PART FIVE

WRITING AND RESEARCH

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Writing in the Social Sciences:
From Field Notes to Scientific Reports*

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The researcher in the social sciences never stops writing. To size up the activity of writing, which continually accompanies research, is to follow science to the place where it is developing. Writing is far from being a transparent tool,[1] a simple mode of expression, which steps in only at the end of research to make the results public. Given that the sociologist or anthropologist spends the majority of his time writing notes, memos, transcriptions, articles, etc., it is remarkable that he is scarcely taught how to do so, and that he talks very little about it, as if his reports were like ripe fruit falling from the tree.

Nevertheless, some practitioners are interested in the scriptural production in the social sciences. The works of Jack Goody (1979) throw some light on the coextensive character of science and writing. The interest of the famous anthropologist in the invention of the printing press led him to study the concrete modifications associated with the appearance of writing: what are the "powers" of writing which, for him, begin from the list or the table? What are the cognitive effects of these graphic inscriptions? The invention of the printing press appears at the beginning of the birth of a critical tradition: with the printing press, texts began to circulate in a crystallized, fixed form, freed from their authors and from the time of their formulation. No longer was it a matter of forms of knowledge constantly in movement as in the oral tradition, where they are also partially recreated at the same time as they are transmitted, but to a stable reference, provided to view, which can then be compared, examined, criticized: we can write other texts referring to them, and a critical discussion can be developed about them. "Powers of writing" depend on the very possibility of a systematic, reflexive, and cumulative knowledge.

Taking writing seriously encourages us to consider, after Charles Wright Mills (1959) and Howard S. Becker (1986) particularly, that to be trained (or train oneself) in research in social sciences, is to be trained (or train oneself) in the author's craft. The insistence on the concept of craft indicates two things: the researcher produces mainly reports, most often writings (Latour 2006); and writing is *worked on*, it assumes a constant effort, and not a preordered group of ingredients as in a recipe. Paying attention to the question of writing is thus to ensure a constant critical awakening, particular to "the diehard empiricism" (Schwartz 1990) of the social sciences. This does not suggest skepticism but, on the contrary, a concern for rigor and sociological imagination as to our formats of writing, our styles and our vocabularies.

The first official report is, however, more unrefined, and could take away beginners' feelings of guilt: writing does not come easily; few and far between are the sociologists who do not have difficulties with writing. It is often seen as a test, a jump – the "transition to writing"– more than as background work, inscribed in time and in a system of discovery. Can we hope to come to terms with writing? In this contribution, we assemble some main proposals to this end. It is a question of both flushing out the lures which hinder writing, make it intimidating and discouraging, and of making a list of "tricks of writing" already explored by researchers that the reader could then test and enrich in turn.

Some False Ideas on the Subject of Writing

These ideas consist in thinking of writing as the simple putting down in words of a thought which is already clear, or results of research developed previously. Writing is thought to be outside of the core of scientific activity, and its qualities are of little importance, no more than they are cultivated. Among the myths that harm our practices of writing, we find both that of writing as a "personal talent" or of "easy writing", the belief in the existence of a "single good way to write" that the "best researchers" are thought to possess and that has only to be discovered. In all cases, it is the very nature of writing as work which is misunderstood.

The myth of writing as an innate or superfluous ability

If "everyone knows that sociologists write very poorly" (Becker 1986), that they often multiply unnecessarily complicated terms, long and bombastic sentences, we could infer that the talent of writing is not among the professional abilities normally required of sociologists. Writing is said to be only a "plus", a supplement to "style", principally ornamental, which some are lucky enough to have a talent for and others not. The context of the text does not depend on its form, as if the core of the research was over before any reproduction in the form of a report.

There is a double misunderstanding here. On the one hand, respect for the norms of writing most often plays an important role in practice in the way that editorial committees of reviews assess articles. On the other hand, this technical

and differentiated language, this "academic prose" that beginners perceive as the very mark of the "science", can be the subject of training. Beginning researchers do not hesitate to imitate this style, thereby giving credit to the idea that there are not other legitimate ways to write, to "do science", than this academic p(r)ose. And if it is important, in particular to publish, to master all the variations of the academic style – each review, each university setting having its own requirements and formats (number of characters, text format, expository mode, etc.) – it is also important not to become obsessed with it, nor become a prisoner of it, especially at the first stages of research. And even, the apparent absence of style can resemble a form of style. Finally, in reducing ethnographic writing too much to that of a final report, we run the risk of losing sight of the multitude of writings that came before this document. However, these minor documents are not only "intermediaries", to repeat a distinction made by B. Latour (2006), i.e. *inputs*, but especially "mediators", i.e. elements which direct the text which will result in an often unexpected way.

The myth of "easy writing" for others

Here, H. S. Becker attempts to set us straight: nothing is more common, among researchers, beginners, students, as well as senior researchers, than the anguish of the blank page. The final draft, in particular, can be hindered by the consciousness of the issues of a text which will reveal to the external world the existence of a work. A part of the problem of scientific writing thus comes from the very institutions of intellectual life and of their community of judgments. To complete a text is to both "congeal" a reality and to open oneself up to criticism, commentary, and suspicion. The regulation of scientific production assumes this peer review: to render an account of the development of one's research is to allow oneself to be told that it could have been done otherwise. Indeed, H. S. Becker states and criticizes the multiple strategies of production currently adopted by researchers to protect themselves against such judgments: beyond the "blank page" or incomplete text, they can still hide behind wording which is excessively cautious, alembic, abstract; and they often have to increase the number of little reassuring rituals to be able to start writing.[2]

Among these strategies of "protection", we also see the various ways of claiming a form of authority. A part of our writing strategies are thus a quasi-ritualistic way of "telling our readers who we are and why we should be believed… ". Among the "persona", the most frequently claimed, is to point the authority of experience (I was there and I saw is the classic authority of the anthropologist Malinowski), the one who argues from an ability of an initiate (profusion of scholarship, excessive accumulation of scholarly details), or the one which intimates, in and by the text, that no matter who would have seen as much.[3] If it is important to deconstruct these classical figures of ethnographic authority, it is not so much to show the scientific fragility of the scientific report

as its artificiality: writing is not at all natural, is rarely given, and implies the deployment of efforts, of reasoning, of strategies which, far from weakening the argument, instead extends its reach.

