituals and spectacles that are blatantly obvious.

These forms of insertion remain, in a sense, "close to the corpus", to the extent that the researcher therefore endeavours, whenever useful, to transform ex post relevant interactions and observations into data, i.e. to organize their conservation, description and trace in the field notebook, whether or not these interactions are significantly dependent on the role assigned to the anthropologist in the local arena.

Consequently this notebook, to which the researcher might become obsessively attached, and which is sometimes invested with a mythical character that it hardly deserves, has nothing to do with the personal diary, or the explorer's notebook. It is simply a basic tool of the trade. This is where participant observation is transformed into data to be treated at a later date. As reflected in the title of Sanjek's work, "*the making of anthropology*", field notes are where anthropology is made[24].

The field researcher also observes and interacts without paying special attention, without having the impression of working, and therefore without taking notes, during or after his stay in the field. Luckily, he does not always have the impression of being on an official assignment. He eats, chats, jokes, flirts, looks around, listens, likes, dislikes. In the process of living, he observes, despite himself, so to speak, and these observations are there, present, unwittingly "engraved" in his subconscious, his subjective mind, in his "I", or whatever we wish to call it. These observations are not transformed into a corpus and are not written down in the field notebook. Nevertheless, they play an indirect but important role in "familiarizing" the anthropologist with the local culture, in enabling him to effortlessly decode other people's actions and reactions. In other words, his interpretation of a given situation becomes almost a reflex. Many of the daily interactions in which the researcher is engaged are unrelated to the enquiry and are not consigned in the field notebook, and are therefore not transformed into

[23] Audrey Richards addressed this subject as "*speech-in-action*" in 1939 (Sanjek, 1990: 212). The increasingly massive recourse to interviews as well as certain exhortations in favour of "dialogic anthropology" which priorizes verbal interaction between the researcher and the populations (Fabian, 1983; Clifford and Marcus, 1986) overshadow the nonetheless fundamental dimension of participant observation.

data. This does not make them insignificant, however. Good neighbourly relationships, the good spirits of nightly chats, jokes exchanged with the pretty neighbour, a round at the bar, or the festivities surrounding the baptism of the lodger's child, are activities that take place outside of working hours. But this is how one learns the codes of polite behaviour (and this understanding of proper etiquette will impact indirectly and unconsciously but nonetheless effectively on the way in which his interviews are conducted); this is how one gets acquainted with the runnings of daily life, how one learns what the spontaneous topics of conversation in the village or at the hospital are (and this will come to bear indirectly and unconsciously but nonetheless effectively on the way in which data relating to the enquiry are interpreted). This is what Erving Goffman underlines in his own terms: "*It's deep familiarity that is the rationale – that, plus getting material on a tissue of events – that provides the justification of such an apparently 'loose' thing as fieldwork*" (Goffman, 1989: 130).

It is not impossible to see the researcher's "brains" as a "black box" and forget about the way in which it actually functions. Yet, all that he observes, hears and sees in the process of fieldwork and in the course of interpersonal relationships, will "go into" this black box, will impact the conceptualizing, analyzing, intuiting, interpreting machine, and will therefore, in part, later "come out" of the said black box and will partially structure interpretations throughout the research process, either during fieldwork or during the exploitation of the corpus, at the writing up stage. This is the main difference - especially visible in descriptive works - between a field worker who calls on lived experiences (through immersion) and an armchair researcher working on the basis of data collected by others. This mastery of the meaning system of the group under enquiry is for the most part unconsciously acquired, in exactly the same way as a person learning a language through practice[25].

2) Interviews

Data production by the researcher, based on the indigenous discourses he has himself solicited, remains a central element of field research. Firstly, because participant observation is an insufficient means of gaining access to a range of required information: it then becomes necessary to call on the knowledge or memory of local actors. Secondly, the representations of local actors are indispensable elements for social understanding. In a sense, the paramount ambition of anthropology is to provide the actor's point of view[26]. The interview remains the preferred and in general the most economical mode of data production, one that is capable of producing discursive data opening access to indigenous, local representations (sometimes described as "emic", see chapter III). Notes obtained from the transcription of interviews are the mainstay of anthropological data.

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26 Sanjek, 1990.

[27] On this subject, Sluka and Robben (2007: 8) make the following comment on the work by Keesing and Strathern (1998): "*Keesing and Strathern draw attention to the gap between the data described in field notes and the lived experiences, sounds, smells, and scenes that cannot be captured in writing but are sedimented in the unconscious. Their use of the term 'unconscious' is somewhat misleading because it suggests that the fieldworker is unaware of these impressions, but Keesing and Strathern are right in arguing that the anthropologist draws upon an unpronounced understanding that is larger than field notes and gives the ethnography a richness which could not have been obtained any other way than through an extended immersion in the field*".

[26] Anthropology is often characterized as "*actor oriented*" (see Long and Long, 1992: 9). It is supposed to put into practice the comprehensive sociology invoked by Weber, who, paradoxically, provides no empirical tools to this end. One might call to mind Malinowski's famous introduction to the "The Argonautes...": "*the final objective of ethnography is to capture the native's point of view, his life relations, to understand the vision he has of his own world*" (Malinowski, 1963: 81).

Countering a common assertion, I do not believe in the existence of interview "techniques". This does not pre-empt the existence of a certain "savoir-faire"[27]. To be more precise, it is possible to evoke an " interview policy ", whose main lines will be now described.

Consultation and experience

Interviews usually fluctuate between two poles. Let's call 'them consultation and personal experience. The so-called "informant" is therefore alternately a *consultant* and a *narrator*, usually both.

1. The interview usually concern social or cultural references on which the interviewee is "consulted". The former, called upon to give his opinion on one subject or another, is therefore supposed to reflect, at least in part, a common knowledge, shared, at least in part, with other local actors, or possibly with all of the given social group. What does he know on such or such a topic? What does he think about it? His "competence" on the local society or his knowledge of one of its segments is solicited. The " competence " of the informant does not imply that the local society necessarily sees him as an "expert" on society or within his professional group, nor does this mean that we should accept the principle of a "key informant ", a great erudite or renowned initiate on whom the researcher might lean in order to produce a narrative that he may then present as being "collective". The notion of "consultant" refers, in this case, to a specific discursive register concerning interview situations rather than the specific status of the interviewee. In the same vein, the notion of "competence", as used here, simply refers to the capacity of the interviewee to have something to say on a referent partly outside of his own direct experience, and does not imply a value judgement of his level of knowledge. Every interlocutor must be regarded as possessing interesting knowledge about his group, profession, or society.

2. But the enquiree can however be also be solicited concerning his personal experience. He might be asked to relate one slice of his life or another, or to give an account of events in which he was an actor. In this case first person narrative is preferred. One particular and systematic form of this is the life history, or "guided" autobiography of the interviewee who becomes the theme of the interview or even of the enquiry itself. There is a wealth of documentation on this subject, strongly represented in the "*Terre Humaine*" collection[28]. However, the "slice of life ", i.e. the restricted, episodic biographical narrative, chosen in keeping with its relevance to the enquiry, is of easier to access and to manage than biography [29]. Hence, depending on the research theme, the personal narrative fragment might evoke a migration, various therapeutic remedies used during illness, conversion to a new religion, the account of a divorce, the sparking off of a strike or the stages of an apprenticeship... Such slices of life represent priceless data to the extent that these may be used at various levels : as a main corpus subjected to comparative analysis, as elements of a case study, or as examples illustrating or demonstrating arguments in the final text.

Interview as interaction

An interview should not be considered as a mining-type extraction of information. Regardless of the situation, the research interview remains an interaction : its proceedings

[27] See, among other texts whose aim is to restitute this savoir-faire, Jean-Claude Kaufmann's presentation of "*the comprehensive interview*" (Kaufmann, 1996).

[28] For a didactic presentation of life narrative also called « narrative interview » see Bertaux, 2006, and Peneff, 1990.

[29] Peneff underscores thus the excessive, « all or nothing », nature of biographies as well as the importance of conducting « *a modest biographical enquiry* », limited to the recent past and to the field of the enquiree's work or profession. (Peneff, 1995: 134).

obviously depend on the strategies of both (or all) of the partners in the interaction, on their cognitive resources, and on the context in which it is situated.

This interaction can be analyzed from various perspectives. The work of Briggs, for example, is based entirely on the observation of the interactive reality of the interview[30]. He offers constructive criticism of the widespread omission of this interactive reality, denounces the myth surrounding interviews and highlights the "*illusion of reality*" and the "*false sense of objectivity*" which this omission occasions. The cultural and linguistic characteristics peculiar to the context and situation of the interview give rise to various "biases" concerning the referential contents, which sociologists and anthropologist tend to interpret at face value. Conversely, Briggs insists on the fact that the interview is an intercultural encounter, more or less decided on by the enquirer, one which provides for the confrontation of different and sometimes incompatible meta-communicational norms. But he falls into the opposite excess, by amalgamating all types of interviews to prove his point. By putting a systematic emphasis on the analysis of meta-communicational norms and indexical significance, he transforms all types of interviews into a corpus of socio-linguistic analysis. As a result (and this is also, incidentally, the tendency of ethno-methodology), the referential functions of the interview, that is the information derived after all through these "biases", are particularly neglected or played down. Yet the research and evaluation of this information lies at the heart of "field policy". Taking the meta-communicational situation into account might be connected to two different strategies: either it allows a better interpretation of elements of information or it constitutes an end in itself and disregards the content of information elements. Only the first is useful to the field researcher.

Olivier Schwartz's measured attitude is therefore preferable in that it signals the maximalist excesses of "critical-analytical" critiques and highlights the various effects of the fieldwork situation (Schwartz, 1993 : 276-77). He underscores the risk of dissolution of the referent : "the fact that the 'things that are said' are not immediately true information about the world […] is no reason to underestimate their informative or cognitive value" (*id.* : 283-84).

It should also be noted that any interview might call on three interlaced levels of interpretation, none of which is to be neglected despite the complexity of their constant embeddedness : (a) information on the world (on the reference reality) ; (b) information on the point of view of the interlocutor talking about the reality in question ; (c) information on the communicational structure of the interview. In an ordinary enquiry, the first two levels comprise the research objective, and the third is relevant only to the extent that it influences these two levels.

Moreover, by overemphasizing the hegemonic communicational effects induced by the interview, characterized as a West-centered model, Charles Briggs underestimes the reactive capacities of interviewees (the ability to resist, sidetrack, or counter-manipulate). The emphasis placed by Pierre Bourdieu and his disciples on the domination effects inherent to the interview situation, given the differences in social status between researchers and their interlocutors, originating in more modest social milieus, goes in the same direction (see. chapitre V).

[30] Briggs, 1986.

Interview as conversation

Bringing the guided interview closer to an ordinary interaction like a conversation is a regular strategy of the ethnographic interview[31]. The aim is to reduce, as far as possible, the artificial side of the interview, as well as the enquirer's imposition of annoying meta-communicational norms.

