

Foreword to Howard S. Becker, *Ecrire les sciences sociales*, Paris, Editions Economica, 2004

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(Translated by Rachel Gomme)

I read *Writing for Social Scientists*¹², in the French translation published here, with special pleasure because it represented the crystallisation, in book form, of a sociological reflection I had followed through several stages. The book also brought back a vivid memory of my first face-to-face encounter with Howard Becker, in 1985. At that time, over a series of conversations we engaged in during free moments at an international conference on the sociology of art in Marseille¹³, we spoke at length about the difficulty our respective students found in writing or finishing their theses. The conference was organised by the Société française de sociologie in collaboration with EHESS and the CNRS; at the time I was involved in the process of decentralisation of these two research and research training organizations, setting up sections for the study of art and culture in the Vieille-Charité cultural centre in Marseille.

Strolling along the sea front, wandering the beaches and coves of the Marseille shoreline as we took a rest from preparing the papers we were both to deliver at the closing session of the conference – which focused on the question of whether the aesthetic “value” of works of art could be reduced to their social value – we came to talk of the practices and techniques, the problems and tricks of the trade specific to writing up research in the different social sciences. Why was it that not only students and doctoral candidates, but often also established researchers, found it so difficult to begin to write, to show others their drafts, to rewrite, to rethink their scientific data through a series of rewritings and, above all, to complete the refining of the text of their theses, study reports, scientific articles or monographs? On his return to the US, Becker sent me the first draft of an article he had written diagnosing the problems of sociological language which arise at the point when it is “put into text” – a painstaking and painful process which Becker calls “composition”, incorporating the sense of the *dispositio*, *elocutio*, and the *inventio* of Latin rhetoric. Becker had already been offering group therapy for some years, in the form of his seminars at Chicago's Northwestern University, transforming them into a workshop for analysis and experimentation in the pathways, pitfalls and stumbling blocks involved in scientific writing in the social sciences.

¹² *Writing for Social Scientists: How to start and finish your thesis, book, or article*, Chicago, University of Chicago Press, 1986.

¹³ This conference (13-14 June 1985) was organized by our long-time mutual friend Raymonde Moulin on behalf of the Société Française de Sociologie, of which she was at that time chair, with the support of the Centre National de Recherche Scientifique (CNRS) and the Ecole des Hautes Etudes en Sciences Sociales (EHESS), and in collaboration with Research Committee 37 of the International Sociological Association. Most of the conference papers were collected in a volume edited by Moulin, *Sociologie de l'art [The sociology of art]*, Paris, La Documentation française, 1985 (reprinted by Editions de l'Harmattan, Paris, 1999).

The nature of the challenge in writing

Was the problem one of psychological inhibition, anxiety or depression, such as may be experienced by any author or artist whose self-esteem is dependent on the judgment of others (peer, boss, reader, editor or commissioning body)? Probably. But are individual “character traits” and the variation in narcissistic equilibrium sufficient to explain the polymorphous anxiety of intellectuals and researchers about the *value* of their performance? Or is it the tasks themselves, and the interactions involved in apprenticeship in the professions, beset as they are by unregulated competition of everyone against everyone else, that feed this insistent, insurmountable and, for some, paralysing doubt? In a society increasingly marked by the fiction of mass education, the publicity given to achievements and winners, and the media's constant rehashing of the same themes, I would argue that we are witnessing, particularly in the intellectual professions, a self-destructive “democratisation” of the childish desire of everyone to acquire, as a birthright, the material and symbolic gratifications of *social rarity* and *cultural legitimacy* that distinguish the “happy few”. The personal “glory” which attaches to someone who produces truth or beauty is presented, against the background of the jargon of the moment, as both the just reward for exceptional universal value and a universal goal accessible simply on request or whim. This stimulates the onset and feeds the fever of failure neuroses, punctuated by brief upsurges of hope constantly reawoken by the illusion of having found the miracle recipe, with the suffering and panic which inevitably follows. The ego aspires to the ideal; at all levels of the psyche, the cruelties of the super-ego bind juvenile ambition for the highest realisation of the self as creator of unique values inextricably with the magical fear of failing pitifully, through bad luck or malicious persecution. The two are tied up in knots which are all the more impossible to untangle because the tightest bonds are those between the imaginary and the symbolic.