If the student, tempted by mimetism, often aspires to slip into one of the *persona* offered at a given time in his academic space, the researcher is often tempted by "representation"; his stylistic choices, more ritualistic than semantic, underline an allegiance to such or such "trend" or "clique", and mark the adoption of a pre-written role in academic space. The desire to say "I am a functionalist" can, for example, prevail over that of saying what one really wants to mean. The spread of these idiosyncrasies, which sometimes thwart the requirement of promotion of scientific writing, shows once again that writing is not generally an easy activity.

The myth of a "good way" of writing

Another erroneous idea would have it that, in writing, there are models or rules to follow which can allow us to avoid all difficulties. The most pernicious model, because we often follow it without even realizing that we are doing is, is that of the academic exercise, the "written test". This idea would have us believe that the best technique, that of both the brilliant author and the good student, consists in writing quickly, at one sitting, in one go. Any starting over on the text, any "crossing out," is thought to be a sign of "cheating" or imperfection. Our academic habits, the result of serious training, pushes us to disdain rewriting which is, however, essential to scientific writing. The research thus becomes an often disconcerting activity for the beginner: it requires a whole other training, and accepts assessment criteria which are quite different from those of academic exercises. A "too perfect" work, in which we see only the endpoint, could, for example, be judged less favorably than a work which shows the reader its development, the questions which remain, the mistakes, misunderstandings or prejudices which were removed, the redirection that was necessary, etc.

The influence of the academic "model" certainly plays a role in the proof of personal worth. By making academic papers a proof of personal worth, the school system has tended to transform writing into an acid test which determines whether we are a winner or a loser. Schools do not schedule such papers as a fun or practical exercise, but as a public and irrevocable test of identity and personal value; this is all we need to encourage blocks or procrastination which constantly puts off the test. As Clifford Geertz reminds us, the most virulent objection, widespread in practically all contemporary intellectual milieus, attests that "stressing the way in which assertions of knowledge are presented" is to harm our ability to take its assertions seriously (Geertz, 1996, p. 10). Dismantling finely wrought texts would amount to accepting the relativism of knowledge. According to Geertz, on the contrary, it is extremely important to understand how the texts are made and readers convinced in order to understand "the criteria enabling us to assess

them" (Ibid, p. 14). Deconstructing ethnographic writing would enable us to better objectivize our judgments and knowledge, to better relate to them, and indeed to encourage communication between peoples.[4]

Training in research in the social sciences is then that of a totally different relationship to writing: writing as constant work, and the work of an artisan more than of an artist. To establish the importance of writing, we then need to go further: not only to refute false representations, but to transform its concrete relationship to writing: try it out as the daily place of a system of discovery.

Writing as Work: The Researcher's Workshop and the System of Discovery

In observing the researcher in social sciences, we cannot but note the multitude of writings which surround him: scientific articles which he writes are part of a vast group of texts – index cards, notebooks, and notes of all sorts – to which we can also add writings of colleagues, classic texts, periodicals, etc. The researcher in social sciences has everything to gain by seeing himself as a professional when it comes to writing, just as much as in the production of ideas or research.

Write, always write: Why keep notebooks, index cards, memos?

To underscore the place of writing, there is nothing better than to increase the number of "little writings", seemingly minor or without a particular issue. In the well-known appendix to *L'imagination sociologique [The Sociological Imagination]*, "Le métier d'intellectuel" ["The Profession of Intellectual"], C. W. Mills recommends all-out writing of index cards: "index cards are to the sociologist what notebooks are to a writer. They are indispensable"(Mills 1967:200). First, let us clarify that index cards – which can also be notepads, notebooks, electronic files, etc. – are the best way to get into the habit of writing: "you run the risk of getting rusty if you do not write at least once every week". Accumulating index cards means that you are practicing writing, gaining ease, making the writing a daily experience and not something exceptional. C. W. Mills suggest that the intellectual artisan subdivide his index cards into ideas, personal notes, reading notes, bibliography, proposals, etc. Keeping them up to date, consulting and sorting them regularly also "maintains an internal alert" that encourages "catching an understanding of ideas, observations, words" which, "once set down on paper, can be useful for more reflective thought", and maintain "free attention" (I. Joseph) which lends itself to seeing new things. Their manipulation helps us on a daily basis to get out of our mental routines, by showing up unexpected connections, "unsuspected relationships" between notations made at different times. This gives room for the effectiveness of the written word. Just as photographers would not walk around for anything in the world without their camera, fearing that they might miss a successful shot, sociologists should not go out without a pen and notebook stuck in their bag!

C. W. Mills stresses in particular the importance of reading notes and advises us to get used to taking copious notes each time we read a work or an article. This is to save time and energy – we can refer back to them more easily later – for analysis. Note-taking, but also underlining, commenting, or summarizing in the margins, etc., encourages active reading, cultivating a suspicious, indeed disrespectful relationship to the text. A book in the social sciences does not read like a novel. At the same time that the reader tries to relink the questioning of the author to his own and assess his contributions, he puts him to the test by reflecting on his flaws, on criticism that can be addressed to him, questions that he leaves open and research that they assume, etc. He can read only some passages that he re-reads many times without necessarily going through a linear or complete reading of the work. But he never limits himself to some quotations. To read is, on the contrary, *to re-read* and *link together*. To this end, we always read and give priority to the introduction and the conclusion, but also the table of contents and the index to understand the logic of the whole subject and its "overarching idea", and the bibliography to clarify the intellectual universe of the work, and to complete its list of books to "skim" or "read." Finally, we should always read the footnotes meticulously; they are very useful for understanding the most subtle aspects of the position of the author.