"Dialogue", an ingredient of all conversations, is not considered here as an ideological obligation, contrary to moralistic post-modern discourses. Dialogue is a methodological constraint. It aims at creating, if necessary, a situation in which the anthropologist's informant may enjoy real freedom of speech, instead of feeling that he is being questioned by a judge or a policeman. In other terms, it is a question of getting the interview as close as possible to a mode of communication that is recognizable in the local culture[32]. The field interview thus tends to be situated at the opposite pole from the administration of a questionnaire - a procedure which is extremely artificial and directive, in the image of the "mining" perspective mentioned above.

This has practical implications for the manner in which interviews are conducted. Some interviews do in fact maintain the questionnaire structure, even though the questions are said to be "open ". Owing to this, the interview guide runs the risk of limiting the enquirer to a list of standard pre-programmed questions, to the detriment of the spontaneous improvisation of a real discussion, marked by digressions, backtracking, beating around the bush, hesitations, pauses, anecdotes... The field interview must, as far as possible, adopt the mode of conversation and avoid the mode of the questionnaire. It is therefore useful to propose a distinction between an interview guide and an interview frame[33]. An interview guide organizes beforehand the questions one asks, and can change into a questionnaire or an interrogation. An interview frame, on the other hand, is a personal "memory jogger "; it helps the enquirer to remember important themes, without disrupting the internal dynamics of an ordinary discussion.

Moreover, while the interview guide forces the interviewer to go through a set of questions, one after the other, in a given order, the interview frame leaves the researcher free to adapt himself to the person he is interviewing: depending on his centres of interest, his competences, or the drift of the conversation, certain points will be put aside whereas others will be given in-depth attention. With each new interview, the researcher goes shopping, choosing certain items on his list.

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[31] This has not always been the case. We know that Griaule, for example, and other colonial ethnologists and anthropologists, used, and sometimes abused of, directivity; see van Beek, 1991: 139-158. Conversely, for Everett Hughes, "*interviews lean towards the form of the society's conversation to the extent that, once the interviewer catches on to the level of the interviewee's discourse and adapts himself accordingly, communication becomes supposedly close to communication between equals, and the information collected is considered as coming from a man talking freely with a friend*". » (Hughes, 1996 : 287)

[32] This corresponds to what Cicourel calls "*ecological validity*" (Cicourel, 1982: 11-20), in other words the degree to which the circumstances created by the researcher's procedures match those of the everyday worlds of the subjects (Briggs, 1986:24). This is why it is often advisable to start interviews with an informal chat, or by so-called ´"descriptive" questions which solicit the interlocutor on a familiar or convenient register of enunciation. Spradley insists particularly on these types of descriptive questions (Spradley, 1979: 81-83). He also establishes a parallel between conversations and the ethnographic interview as being two close types of "*speech events*" whose similitudes and differences he analyzes.

[33] The contents of Jean-Claude Kaufmann's "grid of questions" (*grille de questions*) is relatively close to what we refer to here as a "question frame" (*canevas d'entretien*) : "*ideally, the aim is to trigger a conversation dynamic, which is richer than mere answers to questions, while keeping to the theme. In other words: forget the grid*" (Kaufmann, 1996, 44).

The frame, in other words, is limited to “the questions on your mind”, leaving to improvisation and craft the task of transforming them, in the course of the interview, into “questions that are really asked”.

In fact, the questions on the researcher’s mind are specific to his problematic, object and language. They are relevant only within his own sense world. They have no immediate meaning for his interlocutor. It is therefore necessary to transform them into questions which make sense to the interlocutor. This is where the informal know-how, acquired through participant observation (and through the difficulties and misunderstandings of initial interviews) is transformed, often unconsciously, into the capacity to converse with the interlocutor, on his own ground and in his own codes.

Recursivity in the interview

Field interview has a number of particularities. One predominant characteristic is that, far from being a means of getting the "right answers", an interview must also make room for the formulation of new questions (or the reformulation of old ones). Herein resides another major difference between the interview, carried out by a researcher, and the questionnaire, subcontracted to enquirers. This is another case in which informal know-how pays off.[34] Accepting the interlocutor’s detours and digressions, his hesitations and contradictions, is not simply a matter of "making him relax". In fact this is adopting an epistemological attitude. When an interlocutor goes “off the subject”, or gives confusing answers, it is time for the researcher to prick up his ears. And, far from dismissing the anecdote, he would do well to should go for it, as it is "meaningful” and full of new insights. This brings us to what we may call the recursive nature of the field interview[35]. In fact, the interviewer should rely on what has already been said by the interviewee in order to produce new questions. These questions correspond, simultaneously, to the "questions in the researcher’s mind" (strategic level of the research questions) and the “questions that are really asked” (tactical level of the interview interaction).

This capacity of “immediate decoding”, in the very course of the interview, is at the heart of the field researcher’s know-how. The researcher, far from being a simple “recorder” or a simple “enquirer”, remains as interpretatively alert as possible. He sorts out mentally the relevance of what interlocutors tell him (regarding his research), the meanings that arise from this, and is thus able to identify, in the course of the interview itself, the enunciations or information capable of illustrating such and such a conclusion, of reformulating a particular problem or of reorganizing one set of facts or another. In this respect, the interview, like participant observation, is a privileged space for producing “grounded theories”[36], which are tested as they emerge.

The interview as an invisible negotiation

The enquiree does not have the same interests as the enquirer nor does he have the same representations of the interview. In a certain sense, each tries to manipulate the other. The informer is far from being the researcher’s or a victim caught in the trap of his unbridled curiosity. The informant does not shun the use of active strategies allowing him to take advantage of the interview (to gain prestige, social recognition, monetary returns, in the hope

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[34] "*Appropriate or relevant questions are seen to emerge from the process of interaction that occurs between the interviewer and the interviewees (...); the success of this undertaking is ultimately contingent about the skill and sensitivity of the interviewer*" (Schwartz and Jacobs, 1979: 40). “*The best question is not one in the grid, it is the one that is spurred by what the “informer” just said*” (Kaufmann, 1996, 48).

[35] Schwartz and Jacobs, 1979: 45

[36] Glaser and Straus, 1973. Anselm Strauss (1987: 10) speaks of " *successively evolving interpretations made during the course of study*".

of future assistance, in view of legitimizing his own point of view...) or defensive strategies aimed at minimizing the risk of what he says (giving little or false information, getting rid of the nagger as soon as possible, being obliging by giving the answers he thinks the enquirer expects [37]...). The researcher's problem is a double bind dilemma, that is to say he must remain in control of the interview (since his enquiry must progress) while allowing his interlocutor to express himself freely, in his own style (since this is a condition of the success of the interview).

Empathy and interview

There is another double bind inherent in the researcher's management of the interview. The researcher is bound professionally to give credit to what his interlocutor has to say (however strange or dubious the former may appear in his sense world) and to listen to him with sympathy, approval or complicity. Of course, conflict or contradiction may arise during interviews, but empathy is still the rule. This is not simply an enquirer's trick (or even a form of duplicity on his part). In fact, a primary objective of the researcher in the field is to understand, with as much finesse as possible, the logics underpinning his interlocutor's representations, perceptions or actions. Consequently, he is required to "take seriously" all that he is told and to "be on the same wave length" as the person speaking to him. Rather than a trick, then, empathy is the precondition for gaining access to the logics and sense world of persons being studied by the anthropologist. It is by taking them seriously that he is able to fight against his own preconceptions. This is what Bellah terms "*symbolic realism*" [38], also called "principle of charity" by others[39]. The researcher should take serious account of the "reality" described in the informant's discourse, this reality depending on the significance that the informant gives to it. It is this reality that the researcher must attempt to approach as closely as possible. At the same time, we would hope, the researcher is not naive! The need to be critically alerts him against a literal acceptance of what is related to him. The discourse on reality must not be confused with reality itself[40].

This is a real dilemma. How can we combine empathy and distance, respect and suspicion? As is usually the case in a dilemma, there is no clear-cut solution. However, the attempt to introduce a time gap between these two operations appears to be a sound research policy. According unperturbed credence will precede systematic doubt: it is even a condition of the latter. During the interview, credit is given to the interlocutor's discourse as making sense: in fact, one can only have access to this sense by taking all that is said seriously. The interview is therefore managed on the benefit of the doubt. Later on, critical or even suspicious decrypting will comment on the sense of this meaning, as well as the relationship between the enunciator and the enunciation, between the referent and the context. However, the experienced researcher is able to combine these two levels, or to navigate between them in the course of the interview, empathizing with his interlocutor whilst remaining mentally vigilant [41].

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[37] Look back at van Beek's analysis of Dogon's manner of answering to Griaule (van Beek, 1991).

[38] See Schwartz and Jacobs, 1979: 48-49.

[39] Feleppa, 1990, 102-106.

[40] For example, Jean-Claude Passeron reminds us of the propensity interviewed actors have of putting a « *patina of logic* », according to Pareto's expression, on what they say. (Passeron, 1995: 62, 80).

[41] In the same vein, some authors have suggested that a distinction should be made between "empathy"- an attitude which is a methodological requirement - and "sympathy", comprising personal feelings or even identification, entailing the risk of bias (see, for example, Paillé and Mucchielli, 2005: 71) : « *These personal sentiments would appear to be become a potential biasing feature of the research when the observer's intentions to empathize with the individuals in the setting, that is, to truthfully understand the situation from the actor's perspective, become transformed into a sympathetic stance. A sympathetic stance exists when the observer takes*

Interview and duration

The insertion of the interview in a diachronic dimension stands in contrast with the "mining perspective". Any interview is potentially the first of a series. Moreover, it could be the beginning of a relationship (albeit one that is usually short lived). An interview is not a closed case, over and done with once and for all, but rather an open, improvable file. After several interviews with the same interlocutor, one gets nearer and nearer to the mode of a real conversation; a subsequent interview often makes room to develop and comment on questions arising from a previous interview. Besides, with each new interview, the now familiar interlocutor sees the researcher in a more favourable light, as a person with greater competence. This enhanced status is highly advantageous for the researcher. In fact, the more one has the impression of dealing with an incompetent stranger, the more one tends to take him for a ride[42].

The group interview

The interview is typically considered as an interaction between two persons. It is, indeed, thanks to face to face exchange that a relationship based on trust is developed, that the exchange is delivered from the impact of social control, that discursive strategies can take place peacefully. But the fact that this dyadic form is rightly seen as primordial should not lead us to conclude that this is the only possible form. Interviews are sometimes collective.

It is important to note that the group interview is usually impromptu, involuntary, unsolicited by the researcher. It is related to the fact that the socio-anthropologist works in "natural settings". To have a banal everyday kind of chat with someone means assuming the risk of the family, close relations, neighbours or colleagues - depending on the situation - getting involved in the conversation. The appropriate strategy in such a situation is neither to forgo the possible advantages of this multiple interaction (the opportunism and know-how of the researcher allows him to make the most of the remarks - or silences - of each participant), nor to miss out on the benefits of a private interview (which can always be rescheduled, in a more discreet setting).