And why is it that the difficulty, as far as scientific writing is concerned, is restricted to or more acutely felt in the social sciences, and still more specifically in the case of sociology, in the difficult process of learning to put arguments and evidence into writing? Becker's intensive study, along with other “indicators”, shows exceptional levels of unease in the discipline among those faced with the “obligation” to write a text, particularly when the writer is in a situation where sooner or later the text will have to be submitted to the criticism of a jury, whether formal or informal. Why should this be? What, for example, is behind the very high rate of abandonment of planned theses, through stress, breakdown or disillusionment? Avoiding the work of writing is primarily an avoidance not so much of the linguistic difficulties of written expression, as of the risk of having to think under the gaze of others. These others, as readers momentarily lifted out of this status of voluntary victim, are then placed in the comfortable position of masters who can examine and test, without risk or obligation, a text offered up to their scalpels, naked and immobile, incapable of defending itself on its own through counter-

attacks or defensive arguments, “a fatherless orphan”, as Plato described writing. Improvised speech does not expose the improviser so cruelly to the mockery of his/her listeners, since it can always strike back if it is contested; it is a flow that cannot be immobilised as a target, a thread of fleeting spoken words quickly forgotten in what follows, leaving little trace except perhaps the memory of the prestige, bearing or presence of the speaker.

One of the reasons for the failure of social scientists to express themselves effectively in writing surely lies in the generally diminished levels of willingness and enthusiasm among today’s students, combined with a narcissistic rejection of the impersonal labour imposed, in any consistently pursued, in-depth reasoning, by the “formal”, burdensome, frustrating demands of scientific communication, always potentially addressed to a “universal audience”. But in order to identify the ways in which different writing situations reactivate the childish fear of “submitting” to the mockery of classmates or playground companions, we need to put the rather too general question of the “legibility” or “difficulty” of scientific texts in specifically sociological terms. In what locations, and in the service or exclusion of which social groups, which audiences, which professions, which interactions, which writing strategies, does the semantic fog surrounding sociology’s most abstract concepts and schemas form? Why does it accumulate in the vocabularies of causality and meaning of actions and interactions, around criteria of the truth or falsehood of sociological discourse? Where do the pedantic turns of phrase and the nebulous or abstruse words, which in sociologists’ writing seem to be prompted by the fear of encountering an informed and critical reader or editor, come from, and why and how do they spread from sociology to other historical disciplines (by which I mean all the sciences which are not strictly formal or experimental)? It is fear that leads the wary sociologist to sense danger in handling phrases which are too clear, unscreened and open to the day, in the uninhibited use of indicative affirmations untrammelled by all their rhetorical shadings and nuances, and to apply the “maximum precaution principle”. What are the sources, whether realistic or fantastical, of this fear? The constantly reiterated “It is as if...”, the elastic use of various modes of the conditional mood, the superfluous approximations which conceal contradictions, the use of indefinite pronouns and “vague quantifiers” give rise, in all languages, to most of these “empty formulations”, padding for a stilted tongue or academic dissertation phrases that French philosopher Eric Weill, with pitiless logic, would annotate in the margin with “not even false”. This semantic dilution relieves such formulations of all risk of error, removes their empirical vulnerability; by the same token they are emptied of all assignable *assertory meaning* because, if we analyse the logical significance of these “weak” or cunning systems of assertion¹⁴, they are no more than expressions devised expressly for the purpose of quietly avoiding the scientific obligation always to formulate assertions which refer clearly and unequivocally to a definite mode of confirmation or objection.

¹⁴ For an example (of the use and abuse of the “Admittedly... but...” formulation), see Jean-Claude Passeron, “Logique formelle, schématique et rhétorique [“Formal, schematic and rhetorical logic]”, in Michel de Fornel and Jean-Claude Passeron (eds) *L’argumentation: preuve et persuasion [Argument: proof and persuasion]*, *Enquête* (new series, II), Paris, Editions de l’EHESS, 2002, p. 159-164.