Defenders of grounded theory consider writing and the frequent consulta-tion of memos, with analysis based on field work inscribed from the beginning of the research process, to be indispensable to discovery. The comparison of memos, their blacklisting, already recommended by W. F. Whyte in his postface to *Street Corner Society* (1955), enables us to distinguish categories under which observations will be subsumed and then linked. Memos can, furthermore, make up a system of communication and coordination between members of a research team, today all the easier with the widespread use of the Internet, allowing for exchanging ideas at a distance.

More generally, these multiple little bits of writing, and their frequent re-reading, bring to daily work the miracle of the "good issue" or the "good subject", that students otherwise search for eternally in the heaven of ideas. Keeping index cards or notebooks promotes the development of fields, subjects, problems, while allowing daily experiences to fuel research as well by hybridizing with other notations. The metaphysical question, "how do ideas come?" then becomes: "how to encourage and catch ideas?" It is with index cards that the sociological imagi-nation is cultivated, which distinguishes, according to C. W. Mills, the "good worker" from the technician who is "too educated". A veritable training in agility and sociological curiosity, index cards train us to change perspective, to diversify points of view, and not to give priority to sorting out thought on the extent of one's questioning. They teach us to think by successive strokes and not all in one piece and all at once.

Field notes: Why "note everything"?

To students who worry about the right way to deal with their field journal – What amount of detail? What amount of analysis? – H. S. Becker answers by an instruction that appears to be puzzling at first: "note everything" (1998, pp. 131-146). But can we note everything? Obviously not, the sociologist immediately assures us. Far from the fantasy of an "entirely complete description" or exhaustivity in data collection, he only sees in this the regulating idea, a "sensitizing concept" H. Blumer would say. One thing is certain: it is possible to note many more things than we do spontaneously and this will always be more exact than a description that leaves lots of things aside." Otherwise, we can always tell ourselves, after the fact that we could and should have taken more notes, that we miss some things that escaped when we had them at hand. This is what happened, for example, to W. F. Whyte (1943), after several months of research in Cornerville, when he realized, during a bowling game, that the core of his observations was the organization of gangs, and the neighborhood on the whole, and that he had unfortunately not kept any written record. It is also this which encouraged M. Duneier (1999) to extend his analysis of newspaper vendors to the larger world of the sidewalks of Greenwich Village and start his field work again from scratch, after his main informant, H. Hasan, read a first draft of his study, and pointed out to him that it was too focused on his own idea of the vendor. M. Duneier then completely re-started his research by taking the newspaper stand of H. Hasan, who turned it over to him in his absence, as a post for direct observation and recording. Like others, M. Duneier indicates the advantage of recording – in writing, by audio or video tape – at the time, in the heat of the interaction, or as little time as possible afterwards, the greatest number of elements of what is going on.

The researcher is most often ambivalent with respect to writing. On the one hand, it marks his marginality on the social scenes studied – the situation of the *outsider*, who watches and listens *in order to* write, often being synonymous with inconvenience, isolation, even alienation. On the other hand, it allows him to find himself in this role of researcher, in relation with the academic community. After the stimulation and fatigue of the field, it is in most cases a burden for the most part. Thus, in the field, it is important to find ploys to get away for a few monuments and take down *a minima* some keys words for the record, as well as a maximum number of expressions, terms and exchanges with natives (that we will carefully note in quotes); taking a sample of selected items (photos, documents, etc.) will also help in the exercise of memory by enabling us to "re-see" the scene. The rapidity with which the notes are completed, commented on, and clarified (at the beginning, counting on an hour of work in the office for an hour in the field) will determine their richness, their specificity and their accuracy because, obviously, the longer we put off writing, the less and less well we describe. D. Cefaï, in his courses on training on field research, speaks of the "memory like a

sieve". At the beginning of field work, abundant and detailed notes on the ways of speaking and doing, the way in which one is welcomed and treated, as well as the places, sounds, colors, objects movements, atmospheres, etc. are all the more valuable because the intensity will become dull with familiarity. Next, empathy often takes over. It will then enable us to perceive what is significant or important in such or such a situation for those being questioned: What are they attentive to? What makes them react strongly? What are they looking at? What are they talking about? What constitutes an "incident", "trouble"or "problem"? What do they do about it? How do they assess or comment on what has happened talking to the observer? etc.

In order to show students the usefulness of the advice to note everything, and initiate them in to note-taking, we can show an ethnographic film and ask them to "note everything", and then compare what each one has actually noted. It is always a big surprise for them to discover the extent and diversity of what can be noted. The exercise also allows us to show the relevance of accompanying the raw description with more personal notes (reactions, emotions, association of ideas, links with academic reference or others, etc.) and, on the other extreme, of thought about the approach and the pre-conceptions of the film director and/or ethnographer.

This trick again allows us not to give in to panic when confronted with two possible experiences of the research: one being to consider what one observes as much weaker than expected; the other being to falter under the over-abundance of the real. In one case, noting down everything can bring out motives that had gone unnoticed, which gradually sketch out a framework in the research. In the other case, noting down everything can bring out motives interwoven in a denser fabric and concentrate attention on a particular point, whereas the time is often limited and the centrifugal relevance of the world observed can engender a certain paralysis.

But "noting everything" is also noting otherwise. H. S. Becker draws our attention to the spontaneous tendency of beginning researchers to substitute "analytical summaries" of what has been observed, already rich in implicit interpretations (and therefore conventional): "what they believe to be raw descriptions are usually nothing of the sort, but rather sorts of analytical summaries of what they have seen, summaries developed to avoid the requirement of noting everything" (Becker 1986: 132). A global judgment, "it was sad", for example, will replace a detailed description of the situation. An attempt at specifying the cause of an emotion will, likewise, short-circuit the description of its appearances, reactions it evokes in the participants, etc. To harvest rich data, the sociologist has everything to gain, as in conducting interviews, by giving priority to the "how" over the "why" , and by systematically re-translating his or her research questions in descriptive and not explicative terms: not "why does this group exist?" but "how was it formed?"