But the group interview can also be programmed and solicited. It might involve a group that already exists (the executive council of an association, a family, a class) or an *ad hoc* group put together by the researcher's initiative. The most familiar form of the *ad hoc* group is the *focus group* [43]. The problem with the *focus group* is not so much that it is an artificial situation (why not, as long as one is aware of this and controls the consequences?); the problem is the naive or lazy end to which it is often employed. It is naive when it sees the remarks made, especially when they are consensual, as expressing a collective point of view, and, worse, the point of view of the "community" that the participants are supposed to represent. It is lazy when it becomes the only, all-purpose strategy of enquiry, i.e. a kind of cut rate qualitative enquiry. We are forced to recognize that focus group enquiries, on the rise in Africa, in the context of expertise financed by development institutions or in the framework of so-called rapid and participative enquiry [44], generally combine naivety and laziness.

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the side of, or promotes the perspective of the group he studies » (Johnson, 1983: 215). This is the type of bias that will be analyzed on the subject of populism (see chapter VI).

[42] See J. Bouju, 1990: 161.

[43] See, inter alia, Agar and MacDonald, 1995; Morgan, 1988. "*Saying that a focus group is a workable option for a project is not the same as saying that they are the best way to gather the data for that project*" (Morgan, 1988: 24).

[44] Cf. RRA, Rapid Rural Appraisal; PRA, Participative Rural Appraisal (see Chambers, 1981, 1994; for a critique, see Lavigne-Delville, Sellamna and Mathieu, 2000).

3) Observation

It seems that over the last few years, observational data have been neglected or treated like the ugly duckling of socio-anthropological corpuses. Under-addressed in works dedicated to field enquiry[45],under-used by many researchers who tend to focus only on interviews, observation has, moreover, been excessively criticized by postmodern scholars in the name of the original sin of naturalism or positivism. Observation is, nonetheless, a "natural" cognitive attitude, one which we all apply successfully in the course of our everyday activities. Consequently, it might come as a surprise to find that the field researcher is much too often satisfied with a lackadaisical practice of routine, everyday forms of observation, without making the least attempt to systematize, discipline and organize observation in view of its use in the production of the corpus itself.

Besides, not only are social activities equally accessible through what is seen and what is heard, not only do they produce sense in both of these two registers, but, what is more, certain social activities, and not the least relevant among them, chiefly entail pre-discursive or infra-discursive information, or even silence or latent materials[46].

Chapter IV will focus specifically on various epistemological aspects of the question of the observation and description, which are closely related.

4) Formal procedures of intensive micro survey

In the case of observation or of a guided interview, recourse is sometimes made to a particular technique of data production which I will mention here not because it is an extensive survey in the ordinary sense of the word, but because its aim is a systematic production of a finite amount of intensive data: counting, inventories, nomenclatures, plans, lists, genealogies...[47] It is impossible to make a list of these techniques, because for 10 000 different problems there are 10 000 different techniques, to be invented on one's own; in one instance this concerns the space occupied by co-operators during a general assembly ; in another, the everyday work of a woman and her husband; or, in yet another, it is the chart of family relationships within a municipal council, the list of therapists consulted by each member of a domestic group over three months, the length of speech in a palaver, the number of adjournments in a court case, the percentage of nurses on a ward..

The importance of this type of data production should by no means be underestimated: this is how one learns the "trade"; it is through the search for empirical data with a certain degree of systematic organization that the researcher arrives at the required distance in relation to discourse (others') and impressions (his own). This is where the collection of "emic" data (discursive data aimed at making indigenous representations accessible) is combined with "etic" data (data constructed by means of observation or measuring).

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45 "*Qualitative observation has remained under-addressed in the methodological literature […] Most of the major research treatments of qualitative methods focus on participant observation to the virtual exclusion of observation as a method in its own right*" (Adler and Adler, 1994, 378).

46 The ethnographic observation could then be described as "*the "silent" dimension of the social*" (Hirschauer, 2006: 413).

47 "*Researchers almost systematically use additional techniques of a quantitative type, such as intensive survey, statistics, genealogies and interviews structured around a carefully selected sample* » (Ghasarian, 2002: 10). Maget's manual (1953) had already set out "*all the artifacts which stabilize data : statistics, technical drawings, morphological schemas, graphs, maps and photographs* » (Cefaï, 2003, 29).

Intensive survey procedures offer various advantages. They sometimes provide figures, though not necessarily in the guise of percentages or samples [48]. It is therefore no longer a "qualitative" issue but rather a matter of “intensive quantitativeness" exercised on a limited set of elements. Intensive enquiry procedures produce indicators which, when correctly elaborated, belong to the category of “unobtrusive measures”[49].

Intensive micro survey procedures are in fact observation frames or measures that the anthropologist constructs in the field, when necessary, and in his own style, that is in keeping with his research problematic (in constant evolution), his interrogations (constantly renewed), and his (relatively cumulative) knowledge of the field. Certain techniques (like kinship charts and lists of land holdings) have now become standard due their linkage with a number of now classical research themes and with certain currently orthodox problematics.[50] Learning these techniques methods is essential to the professional training of the anthropologist. But one must insist on the researcher’s capacity not only to use one or another of the intensive survey techniques already on the market, adapting it if necessary to his own needs or his personal field context, but even more so on his capacity to tinker and to invent appropriate techniques for his own use in accordance with the novelty of his object and approach [51].

These techniques might intervene during different phases of the enquiry process, and might therefore assume a variety of meanings. At the beginning of fieldwork it is mostly a question of charting "backgrounds ", in the concrete and in the metaphorical sense. This makes it possible to situate main actors, relevant spaces and everyday life. Such elements provide the newcomer with markers, points of entry, inroads, thus making the acquisition of organized knowledge possible. The last stage of the enquiry is mostly used to verify intuitions, to provide more "objective " elements, to gather proofs and confirmation. During this phase, field research mechanisms are less polyvalent and more focused.

5) Written /documentary sources

Although conventional and non-specific to field enquiry, documentary sources should not be forgotten or minimized.

We may thus evoke in passing:

- scholarly sources on the domain considered (anthropology, history, economy etc.)
- “grey literature” (reports, evaluations, masters theses)
- the press
- archives
- local written sources (pupils’ exercise books, letters, account books, diaries, tracts, etc.)

A part of such data is gathered before the field enquiry (scholarly works on the domain in question - anthropology, history, etc. - and " grey literature” - reports, evaluations, theses, etc.). These allow for familiarization or, better yet, for the elaboration of exploratory

[48] "*Qualitative research implies a commitment to field activities. It does not imply a commitment to innumeracy*" (Kirk and Miller, 1986: 10). “*Examples of methods of survey and inventory abound in works based on observation. The arsenal of methods used to make inventories, surveys, and frequency calculations, are almost unlimited*.” (Peneff, 1995: 123). Becker evokes the usefulness of what he calls « *quasi-statistics* », « *imprecisely sampled and enumerated figures*” (Becker, 1970: 81).

[49] Schwartz and Jacobs, 1979: 75.

[50] The manual by Creswell and Godelier, 1976, provides numerous examples.

[51] See the various examples quoted by Becker, 1970

hypotheses, hunches and personal interrogations. Others are indissociable from field enquiry and are integrated into the former (writing produced by actors - school exercise books, personal diaries, tracts, etc. -, local archives and the local press). Still others are a part of autonomous corpuses, distinct from and complementary to those produced in the field (press, archives).

The fact that the majority of the written data concern preexistent documentary sources should not lead us to neglect the possibility getting local actors to write on matters related to the enquiry: a teacher might agree to make pupils write a composition on a subject suggested by the researcher, a trainee midwife might agree to keep a journal of her night duties, a police officer might agree to write down, in the evening, an account of his missions accomplished during the day…

The frequent - and abusive - assimilation of anthropology with the study of "societies without writing", as well as the fact that the data field enquiries transcribe is mostly oral in origin often overshadows the fact that there is today no society about which there is no written document. Nor is there any society in which writing does not play a role. For the anthropologist, written sources are therefore simultaneously a means of introducing a diachronic perspective and an indispensable widening of context and scale, an entry into the contemporary experiences of the group under study.

6) Audio-visual sources

They have been present since the early days of ethnographic enquiry, bringing to mind the pioneering attempts made by Margaret Mead and Gregory Bateson. But, paradoxically, their use still remains largely marginal in written production (a few scattered illustrations or an annexed CD). As “traces” of fieldwork, they could be classified among survey procedures were it not for their specificity. In fact, as corpus in a strict sense, audio-visual data poses the daunting problem of treatment and analysis and that of the integration of the iconic analyses into a descriptive and interpretative argumentation which is necessarily written. Barring certain specialized fields (body techniques, ethnomusicology, ethno-methodology, etc), it is clear that few really conclusive experiments exist in this field, which, consequently, is still largely experimental, despite the fact that the advantages of such differed and reproducible observation allowed by film viewing, photographs or video cassettes were analyzed some time ago[52].

In fact, audio-visual data often comprises a parallel and distinct product, one which is not subjected to the same epistemological narrative and communicational constraints (like a documentary film or a photographic exhibition). This is, to date, usually the case with such data, but this takes beyond the scope of our present subject[53].

However audio-visual data can also comprise preexisting images and sounds, produced not by the researcher but by the local actors (wedding videos, family photographs, local radios programs), or even by external actors (reports by radio or television). In this case, the production of a corpus is analogous to the treatment of written documents.

[52] *See.* Hockings, 1975 ; Heider, 1976 ; Crawford, Turton, 1992 ; de France 1994 ; Devereaux and Hillman, 1995 ; Maresca, 1996.
[53] *See* Olivier de Sardan, 1999.

Data combination

The almost permanent combination of the various types of data referred to above is one of the peculiarities of field enquiry. This combination is even less susceptible of being transformed into a recipe than certain particular modes of data production. It will suffice to mention two aspects, among others.

Data eclecticism

The field enquiry makes use of all available materials. Its resolutely eclectic empiricism uses all possible modes of data collection[54]. It is obvious that the types distinguished above not only intersect frequently but often exist in synergy[55]. Participant observation allows a choice of relevant interlocutors and the creation of a conversational register during enquiry but also serves to identify and support observation, specific sequences or audiovisual recording. In situ interviews represent a particular form of interaction, while contributing to the integration of the researcher into the local culture. Intensive micro survey procedures rely partially on speech (and therefore on the interview) and partially on sight (and therefore on observation). Local written and audiovisual sources remain attached to local actors and events, and intersect with the day to day life in which the researcher participates and with the interviews he solicits.