There is no doubt that recent changes in the educational ethos which shapes the forms and public image of scientific work from the earliest age have had some effect here. As the social and technological conditions of family and school socialisation change, we see in all classes and social groups the spread of, and emphasis on, behaviours characterised by *stop-start commitment* (alternating brief moments of effort with long periods of “cool”, playful or nonchalant attitude); this can be observed in apprentice sociologists just as in other apprentice intellectuals. But we also need to consider other causes for the acute presentation and rapid progress of a professional disease which, in many intellectual professions, dilutes apprenticeship in scientific language and work in a soup of approximations and caveats. For example, it seems – and this hypothesis is epistemologically even more disturbing for sociology – that the original sin, from which the recurrent failure of the social sciences to manage precision in references and assertory monosemy in the empirical description of their “facts” derives, must be *the language itself of scientific theory* in these disciplines. Here grammar, vocabulary and rhetoric of explication and interpretation have not converged to unite in a more or less homogeneous semantics, as they have in the strictly formal or experimental sciences, through reciprocal exchanges between them. On the contrary, the social sciences have diversified and merged in a confusion, over a series of crises in a scientific consciousness which is almost always “unhappy”, dissatisfied with the methodological status of these disciplines, tormented by their perpetual desire for a renewed theoretical foundation.

This is perhaps the nub of the epistemological issue, which explains the lack of rigour and the congenial permeability of sociological language to conceptual approximations, elisions of connotation, authorial jargon, dialects composed through mechanical accumulation of sentences cut and pasted together, which are themselves simply passively mimicking the lazy syncretism of the most academic teachers. To put it more precisely, the popularity of *the language of avoidance* among students and most researchers in sociology is due to the constant concern to minimize risk: the risk of having to make a public statement, about an affirmation too exposed to objections, or of having to respond to a request for clarification about the evidence for or the meaning of this affirmation. The memory of examinations still stings for students who have had to spend a long time skirting the danger of speaking too clearly before an omnipotent judge or a distant sovereign tribunal, whose judgment is experienced as arbitrary because it is always formulated on the basis of implicit expectations and demands, never fully articulated in the traditional pedagogy of “it goes without saying”. In this situation the defensive use of *the academic language of imprecision* seems the only means the student has to maintain some chance of neutralizing the destructive criticism of examiners who specialise in the sarcastic savaging of apprentice texts, particularly in a science like sociology which is almost universally suspected of being epistemologically unstable or “dubious”. The sacrifice of readability results from this tremulous prudence, reflecting an intense fear of “getting wet”. As Howard Becker shows us, we first need to diagnose, without

disciplinary complacency, this disease which attacks some scientific languages more than others. In this way we can formulate and put into practice a pedagogic therapy capable of countering the disease's long incubation in the interactions of university apprenticeship: what is needed is a social re-apprenticeship as interactive as that in which the fear of speaking clearly originated.

Parallels, divergences, tangential encounters

I had been impressed for some time by the study data and descriptions Becker presented in his earlier work, which posed epistemological questions similar to those I was asking myself in the 1970s. My research was based on logical, linguistic and rhetorical analysis of a body of texts drawn from various texts in the social sciences, both classical and contemporary, deriving from a range of tendencies or schools and engaging different methodologies or styles of argument¹⁵. In 1985 I discovered Becker's initial studies of the struggles of sociological writing, noting the acuteness of his observations and his interpretations, very close to the epistemological description of the scientific labour demanded by sociological research or reasoning that I myself was sketching out at the time.

I would even go so far as to say, on this point, that Becker's probatory approach seemed to me more convincing than my own, if only by virtue of his focus on an individual "case" – that of writing sociological arguments or proofs in a university learning context. Behind our epistemological agreement, which we noted with mutual satisfaction, I was surprised to discover an unexpected divergence with my "interactionist" colleague. Ultimately Becker was more of a sociologist than I, despite the fact that at that time I believed that I was and presented myself as firmly "Durkheimian", resolved in the name of the *Rules of the Sociological Method* to "explain the social purely in terms of the social". I now see clearly that, in his analysis of the relationship between university language and scientific language Becker, without abandoning his interactionism, was orienting himself with the demystifying vein of the objectivist analyses of *Reproduction*¹⁶ on the "arbitrary" symbolics of university "authorities" and administrations.