Asking students to "take note of everything" thus trains them in the art of enumerating without commenting, without trying to infer motives or intentions of persons, thus the flip side of ordinary experience. We are not accrediting here the myth of a "pure description", but observing that there are descriptions less interpretive than others and that these simpler, less analyzed observations teach us much more. There are several ways to take notes in the field, some in first person, giving free reign to emotional flow, evaluative scanning, and an exacerbated reflexivity, at the risk sometimes of making the researcher the main subject of the research.[5] The instruction suggesting that we take note of everything is not aimed at reducing this variety of approaches, but rather indicating that the real is always richer and more amazing than we suspect, and that our propensity for curiosity can be too quickly stifled by the too rapid insinuation of interpretations and analyses which format our perception without our knowing, and in an often definitive way. To protect ourselves against an excess of interpretation on notes in the field, we can, for example, maintain a strict typographic separation of what is part of description and what is part of interpretation, by only writing one or two pages of notes in the field, leaving space opposite free for all sorts of wild imaginings that one doesn't want to either forget or lose on the fly – and which will prove to be extremely valuable later on for some! (Beaud and Weber 1998).

"Noting everything", subjecting ourselves to "massively detailed descriptions" gives us, on the contrary, the possibility of being surprised by one's data and of forming less conventional interpretations: "a meticulous description of details which does not go through the filter of our ideas and theories produces observations which do not tally with these categories, and which thereby require us to develop new ideas and new categories that they could be integrated into without forcing"(*ibid:* 146). Conversely, without this effort, the risk is great that the researcher will only renew his prior schemes of perception of the social world, whether they be scholarly or indigenous: "We see things which we already have ideas about, and we cannot see things for the description of which we have no word or no idea" (*ibid*: 48).

This effort to push back the interpretive moment is among the most difficult: nothing is more reassuring than to interpret, to immediately associate what we observe to sociological readings, at the risk of turning oneself into an "observer or ethnographer going by the book". First, this avoids the unsettling effect of surprise (we see what we came to look for; we find what we already knew); and then, we immediately have the impression of doing science (that "seems" sociological)": "sociologists expect interpretations from themselves and their colleagues. In general, they want to reduce the quantity of material that they will have to handle, by seeing in it illustrations or proofs of ideas that they have, and not something that has to be displayed in large quantity for its particular interest," H. S. Becker laments on this point. If the over-interpretation (Lahire 1996) or

imposition of the problem (Duneier 2006) are the main pitfalls of field work against which the researcher should steel himself the most, all the "tricks" that H. S. Becker provides are designed precisely to support this effort.

"Note everything" is one of these tricks which, by encouraging us to produce richer data, should promote the invention of "new ways of manipulating things"– questions, possibilities of comparison, categories, etc. – they alone able to advance knowledge. The same is true of the trick which suggests that we "see people as activities" and not only to "type" them: "putting people in a category is a way of rendering an account of the regularity of their acts (...) by concentrating on activities rather than people, we force ourselves to be interested in change rather than stability, in the concept of process more than that of structure" (*ibid*: 86). This allows for richer descriptions, but we also avoid immediately limiting the field of observables: "starting with activities allows us to focus the analysis on the situation in which such an activity takes place and on all the connections that your subject of study maintains with the things that surround him, i.e. with his context".

In *Qu'est-ce que la sociologie? [What Is Sociology?]* (1970), Norbert Elias formulates an additional trick: a chain of personal pronouns. Because they are present in all human groups, this reminds us that we cannot represent an "I" without a "you", a "he", a "we", etc. To produce rich descriptions, we need to be attentive to the network of interrelations which weave every individual into a "man among men" and not as "*homo clausus*". This trick involves systematically multiplying the points of view. In the same vein, Georg Simmel suggests that we no longer see the ego in the middle of concentric circles, but an intersection of innumerable social influences. In both cases, the challenge is to develop relational concepts. The metaphors proposed by N. Elias (the configuration of players, the weft of a fabric, etc.) is thus designed to help us to thwart routines of thought inscribed in the vocabulary and syntax of languages marked by Aristotelian categories (matter/form, substance/attributes). These encourage us not to only develop static, reified subjects, and not to only give a secondary status to movement and relation. In "the river flows", the flowing is external, as added to the inert river. We assess the arbitrary and the perverse effects of such a mode of representation. On the contrary, the researcher should, as much as possible, multiply and precisely define the various perspectives and wonder who smells, says, thinks, judges what, according to such or such inscription in a configuration of actions. To render an account of the intrinsic dynamic and relational character of social life is thus a constant challenge for the researcher, who confronts this in and by writing.

Attention to writing is at the core an effort at detachment – which N. Elias evokes through the image of the spiral staircase, which we see ourselves going up (1970). From index cards and field notes to scientific articles, watching ourselves in the process of writing allows us to become aware of choices, implicit preconceptions (with whom did I identify the most easily? What did I pay attention to?), to examine their limits, and to counteract them if necessary. We should

remember that we can take notes without making hypotheses not only on what is important to note, i.e. on what will interest future readers, but also on what is significant, important for the subjects of the research themselves. Clarifying these hypotheses, by re-reading field notes with a critical eye, means that we are erecting a rampart against ethnocentrism or sociologism. It will allow us to return to the field with increased curiosity, and to no longer shut ourselves up in a pre-constructed problem – the temptation of the sociologist being, for example, not to pay attention to the effects of social hierarchy or "symbolic" dimensions of activities studied. Thus, we understand that writing is always re-writing.

Writing is re-writing: Why multiply the versions and let them be read?

Writing is the workshop of the social science researcher, the place where he becomes aware, during all of his research, of problems to resolve or resolved. He runs into choices, implicit or explicit, sees what he has at his disposal, develops and experiments with descriptions, puts his ideas in order and works on their (re)formulation. If it is important to begin to write reports on his research as soon as possible,[6] it is because writing has a major role in discovery. Contrary to what is often believed, we never know in advance what we are going to write, what will finally come out of our confrontation with writing. Bruno Latour reminds us of this: "In our discipline, the text is not a story, a beautiful story; it is the functional equivalent of the laboratory. It is where we do tests, experiments and simulations" (2006). Training in the craft of researcher is thus in the trade of author, producer of "dangerous reports": "Not teaching doctoral students to write their dissertation is like not teaching chemists to do experiments. This is why, from now on, I do not learn, anything else but writing (it's true, I end up giving myself the impression of being an old idiot who always repeats the same thing: "describe, write, describe, write") "(*ibid*).