Eclectic sources have a great advantage over enquiries based on a single type of data in that they allow the researcher to take into account the various registers and stratifications of the social reality being studied. It is therefore difficult to understand peremptory statements about the intrinsic superiority of one type of data over another. In contradiction with Harris who places observational procedures at the top of the pyramid *(etic* : *see* chapter III*)*, in the name of a strongly positivist cultural ecology, Fabian favours verbal interactions, in the name of a dialogic ethnology which calls to mind certain a post-modernist extravagance [56]. Contrary to all this, there is every reason to take into account data entailing a variety of reference, relevance and reliability, providing access to different bits of reality and their peculiar characteristics, and whose overlapping, convergence and intersection guarantees enhanced plausibility [57].

However, the interview is often used as an almost exclusive mode of data production, separated from participant observation, in particular. [58] There is sometimes, in this case, a tendency of standardization at the level of methods of data collection (sometimes under the name of guided interview, free interview, semi-directive interview or semi-structured interview) or at the level of methods of treatment (content analysis, discourse analysis software). Interview sociology thus becomes a particular methodological configuration, through the autonomization of the interview procedure seen as the primary mode of data production[59]. One tends to stray away from what I refer to here as the field enquiry, which happens to be fundamentally polymorphic.

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[54] "*Ethnography consists of a cluster of methods rather than a single one*" (Schatz, 2007: 6).

[55] The term "triangulation" has sometimes been used to describe the recourse to a variety of sources. I prefer to reserve this term to the intersection of information (see below).

[56] Harris, 1976: 329-350; Fabian , 1983

[57] Others have already made the point: Becker, 1970: 32, 56 and 57; Pelto and Pelto, 1978: 53; Strauss, 1987: 27.

[58] Bourdieu's work, *La misère du monde* (Bourdieu, 1993) is a significant illustration of this tendency.

[59] Briand and Chapoulié (1991) consider this to be peculiar to French sociology, less inclined to the practise of observation than American sociology. However, Stanjek, for his part, distinguishes this tendency in urban Anglo-Saxon anthropology, of which he deplores the "interview-based" character (Stanjek, 1990: 247).

Moreover, each research topic and each field enquiry has its own characteristics, demanding the use of certain methods rather than others. Each research context has its specificities: the researcher must adapt to the context instead of trying to force the contextual reality into prefabricated methodological routines. Eclecticism in data production means inventing new combinations, in sync with the problem considered; it implies a skillful dosage or modulation of proven modes of corpus constitution[60].

The case study

The "case study" is one particularly fertile mode of data combination, among others. It brings together, in a single sequence, delimited in space and time, the types of data referenced above. This sequence could be a community event, or a set of specific interactions, and might be structured, from the actor's point of view, as ritual, as a stake or as a "problem" (social and/or individual). The anthropologist combines sources around a particular social situation: observation, interviews, intensive micro survey procedures, written sources. This might take the form of a land tenure conflict, a political nomination, a religious ceremony, a divorce, a strike, an accusation of witchcraft in a village, a therapeutic episode, a local election. There are countless "cases" whose "description" and decoding provide possible inroads into broader research objectives.

The Manchester School is doubtlessly the first to have conceptualized this method in anthropology[61], although it existed previously in the form of practice for a long time, certainly since the beginning of field anthropology: Malinowski or Evans-Pritchard, among others, made abundant reference to such "cases"[62]. The Chicago School, for its part, based its work primarily on case studies[63]. The Italian "micro-storia" has recently imported and systemized a peculiar brand of this orientation into the field of history[64], although history has always made more or less frequent use of this device.

There exist numerous interpretative and theoretical uses of the case study, addressed by an abundant literature, proposing various typologies of cases and of their usage[65]. Some limit themselves to illustration, others describe and analyze the intrinsic significance of local situations, while yet others extrapolate from a reference case in order to produce middle range analyses, the main level of theorization in socio-anthropology.

Glaser and Strauss (1973: 152) remark that "*case studies can serve as examples for pre-existing general theories or as a means of generating new theories*. Stake (1994: 238), on the other hand, underlines the dangers of what we may call the "pretext case": "*Damage occurs when the commitment to generalize or create theory runs so strong that the researcher's attention is drawn away from features important to understand the case itself*" (a phenomenon which will be addressed later using the term "over-interpretation", *see* chapter VII) [66]. Certain case study approaches have become more systematic and attempt to avoid the

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[60] "*All forms and tactics of social research must be continually related and successfully embedded in the social setting being researched. Each method has its values and problems and these must always be weighed against each other*" (Douglas, 1976: 34).

[61] See Garbett, 1970: 214-237; Velsen (van), 1978; Mitchell, 1983: 187-211.

[62] See Malinowski, 1963; Evans-Pritchard, 1972.

[63] *See* Platt, 1992.

[64] See Levi, 1989; Levi, 1991; and Revel, 1989.

[65] See, inter alia, Ragin and Becker,1992 ; Hamel, 1993 ; Yin, 1985 ; Passeron and Revel, 2005 ; Stoecker, 1991 ; Stake, 1994 ; Gasper, 2000 ; George and Bennett, 2004 ; Gerring, 2007.

[66] This may be seen as advocacy for what Stake terms the *intrinsic case study*, in the framework of a typology in which he differentiates between the *instrumental case study* (the case studied based on broader objects which it serves to elucidate and illustrate); the *intrinsic case study* (the case studied for itself, as a specific object); and the

pretext case by including the borderline case, the marginal case or the negative case : the "analytical induction" method thus proceeds from case to case in order to generate and to readjust its interpretations.

However, it is important to differentiate between the *field case,* in the specific sense assigned to it in this work, and the abstract case or the *aggregate case,* understood as any unit of a comparative series, regardless of the level of generalization or abstraction. In the broad sense of the word, even vast multivariate statistical analyses compare cases. In a different register, comparative political studies consider towns, countries, types of constitutions or political regimes as cases that can be included into a limited set in an attempt to discover explanatory variables: these are greatly aggregated macro-social units, with no concrete meaning when seen from the perspective of a policy of fieldwork. Conversely, the field case is directly accessible to the researcher, via his usual modes of empirical data production. To take an example in another discipline, the "clinical case" examined by a practionner (the equivalent of a field case) has nothing to do with the "case of the American health system", which is strongly aggregated, and analyzed by the public health specialist.

Consequently, I do not follow Jean-Claude Passeron and Jacques Revel in the idea that "*the case cannot be defined by the amplitude of the object being considered; it must not be confused with the intensive nature of the examination to which it is subjected. In this regard, Braudel's Mediterranean is as much a case as the objects chosen by micro-historians (with their very variable scales)*" (Passeron and Revel, 2005: 11). In fact, Passeron and Revel are concerned with intellectual operations which require "*thinking through cases*", whatever their scale, which they placed in opposition to the classic hypothetico-deductive scientific model. Thinking through cases, which operates via generalizations and comparisons based on single occurrences, is therefore inherent to social sciences, at least in their qualitative versions, regardless of the nature or amplitude of the cases considered. These authors adopt a broad definition of "cases", explaining why, at times, the qualitative approach or the field enquiry may be called "case study" approach. But if, on the other hand, one adopts as I do a restrictive definition of cases, referring to the production of field data, then size becomes important: the field case, which is necessarily circumscribed, is specific.

The field case is therefore different from the aggregate and macro-social case, on the one hand, and from the broad definition of a case which may be assimilated with all types of qualitative, descriptive or comprehensive studies, on the other hand. Later, on the subject of observation, which is one the dimensions of the case study, I will discuss the necessary distinction to be made between the wide sense and the narrow sense of the term "case study" (see chapter IV).

However, the field case, in anthropology, does not imply imprisonment in the case, or imprisonment in the field. A field case, even in its "micro" version, even in the event of in-depth study for its own sake, even when it is not included in a corpus of cases aimed at comparison, even if it does not generate fresh theorization, always alludes to something larger than itself, beyond itself: a larger set, a broader social context, other cases, a research theme, a problematic. It works as a revealer, descriptor, indicator, symptom or example. Note has been taken of the importance of the "*diagnostic event"*- a social event the researcher chooses in view of its symptomatic properties, because it is « revealing », because, in the interactions it entails, one may observe the interplay of wider social logics, the implementation of more

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collective case study (several similar of distinct cases are collected, analyzed and compared); he adds others varieties appraised as marginal: the case for pedagogical use, the biography and the legal case (Stake, 1994: 237-38).

extensive practical norms, the deployment of recurrent strategies, the weight of exogenous constraints, the effect of external forces or mechanisms.

This metonymic research posture in the face of the case study differs from the posture of ethno-methodology, which, inversely, tends to limit and even to imprison analysis within the strict bounds of selected cases (in this instance, sequences of interaction)

Fieldwork policy

Up to this point, I have provided a relatively quick overview of the primary forms of data production in fieldwork. The process of field research can also be apprehended in a synthetic manner in relation to certain general methodological principles which define anthropology, "despite the odds", as an empirical social science, not merely as a form of erudite journalism, chronicle, or exotic autobiography. Fieldwork, which brings together the various forms of data production mentioned above, is in fact the result of a "scientific strategy" which guides the researcher. This strategy might be more or less explicit or essentially implicit. Implicit strategies sometimes conceal methodological shortcomings. On the contrary, our intention here is to be as explicit as possible, in order to reveal some of the "principles" which seem to regulate or to optimize " field policy " [67].

Getting free from hypothesis

Sociology, marked by a longstanding dominance of quantitative approaches, has often assimilated, abusively in my opinion, the construction of a problematic with the development of *a priori* hypotheses, which empirical research supposedly aims at validating or invalidating, regardless of the use of quantitative or qualitative research strategies. It is also affirmed that "*the organization of a research around working hypotheses constitutes the best means of conducting research with order and rigour*" (Quivy and van Campenhoudt, 2006: 113). Our objective, in this work, is to strongly nuance such an assertion (insofar as it assimilates the formulation of hypotheses and the determination of measurable indicators with the search for causalities) and to show that there are many other ways of being rigorous. The approach through hypotheses is undoubtedly fundamental when it comes to drawing up questionnaires for a statistical analysis. But it is not really adapted to the socio-anthropological fieldwork, where the comprehension of social logics is worked out gradually, where questions evolve and problematics "move" along progressively, as the enquiry advances, where the researcher follows "tracks" instead of being locked up in a search for confirmations, falsifications or verifications [68].

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[67] This *policy of field researc*h is quite different from similar constructions based on the use of the word *politics*. *The politics of field research* (Gubrium and Silverman, 1989) comprises a "political" re-reading of anthropology, on the foucaldien mode, typical of deconstrcutionist epistemology. *The politics and ethics of fieldwork* (Punch, 1986) concerns the micro-politics of relations between the researcher and subjects of enquiry. The *politics of ethnography* (Bretell, 1993) deals with the reactions of subjects of enquiry to the anthropologists writing..

[68] "*Anthropological analysis commences through the discovery of 'problems'. These are not a priori hypothesis* » (Wright, 1994: 11). Or : "*the researches commences with one series of questions and finishes with a very set of interrogations*" (Burawoy, 2003: 434).