Under the Weberian influence of a "sociology of understanding", I had recognized that interpretation is inherent in the construction of historical objectivity in any description of a "state of affairs", and even in the interpretation of the meaning of the simplest statistical table, once we restore it to its context. But by the same token, my recent discovery of the role of

interpretation in historical explanation, in privileging *clinical observation* of social actions, tended to obscure the *institutional power* of objective constraints – in the case of sociology, the formidable institutional weight of academic idiom, the arbitrariness of which Becker was happily exposing. In short, my conceptual framework for interpreting the intellectual fear of betraying oneself through one's writing had remained Freudian in texture, since, in the analysis of a text or a behaviour as in the treatment of a patient, it only engaged the interaction between two interpreters of a set of symptoms, indicators or signs – the analyst and the analysand, the observer and the text of his observation, left alone together to construct the meaning of their hermeneutic agreement. The semantics of an interpretation based on this psychic grammar can thus only return to the most generic form of the "instincts of the Self" in operation here. The only possible therapeutic outcome is the very relative efficacy of the teacher's exhortations to the anxious writer to dismiss the phantoms and phobias, recommending her/him to undertake rational exercises in rereading and rewriting her/his scientific argument in private. In a sociological research project the transference and counter-transference set in motion by the study, in the form of texts submitted unrefined, cannot be so radically abstracted from the reality and the repercussions, tensions and conflicts of social life; the risk here is that we believe, and lead others to believe, in the universal value and efficacy of interpretations which are exhausted merely in the affective "resonance" of interpersonal relationships formed in a clinical context of "private" exchanges.

There was a simple reason for my over-simplification. As in psychoanalysis – but minus the prior agreement with the student on the way his request for help would be approached – it was only in one-to-one tutorials that I encountered the resistance of doctoral candidates who balked at writing or at stating explicitly the principles of a consistent process of rewriting. They did not hear my offers of help; I did not understand their deafness. Moving to a "doing" therapy was "naturally" impossible: for the subject, undertaking a *procedure* of rereading her/his own work, transformed into a reader capable of standing outside her/his spontaneous actions of sociological writing in order to think more precisely about the meaning of what s/he had written, established a "rule" that the author's narcissism must be put to one side, and thus effectively became a *technique of thinking* as unnatural to this subject as that required by "free association" used to free a patient from the need to reiterate rationalisations for his/her everyday actions and feelings. But in the case of the student, the verbal contract was not embedded in the "analytical situation", where it would hold only for the duration of the session. In a socially "normal" dialogue situation, that is to say one that is open to debate – where each of the partners seeks to occupy the position of both analyst and analysand – the *work of thinking* essential to a fruitful, considered *rewriting* process was always deferred in favour of a voluble reiteration of fantastic, ever more ambitious plans, constantly reformulated through oral improvisation and volatile expression, but abstracted from any rereading. Here I was encountering the same phenomena as Becker described: paralysis, refusal, avoidance, protests and other subterfuges characteristic of the trainee researcher put on the spot by the teacher,

¹⁵ A first version of this study of "descriptive epistemology" appeared in *Les mots de la sociologie: analogie et argumentation* [*The words of sociology: Analogy and argument*] (State thesis, University of Nantes, 1980); I systematised this epistemology of "theoretical plurality" in *Le raisonnement sociologique: un espace non poppérien de l'argumentation* [*Sociological reasoning: A non-Popperian space of argument*], Paris, Nathan, 1991.

¹⁶ Pierre Bourdieu, Jean-Claude Passeron, *Reproduction in education, society and culture*, London, Sage Publications, 1990.

who resists encouragement to write and show a draft text, always putting off the task until later, as if to avoid indefinitely the disillusion, ridicule and humiliation of being “corrected”. But I had not succeeded in establishing the rules for a collective catharsis