As a result, writing is above all re-writing. In fact, all problems are not resolved at once, starting with the first version; the first sketch is never the good one. To say that rewriting is the true work of writing is to also say that there is cause to look for the "right starting point", the right way to begin, which often paralyzes us, faced with a blank page or the screen. The writing workshops that H.S. Becker organized for his students were aimed precisely at showing them, starting from their own texts, that the issue of the continuous work of re-writing is the very development of their thought: not a concern for elegance, a supplement of "style", but the way to find what we want to say, to develop our argumentation, to test it and hone it, and be able to express it in a clearer, more concise and convincing way, by avoiding useless words or inferring "false profundity". These workshops allow for experimentation with the unsuspected quantity of work that rewriting and correcting a text require, and the fact that this massive correction of "details" is worth the effort. In economics, the "o-ring" theory (Kremer 1993) attests to the utility – particularly for the quality of the finished product – of calling on

highly-qualified labor at all stages of the production process, including some tasks of minor monitoring, but the elimination of which can have catastrophic effects on the consumer and, in turn, on the producer.[7] Likewise, without extreme vigilance exercised over each detail of the text, the credibility and verisimilitude,[8] the quality of the report is threatened. The soundness of an article sometimes depends on very little things – for example, a simple description!

The idea is valid also, before the correction of the first analyses, for the work on the field notes. Upon a close reading of them, questions of observations appear which lead us to re-do the field research to complement our corpus, whereas we thought that this part of the job was finished. Although field notebooks are very often private objects that we do not like to show because they contain research like a private life, and also rough, primitive notations that we think are not worthwhile. They pay off, however, by being discussed on the actual evidence. The workshops on reading field notes by N. Eliasoph and P. Lichterman in Los Angeles, and those of A. Cottereau and S. Baciocchi, or C. Gayet in Paris, bring together students and researchers on rough re-transcription of observation sessions. They prove to be time well spent to move the research forward, and discover heretofore unexplored, but perceptible paths between the lines, so long as we agree to have colleagues come to the workshop.

This "cooperative" dimension (Joseph 2007) of research is basically epistemological. Research involves interacting with an environment which holds answers in order to continue to act (Dewey 1993) and there is a certain masochism in depriving oneself of the entourage of close friends and colleagues who have agreed to read you and comment, whether it be on the weak stylistic quality of your notes, or a proprietary concern. On the one hand, forgetting that the heuristic character of notes depends much less on their formatting than on their descriptive precision (who is speaking? when? where? with whom? about what? from which empirical viewpoint am I speaking as I am reporting?). On the other hand, it is naïve to believe that the collective discussion of a work in progress undermines the authority of the researcher. Strictly speaking, he remains well and truly the author of texts to appear (there is little chance that his readers will agree to take on in his stead the "dirty work" of writing, according to the still frequent prejudice!). And especially, a text never exists except as interwoven in other texts and discussions that he cites, comments on, summarizes, critiques, as so much expressive support, as Jorge Luis Borges or Gérard Genette had already shown long ago. In short, there is a certain value in considering the discussion of our materials as just one point among many in the research, and not as a test of style or a burglary.

The argument is simple: to write is to think, by versions and variations. We cannot think "all at once": thinking is a dialogue with the data, multiple writings, colleagues and oneself. Like any creative act, it assumes a duration; there is a temporality which is peculiar to writing, where downtime is as important as periods when we write our three pages of text per day. The work of writing,

with the concentration that it requires, the settling that should often precede it, does not follow a linear temporality. Sometimes we write a lot, and other times not at all. Furthermore, it is good to let a work that is almost finished "sit" and then take it up again with a fresh look after several days or weeks. Finally, we should once again stress the interweaving between reading and writing: all re-writing assumes re-reading. No successive versions without constant re-reading, by the researcher himself – who is obviously its first re-reader – and also by trusted readers to whom he does not hesitate to entrust his first drafts. C. W. Mills thus stresses the value of "presentation contexts": presenting our ideas to others, in the framework of informal discussion, research workshops or seminars requires us to specify them, clarify them and make them as accessible as possible. The reading by someone else and re-reading are worthwhile as chances for testing, control and critique (in the Kantian sense, of work on the limits of validity) of the subject. A "presentation context" thus always tends to turn into a "new context for discovery".

For all that, even before the intervention of "re-readers", writing is thus a collective activity, part of a framework of discussions. B. Latour illustrates this quite well in describing this researcher specializing in soil analysis who, in the middle of the Amazon forest, carries his dear colleagues "in his stomach": the importance of the way in which he is going to make the holes cannot be understood outside the imaginary – but no less real – dialogue held with colleagues thousands of kilometers away (Latour 1995:51).

Some "Tricks" of the Writer's Craft

By outlining his "tricks" of writing, H. S. Becker does not intend to state "rules to consciously follow", but to encourage discussion and experimentation of each person on fruitful practices of writing. His "tricks" call for a pursuit of the inventory of techniques developed by researchers and a more discussion than we do on the modalities and strategies of scientific writing, of its "styles", which are still today rarely debated, regardless of the country considered. The question is, however, essential: the public character – debatable and shareable – of our productions depends on our ways of writing, presenting arguments, showing, illustrating, within the scientific community, and also in society itself. Can we really convince without showing the reader specific data and not simple "illustrative vignettes"? How can the causal inference be based on what J. Katz calls "luminous descriptions"? What are the formats and formulas through which we can initiate discussions and make our proofs tangible?

Classify, sort, analyze notes

We would like to give some tricks here for dealing with the possible over-abundance of writing of all kinds (field notes, log notes, memos, etc.), which are not yet final reports of field work. We are not concerned here with the style of notation, but rather classification strategies.