This should not lead to the deduction that field enquiry is able to do without the construction of a research proposal or of a problematic- nothing could be further from the truth! But this preliminary construction, while remaining clearly fundamental, which makes it possible to establish a state of the arts on a topic, to organize and hierarchize a series of research questions, to sketch out orientations, so as not to arrive naive and ignorant in the field, is a provisional construction, destined to evolve step by step, and to change in keeping with the largely unpredictable dynamics.

Reasoning in terms of hypotheses, in the strong sense of the term, in other words becoming the prisoner of a "mental structure of hypotheses", is capable, on the contrary, of cementing the enquiry around a pre-established and rigid interpretative model, obstructing "discovery", "surprise", and theorization based on data. Admittedly, the formulation of hypotheses does not necessarily mean that one must become the prisoner of a mental structure of hypothesis; it can be mere, inconsequential rhetoric … One may use the term "hypothesis" in a "light", or "weak" sense as an equivalent of "pathway" or "provisional interpretation".

The mental structure of the hypothesis, for its part, leads to another risk: that of reasoning in terms of causalities, whereas very often the explanatory register, strictly speaking, is not a part of the socio-anthropological approach, which remains basically descriptive, in which emic meanings occupy a central position (see chapter III). In the field, the question of "how?" is more our business than that of "why". We highlight the processes, logics, norms (official or practical), strategies, representations, but we seldom take the risk of proposing explanations and even less of formulating laws like "if A then B". Nevertheless, asking ourselves "why?" is not uninteresting, or irrelevant, quite the contrary. Economy and quantitative sociology do this quite frequently. At times, socio-anthropology should not avoid facing this question which is liable to produce the positive effects of intellectual stimulation. But our qualitative methods are generally unable to deal with the description of causalities, especially in the form of the determination of independent and dependant variables [69].

Sociology and comparative politics do have methods aimed at searching for explanatory variables based on qualitative corpuses, through the use of Boolean algebra, but this, in my opinion, at the expense of operations of aggregating and dichotomizing data; and therefore comes at a certain heuristic cost[70].

While recognizing the legitimacy of the search for causalities in the social sciences, the explanatory project cannot be allowed to occupy all the space in the social sciences. And the equal legitimacy of the descriptive project must also be recognized since it is in fact a continuum, and because this ideal-type opposition between explanation and description should not be exaggerated. The explanatory injunction, when combined with the frenetic contemporary quantitative emphasis, tends to relegate - with a variable degree of condescension - comprehensive qualitative research to the rank of nice ancillary helpers in the service of "true science"; namely their own[71].

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[70] See the presentation made by Becker, 2002: 259-304 and Ragin, 1987. Charles Ragin underlines, moreover, that these comparative methods concern "*macrosocial units*" with the intention of producing *"explanatory statements"*(Ragin, 1987: 5).

[71] See Quivy and Van Campenhoudt (2006: 84): "*A research in social sciences tends to exceed a simple description of the social phenomena (even if a good description is no easy matter and can be invaluable); it aims at explaining these phenomena*".

Yet socio-anthropology also has other advantages which are just as "scientific", but these are not best served by attempting to approach field enquiry with the language of strong hypotheses or via the logics of the explanatory variables[72].

Triangulation

Triangulation is the underlying principle of any enquiry, be it criminal or ethnographic[73] : one must combine information! Information obtained from an individual source has to be verified: this is as true for an alibi as it is for a religious ritual. This would seem to be a matter of common sense, and historians have long since established the principle. However, there is a certain ethnological tradition which counters common sense, by transforming one individual into the depository of the knowledge of an entire society. We are referring to the famous "key informant" used by numerous "classic" researchers, and exemplified in many respects by Ogotomêli in the book *Dieu d'eau* [74].

It goes without saying that the researcher may get along well with certain members of a social group and may not like others, and one can hardly blame him for preferring to talk to Marcel rather than Jacques, or Usman rather than Ibraheem. Similarly, when it comes to clarifying certain interrogations, all interlocutors are not equal and commonsense dictates a preference for John's competence and clarity over Michael's confused rambling. But overstepping these obvious commonsense statements by focusing on a single resource-person is both a lazy posture and an epistemologically indefensible stance.

Simple triangulation helps the researcher to cross-match informants in order to avoid being restricted to a single source. But one might speak of complex triangulation when it comes to rationalizing a choice of multiple informants. Complex triangulation aims at a variety of information in face of a problem to be treated. The objective is to cross view-points whose discrepancy appears to be meaningful[75].

Hence, it is no longer a question of "blending" or of "checking" information in the aim of getting to the "real version", it is rather a question of looking for contrasting discourses, of turning discursive heterogeneity into an object of study, of emphasizing variation rather than erasing it or playing it down; in short, what is at stake is building a research strategy based on the quest of meaningful discrepancies.

One thus arrives at the notion of "strategic group". The latter may be understood as an aggregate of individuals who, faced with a particular "problem", have the same attitude, this being determined on the whole by the similarity of their social relation to the problem (here "social relation" needs to be taken in the broad sense of the term and might define a cultural,

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[72] It is true that, similar to the term "hypothesis", the vocabulary of causality is sometimes utilized in the weak sense, in both socio-anthropology and history: "*Though historians willingly use the term 'cause', it is often without any particular preciseness, and in a trivial way, in reference to the antecedents as well as the conditions which supposedly contribute to accounting for a phenomenon*" (Revel, 2001: 56).

[73] It is even an element of popular wisdom as illustrated by the follow Songhai proverb: "if only one of two birds sing, you can't tell which has the sweeter song" (*curo fa heen, curo fa mana heen, bor si jinde kaana bey*).

[74] Griaule, 1975.

[75] The term "triangulation", which is used here in reference to the choice of varied interlocutors with the aim of confronting points of view, has been the object of broader definitions, intersecting with various aspects here addressed under other names. Denzin (1978) thus distinguishes "*triangulation through data*" (which we prefer to call source combination; see above), "*triangulation through researchers"* (this is, in fact, refers to collective enquiries or revisits to the field, see below), "*triangulation through theories*" (I evoke instead the refusal of theoretical strictures, or the combination of heuristic points of view, see chapter VI), and "*triangulation through methods*" (which aims at associating qualitative methods and quantitative methods, but which can also intersect with the triangulation of sources).

symbolic, political or economic relationship). As opposed to classical sociological definitions of social groups (for example the Marxist definition of a social class), we do not consider "strategic groups" to be constituted once and for all nor as being pertinent whatever the case may be. They vary according to the problem considered. They can refer to statutory or socio-professional characteristics (sex, caste, profession etc.), kinship affiliation, solidarity or patron and client networks, biographical itineraries or factional affiliations. Thus the notion of strategic groups is essentially empirical[76]. This simply implies that in a given community, the actors involved do not all have identical interests or representations, and that, according to the "problems" in question, their interests and representations undergo different combinations, which owe little to chance. It then becomes possible to construct hypothesis on what the strategic groups are in the face of a given "problem": enquiry will of course demonstrate whether the hypotheses are valid or not, and whether the strategic groups at the finish are those that were expected at the start. Another empirical task is to determine whether a given strategic group is simply the result of a sum of similar, unconnected individual behaviour, due to homologous "positions" in the face of a particular "problem", or whether a distinct morphology exists, i.e. whether we are in the presence of a corporate group, or of a network, etc.

One must also take into account the existence of "invisible" or "external" groups which are indispensable for triangulation. Interviewing (in relation to a particular "problem") marginal individuals who are not acting in a concerted manner, who are on the sidelines, is often one of the best ways of accessing different points of view. In the same way, "bottom rung individuals", mere "underlings" should not be ignored in favour of leaders, more or less charismatic frontliners, or self-proclaimed spokesmen.

Such an approach is obviously in opposition with a certain "culturalist" point of view postulating the homogeneity and coherence of a "culture". A "non homogenous" [77] perspective is more productive from a heuristic standpoint. To find diversity, one has to look for it! This also applies in the approach of a society through its conflicts, though the enquiry situation is liable to provoke an accusatory discourse (emanating from interviewees on the subject of other actors) whose relevance is mostly related to self-legitimation (or to the benefit of the researcher), and does not exclude other levels of co-operation with the stigmatized actors [78].

Iteration

Field enquiry proceeds by iteration: it functions in a back and forth manner. One might speak of concrete iteration (the enquiry meanders between informants and information) or of abstract iteration (data production modifies the problematic which modifies data production which modifies the problematic).

In its simplest and most concrete form, iteration refers to the comings and goings of a field researcher. Indeed, as opposed to a "questionnaire " enquirer, who starts at one end of the street or of the telephone book and ends at the other, the field researcher goes to see X, who sends him to see Y at the other side of the village or of the town, then goes back to see Z who lives near X. This is because his interlocutors are not chosen in advance via a method of

[76] For a more detailed discussion of this concept initially elaborated by Thomas Bierschenk, based on a preliminary approach by Evers and Schiel, (1988), and on its application in collective field enquiry, see Bierschenk and Olivier de Sardan, 1997.

[77] See Agar, 1986: 49-50.

[78] See Althabe, 1977: 67-77, quoted by Schwartz, 1993: 273.

classification (statistic, random) but rather fall into place in keeping with a permanent compromise between the plans of the researcher and the availability of his interlocutors, the opportunities that present themselves, family ties or previously established friendships and a few other variables. The choice of interlocutors usually functions by "branching out", "arborescence" or “snowball”: each interview gives rises to new orientations, to the possibility of other interlocutors, through direct or indirect suggestions in the course of the interview. The dynamics of the enquiry thus plots its own course, one that is basically unpredictable at the outset, that is illegitimate in the eyes of a statistician or of an epidemiologist, but which nonetheless reveals the "real" networks of the milieu under study. The individuals in a field enquiry (as opposed to individuals in a sample enquiry who are per se and of necessity representative of abstract standardized variables) are not abstracted from their concrete life conditions, from their personal, family or patron and client networks, or modes of sociability. Field enquiry is therefore adapted to local social circuits, to their complexity, embeddedness and distortions. It is not at all linear. Of course, the researcher constitutes lists of persons to interview, but such a list is continuously updated and modified. His research does identify “strategic groups”, but these are adjusted as the work progresses.

But iteration is also, in a more abstract sense, a back and forth movement between hypotheses (in a weak sense) and data, interpretations and results. Each interview, each observation, each interaction provides the occasion for discovering new lines of research, for modifying one’s problematic, for elaborating new ones. Throughout the stage of fieldwork, the researcher is engaged in continuous interpretations, in the course of encounters, observations or interviews, though this occurs in a latent and inexplicit manner. The phase of data production can also be analyzed as a continuous restructuration of the problematic, due to contact with data and through an incessant re-arrangement of the interpretative framework as the accumulation of empirical elements progresses. Baldamus thus mentions "reciprocal double fitting" and supports this notion by using the image of a carpenter adjusting a door in its frame by alternatively planing the door and the frame [79].