I admit that I can count on the fingers of one hand the few cases in which I managed, in one-to-one encounters with doctoral students, to “sociologise” the student’s thinking through a concerted rehabilitation process based on writing, rewriting and rewriting. Becker, however, had succeeded in introducing a programme of exercises derived from a more appropriate method for analysis of university socialisation: the rules of the game had been formulated and applied within the context of a co-operative process, which still remained an ordeal, but made it possible to restructure the relationship between reasoning and writing, because the evasions were less effective in a group where the members knew one another, as regular participants in the same socio-drama. In this case the sustained observation operates in a context of multiple interactions, themselves embedded in the familiar context of the university institution which vouches for the professional usefulness of such time-consuming and frustrating work, which it justifies by relating it to the memory of previous failures, and the continuity of the participants’ projects and already substantial experience. This volume includes lengthy quotations from young researchers who were at the beginning of their career, like Rosanna Hertz and Pamela Richards, who were able to present a retrospective written analysis of their process, its implications and its key points – refusal, block followed by crisis and emergence from crisis (see Becker, 1986, 26-31 and 111-120, translated as Becker, 2004, 33-37 and 117-127). The subjectivity of these testimonies, with their dips and high points, outlines the role of the various *personae* who for each individual symbolically occupy the place of the “good” or “bad” models of writers or readers, teachers or pupils. Hertz’s account, for example reveals which models of writing were spontaneously experienced as “classy” (as opposed to “folksy” or “inside dopester”), and which ones gave an indication of the modes of expression that conferred “authority” and the right to say things as they should be said. This demonstrates the role of peers, and of the close or more distant circle of colleagues, present and past, as “assumed readers” – all their real or symbolic weight bearing on the controlled expression of anyone who writes a text for an audience. As we shall see, the majority of Chapter VI is taken up with Richards’ retrospective recounting of her anxieties, dreams and daydreams, the “benefits” she gained from analysis of writing exercises undertaken at Becker’s recommendation or at his insistent demand for writing: it is effectively written by a collective author derived from an interactive experiment which examines what happens to the dramas and work of writing when they are rewritten during the course of the experiment itself, in order to discover and say what one is experiencing.

Here we come to the heart of the question of why battling against inhibition is less effective as self-therapy when it is conducted as a monologue or in a one-to-one interaction than when it is embedded in a situation of multiple and repeated interactions which bring into play various

intellectual forms of symbolic power, submission and rebellion, as well as the full range of social roles, affects and complexes which have governed the formation of attitudes of romantic exaltation alternating with stubborn resistance to the labour of writing. What has been constructed through and in the context of repeated interaction, through the training and learning, joys and suffering of socialisation, can only be taken up and reconstructed in an interactive context which is itself endowed with revived desire for learning, resupplied with self-representation and thus made accessible to new emotions.

Sociological thinking and writing

During our conversation by the sea, we coincided in our diagnosis of all these issues, particularly since we were able to provide specific examples of the forms of resistance that our attempts at pedagogic intervention in the face of the “fear of writing” or the refusal to “show what one has written” had encountered in our students and colleagues: many, and often amusing, were the instances of evasion and bad faith to be found just as frequently in the digressions of wily experience as in adolescent panic. There were two reasons for our epistemological agreement that *sociological reasoning and sociological writing are indissociable*. On the one hand, we accepted the theory, now widely shared in much writing in the human sciences, that the structures of thinking and of the oral or written expression of that thinking are mutually inherent, whether in everyday or in scientific languages. This thesis runs counter to the impression the individual derives from internal experience of the link between “thinking” and “saying”: analysis of the difficulties or weaknesses of a language communicating information or explanation shows writers who have difficulty with the writing of that language almost always experience and conceive of it as a subordinate tool of, or even an accessory to, thought. In the spontaneous metaphysics of self-consciousness, “thought” – whether communicated clumsily or accurately, expressed or silent, oral or written – can only be conceived as a substantive reality which always remains equal to itself in the interiority of a reflection unaffected by the way in which it is communicated. Furthermore, Becker and I, like all sociologists who rebel against traditional or modern scientisms, subscribed to the premise that we can only describe as social actions those behaviours and intentions for which we can identify the actual or virtual “others” who provide the orientation for these actions (as Weber formulated it).

Sociologists’ relationship with sociological statements is never either immediately, or somewhere in the ether of Logic, a “pure” relationship with the sociological truth of these statements. In describing the forms and evolution of such utterances, we must not forget that this relationship was constructed during the years of study (and already preconstructed in the prehistory of representations of “correct expression” and “truth-telling”). Before it was even conceived, students’ relationship with sociology – like any relationship to the legitimacy of a science or a belief – was represented and perceived as a relationship with a “higher authority”:

a hierarchical establishment on the model of a Pantheon of gods or a family. This authority was fantasised as a supreme court which summoned apprentice sociologists, “caught red-handed” with their incorrect texts, to appear before it, bringing their error-strewn essays as evidence. This court even appeared in dreams and daydreams and, whether cruel or benevolent, was always imagined as entitled, “since time immemorial” and by ancient right, to impose implacably its undisputed power to condemn, without right of appeal, those subject to its jurisdiction – the bad students, who became the ridiculous, incompetent, clumsy, mediocre or uninspired researchers – and occasionally to confer good reports or rewards. This sociology of the origin of intellectual and scientific norms in the minds of students needs at least to be sketched in if we are to understand students’ behaviour in accepting or skirting logical and methodological norms, particularly when they mix acceptance and avoidance. Hence the satisfaction that Howie and I shared, I believe, at each meeting a colleague who did not minimise the epistemological importance of this field of sociological enquiry, which was admittedly conveniently close to home, since it was located in our lecture theatres, but which allowed us to get closer to the scientific and pre-scientific meaning of what all the field studies, and their various expressions in text form, signified than we could have done in a less familiar field.