For the moment, we have tried to de-naturalize writing, and present it as an activity necessary at all times in research (and not only in production of the final report), as a fastidious activity (in the sense that we should " note everything "), but also as an activity which depends on the knowledge produced (because the text is a laboratory). We have particularly stressed the primordial importance of field notes. We have thus suggested that you write, again and always, at the risk of giving in to feelings of frustration, distress, even exhaustion, faced with the quantity of materials amassed throughout the chain of writing which makes up sociological work. What then do we do with the mass of data collected? What do we do with all these notes which obviously do not all allow us to completely answer the very questions that we ask ourselves? How do we read, sort, classify, and analyze? We will quickly present some classic suggestions which share a common analytic, inductive approach, taking the greatest care to very precisely link the analyses to graphic inscriptions from field work.

The main part of the subject is dedicated to grounded theory, a trend embodied in particular by A. Strauss and B. Glaser (1967),[9] which played a fundamental role in the United States, starting in the 1960s in the epistemological and methodological legitimization of qualitative sociology, in an academic world dominated at the time by quantitative survey research.

While leaving aside the very moment of writing and note-taking, grounded theory provides very fruitful leads for extricating oneself successfully from the corpus of notations from field work. From a general standpoint, it rejects all hypothetical-deductive approaches of field work and instead sets forth an abductive approach where production, analysis and theorization of data alternate, complement one another and deepen mutually". "There are not two phases", write A. Strauss and J. Corbin (1990:365), "one of formulation, the other of falsification of hypotheses, but a double movement of generation of codes, categories, of their properties and their relations in data analysis, and simultaneously, the testing of their validity, elimination of negative cases, modification of qualitative samples, controlled refinement of identities and differences up to the point of saturation."

Grounded theory has, in particular, further perfected procedures of data coding, including field notes. It distinguishes several phases of coding. First, it recommends an analytical reading and dissection of the corpus, line by line, according to categories the production of which is done, obviously, by groping about, as we better define what precisely we wish to study. Then the researcher is invited to return to the field and formulate "generative and comparative questions which expand the research guide of the researcher on his investigation sites" (ibid: 373), and to specify as result and by comparison the analytical categories discovered before. This operation is called "open coding", opposed to "axial coding" and "selective coding". "Axial coding" consists of better understanding the existing relationships between the various categories of analysis of the action studied. It is particularly

concerned with distinguishing contexts of emergence and behavior of certain phenomena and questioning their relationship. Finally, "selective coding" is "the process by which *all* categories are unified around central categories", whereas the categories which require further explanation are fleshed out with descriptive details"(p. 375). Coding is complete when data production and analysis no longer lead to discoveries and reach a "saturation" point.

This is an approach which claims, in perhaps a bit naive way (Burawoy 1998), to be largely inductive, to generate knowledge from the field. The theory – what must be explained and the way in which to explain it – emerges in the field and does not pre-date it; only incremental production and analysis of the corpus give it form. Even among its partisans, many criticize grounded theory as not very defendable scientism, but recognize that it has the merit of having opened up reflection on the tools, stages, procedures of production of knowledge in a qualitative approach.

In a fairly close vein to grounded theory, representatives of analytic induction give some useful tips to make sense of the mass of materials accumulated in the course of research.[10] They consider that work on the data consists of a permanent re-definition of the question of research and explicative elements, until a locking of "theory" is accomplished, meaning that all cases to be explained are done so by the same explanation. In other words, the analysis of materials is a re-adjustment, not only of the importance of the activity, the phenomena, or the fact to be explained, but also of explicative factors, as the analysis discovers negative cases, i.e. not explicable in the envisaged way, or the consideration of which leads us to re-define the spectrum of experiences to study. The driving force of analytic induction is the discovery of negative cases in the corpus. These cases resist the explanation, which up until then explained other cases, without, however, leaving the field of study. Analytic induction is particularly useful for describing careers and their stages, i.e. the processes without which the observed phenomenon happens.[11]

Thus, grounded theory, as analytical induction, gives us leads for giving meaning and order to the mass of data accumulated during field work. Once these data are coded, and "explanatory theories" formulated, it is then necessary to render an account of these new analytical proposals, by producing descriptions which will shed light on the causal inferences detected in the analysis of the corpus.

How to write the argument – between description and explanation?

Once the notes have been reviewed, the memos sealed and the materials analyzed, writing the report is facilitated, but still sprinkled with tests that we can re-group under this question; how to write the argument? Counter-intuitively, perhaps, but in accordance with all the work on the data previously completed, we must once again trust the description, much more than the explanation. When great sociological problems overpower the author, the best thing to do is probably to refer to

descriptions contained in notes, and selected during their analysis, rather than to call on explanations which short-circuit the field and which reach out to each all the more, in that our knowledge of the field is important. In other words, as B. Latour (2006) suggests, with respect to the descriptive and political point of view, we should again slow down on the path of explanation when we see it emerging on the horizon.

As long as possible, we should strictly connect our inferences to detailed descriptions. These descriptions are obviously not verifiable, but they will be all the more realistic and credible, as M. Hammersley (1993) stressed, as they are explicitly situated within a corpus, positioned within an argument, and make public their production constraints. Thus, their quality will depend largely on their fallibility.

To fight against the blank-page syndrome, several authors recommend mobilizing the resources of rhetoric, in a perspective which is no less realistic. We must suspend the demand for an explanation (the why?) by making the description (the how?) intriguing. In a series of recent papers, J. Katz suggests formulating a certain number of tricks so that we "[qualify descriptions] as "revealing", "colorful", "vivid", "poignant", or "strategic", "of great richness", "dense in texture", or "finely qualified". They are developed because they show how conducts are "crafted", "anchored" and "situated" or because they contain "paradoxes" or "enigmas" which fascinate both the researcher and the reader" (Katz 2001). The author thus distinguishes seven sets of assessment criteria for ethnographic reports. We will discuss only one in detail, in which the description is meant to respond to an enigma, a paradox, or an absurdity.