Making interpretations explicit

This point is linked to the preceding. The fact that interpretations and reformulations of the research object occur during data production often leads to a contradiction or paradox. Prolonged fieldwork, because it is comprised of continuous retroactive processes between data production and interpretation, questions and answers, supposes permanent verbalization, permanent conceptualization, permanent auto-evaluation : an ongoing intellectual dialogue. But a prolonged field enquiry is for the most part a solitary exercise, and this loneliness is not particularly propitious for conceptual dialogue. The researcher is forced to dialogue with himself, but this dialogue is mostly virtual, unfinished and implicit.

The field diary plays an important role in this regard by allowing regular "summing ups" and a makes up for the lack of scientific dialogue throughout the enquiry. Clearly, the field diary has other functions which are more frequently underlined. On the occasion, it gives rises to specific end product (*see* chapter V), from *L’Afrique fantôme* to *Tristes tropiques* or *Les Lances du crépuscule*[80]. But it is also a reflexive frame for interpretative processes linked to data production, during the stage of fieldwork as well as a method of solitary explanation. This function is generally ignored despite the strategic role it plays throughout the entire enquiry. This can also be assured by a constant compilation of index cards noting

[79] quoted by Katz, 1983: 133, and Seur, 1992: 137

[80] See Lévi-Strauss, 1995; Balandier,1957; Descola, 1994.

interpretations. This is what Strauss calls "*memoing*", an operation to which he ascribes a central role, during the field stage, alongside data collection and coding[81].

Making interpretations explicit through erbalization can be obtained through dialogue with a "research assistant", usually a member of the local milieu, engaged in a durable relationship with the researcher who progressively initiates him into research methods and interrogations. Obviously, the research assistant is himself a source of bias [82]. But he can be of great help in "semiologic translation" (that is as a passage between the systems of significance of the local milieu and that of the researcher), over and beyond the simple function of interpreter (" linguistic translation ") which he often occupies.

Lastly, there is the solution of team work, which is extremely uncommon. Verbalization and objectivation are, in this case, assumed by the existence of a debate at the very heart of empirical research. We are all aware of the central role which debate plays, or should play in the social sciences (this is probably the only guarantee of epistemological plausibility). But debate is usually - at the optimum- *ex post* and rather "rigid". Introducing debate into the enquiry, through collective work, on the very level of data collection and of the interpretative strategies manifested therein, is a procedure that should not be ignored. We have experienced it many times through the canvas "ERCIS".[83].

Constructing "describers"

This is one method of making interpretations explicit through researching *ad hoc* data in order to fuel interpretations with field cases and examples, organising the production of a "sub-corpus" around a specific topic. This introduces a link between interpretative concepts and empirical corpuses, allowing for the construction of the latter around relevant themes. The search for coherent and meaningful data (discursive or otherwise) aims at verifying, infirming or amending a hunch, or at producing the same on the basis of more or less explicit intuitions; it allows a combination of method and improvisation and introduces order and organization into fieldwork, which is otherwise very susceptible to moods and impressions.

Each investigation produces its own describers: for instance, they determine the topics of "slices of life" to collect, they carry out systematic surveys into popular semiology, they organize a precise series of targeted observations, they focus on certain key actors, whether eminent or obscure, they make a thorough panorama of existing associations, they choose significant conflicts…

In the case of multisite comparative studies, which are increasingly numerous, the construction of common describers is, moreover, essential in view of a certain homogeneity of the data produced data, thus ensuring their comparability.

The term "indicator" might also be used, although it is commonly understood as having a strong "quantitativist" connotation. It is in fact a matter of constructing relevant sets of "qualitative" data which might be used to corroborate or infirm, and especially to modify, specific interpretative propositions. What are the specific "observables" provided for testing such and such a supposition or for producing new ones? Indicators may also be used as markers for possible research trajectories or as landmarks identify research trails.

We may prefer to "indicator" another word, "clue". Each field enquiry thus elaborates its own clues ("indices" in French), be they multiple or heteroclite. These are never

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81 "*Writing in which the researcher puts down theoretical questions, hypotheses, summary of codes, etc*."(Strauss, 1987 : 22).

82 See Rabinow, 1988.

83 See Bierschenk and Olivier de Sardan, 1997.

standardized, but rather clear-cut and specific. The advantage of the socio-anthropologist is that he is able to construct clues while developing describers, whereas the historians must be content to identify signs or to discover traces. Carlo Ginzburg uses these words - clues, signs and traces - in support of the "clue paradigm", which brings to mind Sherlock Holmes, and which, in his opinion, characterizes the micro-storia[84]. For doesn't the term "enquiry" link the detective, the journalist and the socio-anthropologist? [85] Admittedly, their objectives, professional constraints, deontology, analytic interpretative procedures, and in part, methods (or usage of clues), differentiate these professions, but this does not prevent them from sharing certain methods of empirical knowledge construction: don't we use the words "hunch" and "rigour" to characterize excellence in the work of all three, alongside the professional expertise of the seasoned investigator when it comes to finding traces or sources and getting them to tell a story- i.e. interpreting them?

Saturation

How do we know that it is time to put an end to the phase of field enquiry? The frame of field enquiry, as opposed to a sample survey, does not include a fixed stop sign. In fact, what happens is that the moment arrives when the researcher is forced to notice a decline in fertile observation and a decrease in the productivity of interviews regarding the "problem" being investigated. With every new sequence, with each new interview, less and less new information is obtained. This means that one has more or less "exhausted" the representations in a given field, or the range of strategies linked to a particular arena. How long this process takes obviously depends on the empirical properties of the field or arena in question, that is, on the characteristics of the research theme defined by the researcher with respect to a specific social group.

Glaser and Strauss are the first to have developed this notion of saturation. But they gave it with a more theoretical bend, by associating it with the progressive construction of "categories" (sorts of ideal-types) which makes it possible to compare groups and societies : " *Saturation means that no additional data are being found whereby the sociologist can develop properties of the category. As he sees similar instances over and over again, the researcher becomes empirically confident that a category is saturated* "[86].

Of course, the principle of saturation is more than a mere stop sign : it is a methodological guarantee of prime importance, one that is complementary to triangulation. By postponing the end of research on a theme or sub-theme until no new data are collected on that theme or sub-theme, we prevent ourselves from being satisfied with insufficient or sporadic data, subscribe to process of relative data validation, and make room for confrontation with divergent or contradictory data. "*We go for constraints that impose the postponement of induction*"[87].

The concept of strategic group is, in this regard, complementary to the principle of saturation: by varying interlocutors in order to take their differing perspectives into account, the researcher retards, in a rational manner, the limits of saturation. When this point is finally

[84]Ginzburg, 1980 (also cited in Ginzburg, 1989). Since then, this metaphor has been extensively used to describe the relationship between qualitative data and interpretations; for example, in political science, see Gerring and Thomas, 2005: 13.

[85] "*I shall argue in this book that the investigative methods of journalists and some of the investigative occupations, such as detective work, are of great importance in developing all forms of field research*" (Douglas, 1976: 14).

[86] Glaser and Strauss, 1973: 61

[87] Schwartz, 1993: 286

attained, saturation is all the more relevant insofar as the entire range of postures has been explored. The same with the quest for counter examples.

In fact, the taking into account of counter examples appears to be one of the prerequisites and one of the advantages of field enquiry (*see.* Chapter VII). But on this topic - and others - there are researchers who lack methodological vigilance. While a statistical enquiry is largely satisfied with an explanation of 80% of situations, field enquiry treats the exception, the "negative case" [88] with the same attention as the modal case. Departure from normal behaviour and discrepancy in conversation thus serve as an eye-opener or a strong indicator of norms and of the modalities of contrast among norms. Consequently, the principle of saturation aims at describing the space of the possible within in given time-space, with respect to a given "problem".

The control group

It is usually useful, even necessary, to choose a site of intensive research based on a social network, which might later serve as the base for more extensive enquiries. The "control group" varies according to the theme of the enquiry. While the scale may vary, the context is always restricted: a family, a village, a group of youth, a workshop, a neighbourhood, a police station, an urban block.... Within a given social space, participant observation, in-depth interviews, survey techniques and the search for written data may be combined. At any rate, a basic requirement of participant observation is a relatively prolonged stay within a group, network or setting. Intensity also provides for a continuous intersecting of various sources of information. The restricted and in-depth working context allows the researcher to establish relationships among different types of knowledge and various registers; it also permits a transversal, "holistic" approach in which social actors are apprehended according to the diversity of their roles. In this way religion, family ties, politics, sociability, patron and client networks, production and other such social categories, which cannot be understood simultaneously in an empirical light on a large scale level, can hereby be interrelated, given the proximity to social actors and to their everyday interrelations. Actors continuously "stroll" from one configuration to another. The researcher makes note of personal and "multiplex" intermingling. The interrelation of "spheres" and "levels" of social practice, usually dissembled by analysis, remains a major advantage of field enquiry, even when, and indeed especially when, confronted with a very specialized topic.

The trap, into which many have fallen, is to focus exclusively on this "control group", resulting in the production of nothing but monographs of micro-groups in micro-societies. In fact, moving on to a larger site, on which the stay is counted in days rather than months, seems indispensable. Previous work in the "control group" thus allows one to take full advantage of extensive work by providing an initial context of references. How can one compare without having a base for comparison? In other words : in as much as a prolonged stay with a "control group" appears to be a good research strategy, leaving the group, "stepping aside", appears to be equally important. By getting on the sidelines, by enquiring elsewhere, one is able to obtain new perspectives and make changes, which often confirm as well as complete, invalidate or relativize the picture.

Hence, what the notion of control social group attempts to do is to reclaim one of the assets of yesterday’s ethnographic monograph (prolonged stay among a small group) while avoiding its drawback (imprisonment in the micro level) and adapting it to new contexts, in particular to comparative multi-site enquiries on non-localized or professional groups.

[88] Becker, 1970: 68 and 107.

Lastly, it must be noted that a "control social group" is sometimes comprised of a single strategic group considered to be central. But it usually includes persons from several strategic social groups.

Key informants

The key informant can obviously be considered as an extreme case in which the "control social group " is restricted to a single individual. In many instances, the strategy of recourse to a single individual stems from a culturalist approach which takes a single individual as an expert and the depository of an entire culture. He's supposed to be an "omniscient informant". The culturalist point of view is moreover combined with a lackadaisical research strategy. However, the problem of the key informant goes well beyond the uses to which it is sometimes subjected.

Let's get the matter straight: there is no researcher who does not have a favourite informant. But the preferential recourse to a particular interlocutor can and ought to be combined with the principle of triangulation. It is indeed impossible to do without key informants. This is due to a number of reasons. One is that interpersonal affinities play an important role in field research. Then, because from one research theme to another, from one "problem" to another, local competencies are varied and unequal. Another is that communicational skills vary widely from one researcher to another. For all consultants and all narrators are not equal in terms of volume and quality of information.