This approach might appear picky or pettily sociocentric to most philosophers, who believe that great thinkers are made only by “great subjects” and a remote gaze. But the sociology of scientific practices is, like their history, an indispensable prerequisite for their epistemology. Errors or approximations in articulation are not just errors of vocabulary or language, rhetorical clumsiness or grammatical weakness which only a grammarian or stylistician would find reprehensible. They are in fact logical barriers raised in the path of affirmations of “fact”, fences in which the barbed wire abstracts the sense of an argument from the scientific norms of theoretical coherence and empirical verification. Evidence is not subject to the monitoring of “relevant features” required by historical comparison, or to the interpretation of the parameters or results of a calculation of probability, or indeed to the work of argumentation which organises suppositions from various sources as to the social meaning of an action or an interaction into a rational argument: the empty circumlocutions of semi-scientific dialect, more effective than the easily identified naivety of everyday language, dispense with all that.

Writing and rewriting an argument in order to *think what one has written*, while asking oneself *what the reader will read in it*, is the only universal remedy for errors of writing and thought, which are always linked. As Becker emphasises right at the beginning of this book, stratagems or recipes for writing, of which his long study of writing practices has revealed the magical and propitiatory functions, are not simply all-purpose techniques aimed at releasing “writer’s block” using a generic treatment, or improving beginners’ muddled efforts by means of simple “stylistic exercises”. There are plenty of “essays”, short and long, which limit their pedagogic

strategy to recommending to everyone the same solo exercises as a way of overcoming difficulties in writing – just as there has always been a multitude of academic handbooks on good writing for use in primary and secondary schools, founded on Boileau’s deceptive aphorism that “whatever is well conceived is clearly said and the words to say it flow with ease”. There are more such handbooks in the United States than in France, and Becker makes frequent reference to them to demonstrate their limitations. Even when they are based on contemporary linguistic analysis, these manuals assume that all forms of writing are equally subject to grammatical and lexicographical analysis of discourses whose texture never varies, and which may be understood and successfully communicated to any readership. Thus they happily confuse the six functions which Jakobson so carefully distinguished – the phatic, referential, emotive, conative, metalingual and poetic¹⁷. The problem we are concerned with here obviously relates purely to the “referential” (here called the “assertory”) usage of statements made in a natural or artificial language. Thus we clearly need to describe the “logical space”¹⁸ in which the statements of a science like sociology *assert*, before we deny or affirm its methodological “specificity”: is a historical science a science “like all the others”, or not? And if we ignore the complacent “yes” of scientific positivists, does answering “no” mean consigning social sciences to essayist literature, or does it imply a duty to describe a different “regime” of scientific being, a different “style” of handling procedures of evidence?

Becker and I thus discovered that we had in common at least our methodological concern with the “case study” and “collections of cases”. Logical, methodological, juridical or ethical approaches to the individual instance, and particularly to the reasoned “description” of it as a “case”, raise a central epistemological question in all social sciences, once these disciplines admit that they cannot be reduced to the epistemological status of “nomological” sciences (those which can formulate “universal laws”). The case study or the series of cases pose in crucial form – in the sense of an *experimentum crucis* – the question posed in any historical science by the relationship between concepts and individual instances. Becker had already written *What is a case?*¹⁹ in collaboration with Charles Ragin, in which he examined his operative definition of the specificity of descriptions made by the researcher in an individual, localised “terrain”. I myself had begun to explore studies by historians of science looking at the position of Hippocratic collections of “cases” in Greek medicine vis-à-vis mathematics and geometry; I had then followed the succession and reworking of “clinical” approaches as far as Charcot and Janet’s studies of cases of “grand hysteria” and the archetypal “cases” in Freud’s

¹⁷ Roman Jakobson, “Closing statement: Linguistics and poetics”, in *Style and language*, ed. Thomas A. Sebeok, Cambridge, MA, MIT Press, 1960, pp. 350-357.