This is an ordinary way of questioning the world and reproducing the consistency of it, because what is at play, in the absurdity, enigma or paradox, is indeed that things hold despite all that! It is an issue also of overcoming empirical obstacles and making them descriptive domains that stimulate the curiosity of the reader, for "if the sense of mystery is not peculiar to the ethnographer, he can hope that his final explanation will also be of interest for a broader public" (ibid).

If these forms of interpolation threaten all researchers in the field, they do not always crop up suddenly during research. We must open to surprise in cultivating this floating attention which we mentioned above, and perhaps also in asking this question on a regular basis: "What's missing in this picture?" This question is worthwhile as a variation on E. C. Hughes' trick, repeated by H. S. Becker, suggesting that we consider that anything is possible (Becker 1998:148-150). We must precisely force ourselves to question what is given, in order not to lack important and decisive information to understand what is happening.

By way of example, take this illustration from the first field research that one of us did. The work dealt with the use of public space by the homeless in the neighborhood of Les Halles in Paris. The idea was to question the preconceived notions of the homeless as crazy or totally dissocialized people. In a Goffmanian

vain, it was interesting to describe the various scenes of homelessness in Les Halles, a neighborhood which is richly endowed with shelters and distribution of material goods for the homeless, particularly dense and favorable for providing ways to get by, including several stories, corners and niches favorable to quasi-private uses of space. The issue was to understand the variety of roles played by the homeless and to tie together the stages where these roles were played. Rules, norms, values should certainly govern the relations between the homeless and their use of space.

After several months in the field, at the very moment of leaving the research site, and after reading Tricks, the ethnographic apprentice asked himself if, basically, nothing was missing in his observations. Then, suddenly a striking void appeared: there were no beggars in a neighborhood where the possibilities for gain seem to be numerous and accessible! More specifically, the people met in Les Halles – several dozen homeless who spent a good part of their time there – never begged in the neighborhood. This enigmatic observation re-opened the field research. In notes, we saw signs of beggars but they were unknown to the homeless being studied: "Romanians" who came to beg for a few hours each day, said themselves, that they would never do this in their neighborhood. We had put our finger on a moral code (Anderson 1999), which proscribed certain behaviors and established forms of mutual recognition between the homeless, but also with the people of the neighborhood, particularly merchants and residents. We had discovered a descriptive domain which allowed us to draw a deeper picture of Les Halles.

Description thus appears as the most certain preliminary re(source) of the ethnographer. The ethnographic argument will be all the stronger in that it will be based on valid descriptions, according to criteria that certain ethnographers are starting to show today. Short-circuiting the description, in favor of explanations that are too quick, too cursory, too staggered vis-à-vis the pratical contexts of the activities studied is a trap we easily fall into. A way of avoiding this trap is probably to cultivate curiosity and surprise during our observations, getting involved all the while, stepping back, a step of reflexivity, thanks to our notes, index cards, memos, to put a finger on all sorts of mysteries which are woven into our research and reports.

H. S. Becker's Tricks: Polish, Specify, Clarify

In conclusion, we would like to present some advice on form which aims to simplify the text without reducing its quality or limiting its scope. This is a very simple advice on writing that we owe once again to H. S. Becker. We will summarize it very quickly, especially in the hope of giving the reader the desire to dive into the illustrations in the work (Becker 1986).

H. S. Becker first encourages us to adopt a simple and precise style. Clarity, simplicity and conciseness will protect us from two main pitfalls: "literary temptation" on the one hand, and "useless complication" on the other. Generally speaking, any wording here benefits from being re-worked "by ear", by reading it out loud or in one's head.

The sociologist also suggests that we avoid passive constructions and free indirect style: the former fails to define the authors of the actions described ("the criminal was tried" hides the judge and the protagonists of the court's decision), without encouraging us to extend our investigations; the latter does not enable the reader to know who is the author of statements reported without quotation marks.

H. S. Becker then recommends that we cut down our tests to clarify their meaning: reducing the number of words, avoiding repetitions, means that we are required to take responsibility for the flaws in reflection, and not cover them up with vague wording. Superfluous words are often designed to protect the researcher against the risk of error: by reservations and signs of modesty, they express the awareness that we have advanced too much, or that we are not right. More generally speaking, a useless word is a word that adds nothing: "it does not refine an argument, express a significant reservation, or add convincing detail". A simple test enables us to identify useless words and phrases – skim through one's text by monitoring each word: what happens if you take it out? They are more numerous than we would think.

H. S. Becker also suggests that we make the reasoning and central mechanisms of our arguments more explicit by reducing, prioritizing, and coordinating arguments. We need to put syntax in the service of argumentation: instead of enumerating three points, we will show, for example, how the ideas are interlinked, or we will accentuate the most important idea instead of leaving it on the same level as the others. Making syntax work towards argumentation also means that we place subordinate ideas in subordinate clauses and not in the main clause of the sentence.

Moreover, H. S. Becker notes the excessive use of abstract words which most often serve as fillers: they mean nothing in themselves, but are there instead of a true idea that they help us to avoid having to formulate and specify. Thus, we need to avoid "the vague abstract" (and banish hollow words), "the general abstract" (and to deal with the case before generalizing and not vice-versa), "the abstract abstract" (and attempt to give images, examples), "the programmatic abstract" (and to do instead of saying what should be done).

We are also invited to better discriminate between useful metaphors "overworked metaphors", these hackneyed expressions which do not add anything to the text. One way to test this is to systematically take them seriously by trying to draw them out all the way: does that really add something to the text?

Finally, three tricks are aimed more specifically at overcoming the anguish of writing. The first, useful to start off writing, is free writing: here we put down on paper all ideas which come to us without paying attention to the form or their implications. This allows us to dive into the water, dispelling straightaway the idea of perfect and definitive writing. The idea is to end up with a stock of notes, in bulk, which will provide material for a first pass at ordering notes, which will also ask questions about the usefulness, the relevance and the accuracy of each word (related to its synonyms, its various connotations); from one draft to the next, writing is thus continual re-writing.