It might also be necessary to make a distinction between various types of key informants. Some are generalists and give clear and comprehensive access to customary representations. Others are "brokers", "mediators" or "porters", who open the path leading to others key actors or to almost inaccessible social scenes[89]. Lastly there are the experts in the role of consultant and narrators. Though the criteria of expertise vary from one field to another, each domain or theme still has its experts, seen from the researcher's point of view.

The instances of recourse to a key informant as well the type of key informant selected changes depending on the phase of the enquiry. Finding a mediator, a "broker" or a "resource person" to lean on is definitely necessary at the beginning of an enquiry. Emancipating oneself from this person usually occurs at a later stage.

Managing "biases"

Field enquiry, like enquiry through questionnaires, obviously has its own biases. Fieldwork policy navigates on sight among these biases. But this is unavoidable. The problem is how to control biases. Four examples of this, directly associated with the production of data, can be noted. Biases of a more interpretative type, related to the intellectual posture of the researcher will be addressed elsewhere (*see chapters* V, VI and VII)[90].

[89] See the notion of gatekeeper (Schwartz and Jacobs, 1979: 55).

[90] I will not take "statutory" biases into account here, not that they do not exist, but because their impact is very difficult to evaluate concretely, and varies considerably according to the various facets of the researchers savoir-faire (see chapter V). These statutory biases can refer to the social condition of the researchers (largely underlined by Pierre Bourdieu and his disciples), to their national or ethnic origin (evoked in post-colonial studies), or to gender (highlighted by feminist authors denouncing "a male bias", see Ghasarian, 2002: 17).

The modification of behaviour

This section addresses the inevitable question of the impact of the researcher's presence on the behaviour of persons being studied.

The extent to which observation modifies the phenomena observed is an old and ongoing debate (from Heisenberg to Gadamer). Although there is no consensus about theoretical solutions, some practical solutions do exist.[91]

1. A not insignificant amount of behavioural habits are slightly or not all modified by the anthropologist's presence, and one of the dimensions of the researcher's savoir-faire is to be able to estimate which ones. Becker has pointed out that the researcher is often a negligible constraint or stake as compared to those which weigh daily upon the group[92]. The prolonged presence of the ethnologist is of course the main factor which reduces the disturbance caused by his presence: people get used to him[93].

2. As for the problem posed by those behaviour patterns which are significantly modified by the researcher's presence, there are two radical solutions:

- The first is to attempt to cancel this modification through various procedures, all aimed at eliminating the foreign aspects of the status of observer, getting the researcher assimilated as a native, indistinguishable from others involved in the local game: this gives rise, on the one hand, to an endo-ethnology, or to the training of "indigenous" enquirers [94], or, on the other, to "conversion", "disguise" or "indigenization " of the exogenous researcher[95] .

- The second is to attempt, inversely, to take advantage of the situation: the process of modification thus becomes an object of research. The enquiry takes itself into consideration, as it were, and becomes its own evaluator. In France, Devereux is certainly the first to have attempted to think about " *exploiting the disturbances caused by observation*" in the social sciences[96]. Althabe later insisted on the methodological applications to be derived from the fact that the anthropologist "*figures among the actors of the social field which he studies*"[97]. Using one's own presence as a researcher as a method of observation thus becomes one of the dimensions of anthropological savoir-faire. Reflective analysis hence becomes inevitable (*see* chapter V).

In reality, the attitude adopted is usually mid-way between these two extremes. Little by little, the anthropologist places himself in the position of a "friendly stranger" or that of "a companion along the road", or rather is placed in this position by the "host group". His "integration" is a relative but real. However, it does not free him from observing the effects produced by his presence, including the type of "integration" conferred.

Getting caught in cliques and networks

The integration of a researcher in a society never concerns the entire society, but rather particular groups. The researcher gets integrated into some networks but not into others. This resulting bias is both daunting and inevitable. The researcher always runs the risk of being

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[91] This can be compared to the bias of the cinematographic eye (see de France, 1979) in documentary or ethnographic films: the presence of the camera clearly modifies the attitudes of the persons being filmed, but to varying degrees depending on the theme, duration and immersion of the producer, among other factors.

[92] Becker, 1970: 46-47.

[93] See Agar, 1986 : 39-37; Bouju, 1990 : 157; Schwartz, 1993: 278-279..

[94] See, for example, Labov, 1976.

[95] See, for example, Favret-Saada, 1977.

[96] Devereux, 1980.

[97] Althabe, 1990 : 130.

assimilated, often despite himself, but at times with his compliance, with a certain local "clique" or "faction". This is doubly inconvenient. On the one hand, there is the danger of echoing his adoptive "clique" and repeating its opinions. On the other, he might be rebuffed by other local "cliques ". Getting involved in a clique, by choice, accidentally, or as the result of a strategy of the clique in question, is clearly one of the main problems of field research[98]. The very fact that local actors are bound together in networks makes the anthropologist dependent on the former in the production of data. He easily becomes a captive of one network or another. Recourse to an interpreter, who is always a "key informant", introduces particular forms of "involvement": the researcher is then dependent on the hostilities and affinities of his interpreter, and the affiliations or ostracism to which the status of the former is subjected[99]. For this problem, and for others, there is no miracle solution. However, the know-how, tinkering and other indirect strategies that the researcher uses to prevent getting caught up in cliques are all based on an awareness of the problem[100]. As we can see, methodological lucidity is, once again, the first step in every solution.

Source monopoly

The monopoly a researcher often exercises over the data he has produced, and even over the population among whom he has worked, is undoubtedly a methodological problem peculiar to field enquiry. The access that historians have to the sources of their colleagues and the possibility of revisiting primary sources over and over again stands in dire contrast with the jealous and frequently deliberate solitude of the ethnologist. How can anything approaching critique of sources take place in such a situation?

There are only two solutions to this problem. The first is to have several anthropologists working successively or simultaneously on an identical or on a similar field. We may recall the famous quarrels that have arisen in such situations, Redfield /Lewis or Mead/Freeman, and the multitude of problems arising from the decoding of such divergences[101]. However, confrontation, sometimes direct and differed, between researchers on the same field does not always assume such antagonistic forms. It can also be complementary, and, at times, even convergent.

The second solution is to open at least relative access to one's sources, to the corpuses that one has produced, or to samples of these corpuses, in order to authorize future reinterpretations to be carried out by others. A minimal form of this is to allow the reader to recognize as far as possible who is speaking at each stage of the ethnographic text, by giving to each his due, in view of preventing the "*suspicion of intuitivism* "[102] and the accusation of imposing meaning. It is important to ascertain that the interpretations of the anthropologist remain clearly distinguishable from what his informants say, that sources of description are identified, that indirect style is not allowed to conceal an amalgamation and a concatenation of real enunciators : exemplification and attribution of discourse thus becomes the necessary expression of scientific caution. This brings to mind Malinowski: "*In my estimation, the only ethnographic sources with scientific value are those that allow a sharp distinction between,*

98"*Most scholars who have thought about the methodology of fieldwork warn the ethnographer about identification with only faction of the community he is studying*" (Barnes, 2003, p. 171).

99 See Berreman's very clearly analyzed example, 1962.

100 For example, Erving Goffman, in the face of what he calls the "*the affiliation issue*", gives the following advice: "*You can't move down a social system. You can only move up a social system. So, if you've got to be with a range of people, be with the lowest people first*" (Goffman, 1989: 130).

101 An assessment of controversies of this kind can be found in Shipman, 1988. This also brings to mind van Beek's paper on Marcel Griaule, quoted above.

102 Schwartz, 1993: 284.

on the one hand, the results of the direct study, the data and interpretations provided by the native, and on the other, the deductions made by the author".[103]

Clearly applying such a constraint is easier said than done, and there is no anthropologist or sociologist who does not break this rule. Besides, it has become an epistemological exercise to reread and analyze anthropological classics in order to underline ambiguities in techniques of narration, and in particular the use of indirect speech which makes it impossible to identify the author of each enunciation [104]. But no one, not even the most vigilant critics of other people's approximations, entirely escapes the cloud of ambiguity. Being clear, "as far as possible", about who says what and specifying conditions of data production (interviews as well as observation) are relative rather than absolute guarantees[105]. This makes it even more important to set down a few rules for yourself. There is no way of avoiding Spradley's two "principles": the *language identification principle* and the *verbatim principle*[106].

Representations and representativeness

Inappropriate use of the language of representativeness constitutes another bias. This is the case when the accounts of a number of individuals is presented as a reflection of "a culture", be it the culture of a social class (popular culture, working class culture), that of a people or that of "an ethnic group". Field enquiry generally refers to representations and practices and not to the representativeness of representations and practices. It allows for the description of the space of current or prominent representations and practices in a given social group, without providing any possibility of assertion about their statistical distribution, even though the recourse to intensive micro survey procedures sometimes allow the production of focused exhaustive data and/figures. We should not expect the impossible from field enquiry. It is able to propose a description of the main perceptions or behaviour that major groups of local actors have concerning a given problem, no more, no less. This can also allow for a description of various representational logics or pragmatic (strategic) logics in a given context. No more, no less. It will have nothing to say about the quantified representativeness of these representations or strategies, barring recourse to another methodological configuration.

The subjectivity of the researcher

The researcher's personal role is a resource. As mentioned above, it derives from immersion, progressively opening access to local codes and norms. But this role also entails a bias. Most data is produced during the researcher's interactions with others, by the mobilization of his own subjectivity, through his "staging". These data therefore contain a certain "personal element". Such a bias is inevitable: it should neither be denied (positivist attitude) nor glorified (subjectivist attitude). It can only be controlled, and at times utilized or

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[103] Malinowski, 1963: 59.

[104] See, for example, Borel, 1990 ; Geertz, 1988.

[105] Gretel and Pertti Pelto use the term "*operationalism*" to name this necessary explication of the specific data on which anthropological enunciations repose: "*Strict operationalizing of all field observations would be almost impossible to achieve […]. The need for operationalizing descriptive constructs in research depends on the level of use of particular types of information*" (Pelto and Pelto, 1978: 44).

[106] Spradley, 1979 : 71-73. Let's note, moreover, that conventional signs (quotation marks, parentheses …) help to differentiate between quotes from informants (who are always identified), résumés of what informants say, and the researcher's descriptions and perceptions. Some scholars even propose standardized conventions for note taking. (*See* Kirk and Miller, 1986: 57). Besides, when enquiry is carried out in another language, the publication, in annex or notes, of the transcriptions of quotations in the vernacular is a methodological constraint which is frequently disregarded.

minimized. An attempt will be made (see chapter V) to define certain dimensions of the researcher's implication and the explications he introduces for the benefit if his readers.

This is another one of the functions of the field diary mentioned above: it helps the researcher to manage his subjective impressions. It allows him to evaluate his personal affect, to give an account of the modalities of his personal involvement. Team work, also mentioned above, finds herein another of its advantages. Collaboration and complementarity also serve as a mutual check to subjectivity. This check is clearly relative but nonetheless significant.

Many other problems may be mentioned. The "question of subjectivity" is too complex to be treated here in a systematic manner. It will suffice to point out two adjacent problems.

The first concerns the constant pressures stereotypes and ideologies exert on the anthropologist's outlook (see chapters VI and VII). Now, the anthropologist is far from being the only one to be subjected to these pressures. The same applies to all of the social sciences: they are constantly threatened by misinterpretation and over-interpretation, from the construction of the research theme to the multiple levels of interpretation they implement.

The second adjacent problem has no definitive solution but can be negotiated in practice: everyone, with whom the anthropologist interacts, from the fortuitous interlocutor to the key informant, is constantly playing a game for the benefit of the anthropologist or of someone else. This is therefore a universe of impression management. This is a research path covered by Goffman[107]. This is another common sore point in the social sciences, regardless of the type of data considered.

Conclusion

Plausibility and validity

Diverse contemporary efforts have been made to define the conditions of *validity* in ethnography. They are, by and large, independent of formerly predominant positivist conceptions. The three criteria proposed by Stanjek provide good illustrations: they combine, in their own way, many of the questions mentioned above.

1- To what extent is the anthropologist's theorization grounded in field data presented as "proof " ?

2- Is information given concerning the "field itinerary ", i.e. are we told who informants are and what methods of collecting information were used?

3- Are the interpretative decisions made in the course of fieldwork explicitly mentioned?

I am not sure that this is a question of "criteria" or that criteria can be defined in such a manner. What is certain, however, is that the preoccupation with data validity (another way of defining "qualitative" rigour as an objective) - which I have attempted to outline above - must be placed the heart of fieldwork. To my mind, it is only on this condition that anthropology can claim to be plausible. It is a matter of vouchsafing, as far as possible, the anthropologist's

[107] See the pioneering work of Berreman, 1962.

assertions, based on data produced during enquiry, and of guaranteeing the reliability and validity of these data.

Sometimes, the restitution of the enquiry to subjects interviewed is proposed as a decisive test of validation. Whilst restitution, when possible and relevant, seems desirable and even necessary, this procedure cannot be taken as the ultimate criterion of the evaluation of empirical sufficiency: such a belief is tantamount to forgetting that all social groups are traversed by cleavages (rendering illusory, for the most part, any consensus of interviewed subjects on the results of an enquiry concerning social stakes), and to ignoring the fact that the researcher's perspective is necessarily different from that of the persons he interviews. "*Few people like […] to be objects of anthropological research. In addition, most communities are traversed by conflicts. This makes it impossible to achieve a restitution that satisfies everyone*" (Burawoy, 2003: 441). We cannot but concur with Michael Bloor's conclusion that "*While the previous analysis shows that members' pronouncements on findings cannot be treated as a test of validity, it should also be clear that a member validation exercise can generate material that is highly performant to the researcher's analysis […]. In particular, negative reactions from members should be a stimulus for a reanalysis"* (Bloor, 1983: 172).

In fact, the reactions of interested parties, during *ad hoc* meetings or as members, are always useful. Sometimes, they can make it possible to rectify badly established facts or excessively unilateral interpretations, and provide - by eliminating, as far as possible, misunderstandings about facts, words or meanings - a relative, minimal and partial validation**.** [108].

They can also provide confirmations (even through their disagreements with the author), produce new research materials, open eventually open paths.

But it is on the level of external readers (pairs, students, intellectual public) that the plausibility of a socio-anthropological research mostly comes into question[109].

Plausibility, then, is primarily ensured by what one may call "the final presence of data" in the researcher's written production, over and beyond the use of data as grounds for interpretation, in order to give the reader direct access to a few samples of the sources used. Some data will, in fact, be used more or less as "prime" materials or re-framed, as grounds, arguments or examples validating statements the researcher makes in his final narrative and analytical framework. The sociologist working on the basis of questionnaires "places" his tables and factorial analysis where the anthropologist "places" his interview extracts, descriptions, intensive micro surveys and case studies. These data, resulting from corpuses, derived from field notebooks, are "edited " (as in the case of a film), cut, refitted, produced, according to the researcher's narrative and demonstrative statements [110]. Descriptions are rewritten, sometimes in a very different style from the stenographic notes in the field notebook (and are often saturated with interpretative annotations, in the style of Geertz's "dense description "). Interviews are presented via relatively brief quotations, in translation, estranged from the conditions in which they were spoken. Case studies are contracted, impoverished, sometimes concentrated on a standard form. Their multiple sources are reduced

[108] This corresponds approximately to the term "*validity of meaning*" (Pourtois and Desmet, 1988).

[109] It is true that, increasingly, the members of a group that has been studied are readers of the report that concerns them. (*See* Brettel, 1993). In this case it is possible to make the similar remarks to those made on the topic of restitution.

[110] George Marcus uses the film-editing metaphor (Marcus, 1995a), but in aid of a "postmodern" line of argumentation (which is not mine, as one might well imagine), underlining the artificial character of narrative procedures and the total dissolution of realism. I will not enter this debate which raged during the 80s and 90s, on the subject of ethnographic writing (except on the question of the role of the researcher, *see* chapter V).

and levelled out, their complexity simplified. Yet, in spite of all these constraints, the simultaneous presence of descriptions, quotations, and cases in the final anthropological product (report, article, book) reflect empirical fieldwork, guarantee validity and makes room for criticism. This validity throws back in part to an "anthropological pact", (see chapter I below), which confirms the reader in the idea that the anthropologist did not invent the discourse he relates nor dream up the description he proposes. The "touch of reality" due to the selective use of data produced during fieldwork is not merely a rhetorical stunt. It also bears witness to the empirical ambition of anthropology. It distinguishes empirically founded ethnological interpretation from free hermeneutics, philosophical speculation or essayism. During the writing up phase, traces and evidence of data drawn from fieldwork come to the surface. The reader is not merely gratified with abstract models, he is provided with aids, support, examples or quotations which bring him into closer "contact" with the sense world described. The latter becomes more tangible, making it easier to access to the meaning of the words employed and the scenes experienced.

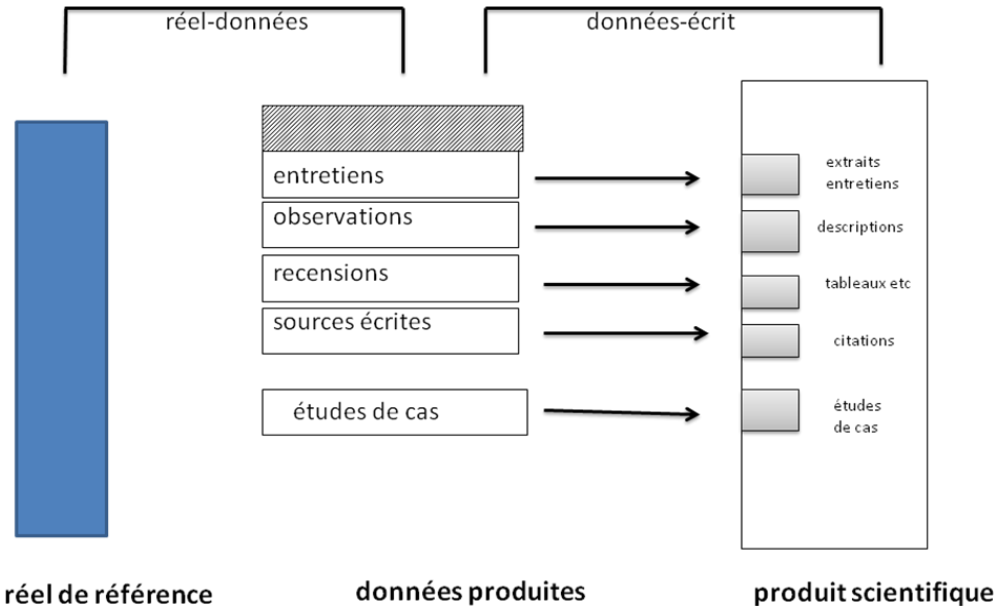

Diagram 4. Traces of data in the final text

Consequently, our concern here is neither seeking pre-interpretative purity in data nor collecting data from an external social reality. Field enquiry is subjected to the same constraints surrounding the construction of the research object as any other social science practice. In the countries of the North and in the countries of the South, in the midst of Western societies and non-Western societies, in country or in town, in factories or in stadiums, fieldwork is regulated by the scientific project of describing, understanding and comparing logics of action and of representations - and their systems of constraints - which do not correspond a priori to the usual norms of the researcher's personal universe, and which he must learn to navigate. This leads to countless misunderstandings. The researcher's know-how, as defined above, ultimately consists in resisting these misunderstandings, and transforming, for his readers, the unknown, the exotic or the picturesque into ordinary and familiar experience. The use of what Geertz calls "concepts close to experience", or to what Glaser and Strauss refer to as "sensitizing concepts" [111] go in the same direction.

[111] Geertz, 1986: 73 ; Glaser and Strauss, 1973: 38.

This familiarization plays a central role and it has thus been affirmed that at the end of his fieldwork the researcher should be able to act like those he has studied if he were in the same situation. "*Comprehension can be displayed in a variety of ways. One classic test that some ethnographers try to pass is: if you think you understand the X then you should be able to act like X. This goal is represented for example in Goodenough's (1957) definition of "culture" as the knowledge necessary to behave appropriately* ". [112] This criterion of "accomplishment" in the ethnographic field is generally accepted: "*I feel deep empathy for the truth of Evans-Pritchard when he claims, in substance, that he is capable of reasoning in the logic of those he studies*" [113]

For the most part, the validity of data collected in the field is related to a "criterion" of this type, on the condition of radically extirpating exotic fantasies. "Modern" research themes, wether in Africa or in Europe, stripped of the irritating and recurring opposition between "Them" and "Us" (people of the South/intellectuals of North), are better equipped than colonial ethnology and its favourite objects when it comes to understanding the logics of others without getting drawn into the picturesque or fascinated glance: an enquiry on corruption requires an understanding of the logics of the corrupted customs officer; an enquiry on the leaders of Trotskyist organizations means managing to penetrate the logic of a revolutionary militant; an enquiry on the World Bank means understanding the logic of a development expert. But we should not forget that, in addition to the exoticism potentially induced by distance, one can also fall victim to be an exoticism potentially derived from proximity.

At any rate, it remains obvious that the "criterion" of familiarization, or of personal comprehension of the logics of others, is in no wise easier to formalize, objectify and quantify than the *data* whose evaluation it is supposed to ensure.

Yet, all enquiries are not equal, all data are not equally valid, all descriptive statements do not have the same veracity, all social logics are not understood with the same finesse, and the plausibility of interpretative assertions also varies according to the quality of the empirical references on which they repose. This is why a policy of fieldwork is required

[112] Agar, 1986: 54.

[113] Augé, 1975: 315 (cited in Fassin, 1990: 100). Evans-Pritchard provided a very cautious formulation: "*To get know the people one studies really well, to see and hear what they do and say*" (quoted *in* Barnes, 2003: 169).