The second trick concerns the construction of the outline: in making index cards, then piles of index cards, by giving them a title, classifying and re-classifying them, one can gradually specify categories, groupings of themes or close cases, and thus the possible structure of the text to come.

Finally, we can never make too good a use of the difficulties which prevent making progress in writing. It is often in describing the problem encountered, by searching for what it can teach us about the subject studied, that we advance in the construction of the problem. Just as on the research site, we sometimes have the impression that nothing is going on (Becker 2002, pp. 160-164); when we come to describe what has happened, we struggle to say that there was nothing interesting. We must endeavor to describe this "nothing". If we judge something to be trivial, without interest, it is often because we have already incorporated the ideas of the research subjects. In looking at things from a different angle, in changing perspective, in increasing the points of view, the tasteless takes on a taste.

Conclusion

Writing is not only a mode of expression of the social world, but also of knowledge and discovery. Varying the formats or styles of writing means that we authorize ourselves to discover other aspects of the reality studied, and to increase our rigor and reflexivity. Thus, stressing the question of writing differs appreciably from the stress associated with post-modernism and what is commonly called, in anthropology, the "crisis of representation" (Clifford Marcus 1986). The issue of reflections, like those of Jacques Goody, H. S. Becker or B. Latour, to mention only these three authors, consists in associating the consideration of the depth of writing, its cognitive effects and particular practices with the realistic context of the sociological approach, for which reality does not break up into the variety of its modes of understanding.

Ethnographic publication is only the last link in a "chain of writing" (Fraenkel 2001), from note-taking on the fly on the research site, to versions of the text before publication, going through numerous more or less systematic inscriptions, insights, analyses, comparisons, coding. This heterogeneous and disjointed work

of writing winds the ones into the others. Although the ethnographic report is not developed in the linear system of speculation and its refutation, it still claims to provide an original answer to the question "why?". This question goes through valid answers to the question "how?" i.e. by detailed, contextualized descriptions anchored in ethnographic experience.

This last remark brings us to the readers who, at the end of the day, judge the plausibility of our writings by the yardstick of their indexation on research situations. But they can also accompany the research, as native researchers who are also authors: letters and e-mails, narratives and analyses upon the request of the researcher, diaries, without taking into account all the written signs of their practical activities (Laé 2008). The most innovative modes of writing today in the social sciences – shared blogs, fictitious dialogues (Latour 2009), cooperative ethnography (Joseph 2007), etc. – have one thing in common, to further socialize the research, by enabling the reader, near or far, colleague, well-informed native or simple passer-by to participate in the field research underway.

Notes

1. We should recall the particularly deceptive expression of the French moralist Boileau: "What is well designed is clearly set out. And the worlds to say it come easily" (*L'art poétique,* chant 1, 1674).

2. Arrangement plays an important role here. With respect to its role more generally in maintaining the ability for action, the work of J.-C. Kaufmann (1991) is very instructive.

3. On the updating of historic figures and ethnographic authority: Clifford, 1983. This article was a major reference in the interpretative change in direction and in post-modern criticism in anthropology, which denounced, from both a scientific and political viewpoint, the operations of silencing research subjects.

4. (*Ibid*, p. 129-146) The reflection of de Geertz on writing is generally more sympathetic to a political anthropology which runs through his work (Cefaï 2007).

5. The interpretive change in direction in anthropology, encouraged by C. Geertz, strove to bring the author back to the core of ethnography, to re-assess his place in the objectivization of knowledge, sometimes even to consider that the only thing worthy of being studied without doing violence to the subjects of research is no longer anything but the researcher in the process of conducting it... This constructivist slope, taken particularly by the representatives of auto-ethnography, was sharply criticized because it no longer leaves the possibility of asserting anything of value on the world, and thereby strips the social sciences of its systems of scientific nature in the aid of stylistic research unsuitable for the sociological project.

6. For grounded theory, the first analyses begin even from the preparation of the first materials: "In such a way as to miss nothing that could make their saliency, the researcher should analyze the first materials as so many signs" (Strauss and Corbin 1990:366).

7. The name of this theory refers to the crash of the shuttle Challenger in 1986, due to the failure of a simple (!) toric seal.

8. M. Hammersley proposes to gauge jauger the quality of an ethnographic text by the yardstick of its plausibility and credibility. He writes, "By plausibility, I mean the degree of admissibility of an argument, given its compatibility with the horizon of knowledge that we consider as true"(1993, p. 301). He underlines the importance of the community of researchers in scientific assessment (and correction). He continues, "By credibility, I mean the degree of admissibility of an argument, given that the possibilities of error implied by the work of research seem relatively weak to us" (p. 300). In doing this, M. Hammersley relates the quality of the report to its fallibility. A good description should "give the reader the information necessary for the assessment of its validity and relevance, rather than present itself as [a definitive, irrefutable and unquestionable truth] "(p. 301). Needless to say that this fallibility is constructed during the crossing out, scribbling and resumptions of the original report.

9. For a presentation of conceptual positions and evolutions of grounded theory, see Chamarz, 2001.

10. The first version of analytical induction dates back to the 1920s and the work of F. W. Znaniecki. However, this method of data analysis has been given little notice in the French-speaking world. See the synthetic presentation of this provided by J. Katz, in "Analytic Induction," On-line at: HTTP://www.sscnet.ucla.edu/soc/faculty/katz/current.htm

11. To become a recreational marijuana smoker, writes H. S. Becker (1986), there are necessary five stages to go through: be put in a situation of smoking by spending time with a group which regularly practices this habit, learn to smoke correctly so that the drug will have an effect, be aware of these effects (one can "be high" without knowing that one is high), link it to taking the drug, and then learn to associate these effects with pleasure. According to his observations, no recreational marijuana smoker became so without going through these stages. In linking the experience of this illegal consumption to a series of trainings, H. S. Becker thereby rejects a certain number of psychiatric and individual explanations, and in an even more convincing way, given that his report on the career of the smoker is extremely fallible: by resorting to analytical induction, he makes his analysis conditional on the discovery of a single negative case, such as a recreational marijuana smoker who had not gone through all the stages described by the author.

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