¹⁸ Wittgenstein used the term “logical space” (*logische Raum*) to designate the world of logical constraints necessary and sufficient for at least defining the possibility of truth or falsehood attached to the statements of a discourse: *Tractatus logico-philosophicus*, London, Routledge & Kegan Paul, 1922 (first edition 1921), proposition 1.13, p. 3. Hence the rigour, which to Wittgenstein’s contemporaries seemed excessively “logicist”, of the last proposition in this work: “What we cannot speak of we must pass over in silence” (*ibid.*, proposition 7, p. 188). Clearly Wittgenstein abandoned this in his later philosophy of language. In fact this minimum of logical coherence defines the range of what can be truthfully stated in a given “assertory space” very differently depending on the different methodological circumstances of the scientific discourse.

¹⁹ Howard Becker, Charles Ragin (eds), *What is a case? Exploring the foundations of social inquiry*, Cambridge, Cambridge University Press, 1992.

Five Lectures on Psychoanalysis, during the emergence of modern clinical psychology. This revealed a relationship between the clinical method and the historical method, which in its turn revealed a relationship between what is required in the description of “cases” in history and in the description of “context” in sociology. What all these approaches have in common, in fact, is that they proceed by descriptions and comparisons of cases, without ever reducing them to the inert status of “examples” which can be interchanged within a given category and are subject to being included in the study as soon as they are shown to conform to the unambiguous criteria for exclusion or inclusion in the category. In effect, the *inductive* approach – a *vertical* “subsumption” – is adequate for identifying such basic elements and formalising them in an abstract structure, and for identifying repeated co-occurrences as scientific facts, in an experiment in the strict sense. Conversely, *thinking by cases* produces intelligibilities, and thus, by traversing and reconfiguring collections of cases *horizontally* – i.e. by treating the “relevant” features of a coherent interpretation of their semantic analogies as an *ideal type* – places the question of what “speaking and writing truth” means in sociology in its true “epistemological place”.

The general problem of the operators and forms of a scientific language – formalisms or contexts, amorphous or structured states, normalities, exceptions or deformities – only acquires its full clinical meaning (both symptomatic and aetiological) when it is posed as we work in depth on “cases” whose sociological relevance is constituted by their social complexity.²⁰ Reading a number of Becker’s texts²¹ had convinced me that with extreme cases, that is to say cases that are extremely localised and very restricted in scope – “marginal” cases in some of the “art worlds” he described, or quasi-pathological ones, like that of the misunderstandings that arise in “inter-comprehension” between sociologists, who whether students or established are all equally lacking any possibility of arriving at a single or dominant theoretical paradigm (and thus are always potential competitors or adversaries in their struggle to defend at all costs the truth of their statements) – with such cases, we enter a realm of epistemological singularity such that it fully exposes the peculiarities of a scientific communication which aims to give an account of such social objects. Communicating a form of knowledge like that of individual historical instances can only operate *without semantic losses* through texts which synthesise multiple dimensions of description. This is certainly the case for sociological writing, in day-to-day fieldwork or literature research, in the simultaneous recourse to several theories of the social, and in the communication of sociological results and the presentation, in legible texts, of both its heuristic progress and the evidence ultimately assembled.

²⁰ In anthropology too, at least the anthropology which holds fast to the “casuistic” ambition of the first ethnographers, only in-depth analysis of “cases” can define the scientific function of a “thick description”, by contrast with a “thin description”: see the first part of Clifford Geertz’s book *The interpretation of cultures*, New York, Basic Books, 1973.

²¹ Howard Becker, *Outsiders: Studies in the sociology of deviance*, Glencoe, Free Press, 1963; *Art worlds*, Berkeley, University of California Press, 1982.

The vision of sociology is indissociably both synthesising and historical, thus setting the discipline a task which is at once statistical and historical, comparative and clinical. The price of this totalising ambition, limited only within the programme of specialised social sciences, is that today our discipline is condemned to writhe ceaselessly on a Procrustean bed which requires it, simultaneously and at every point in its arguments, to conform both to the prestressed posture of an exact science and to the very different one of an interpretative science of historical “objects”. It is a heavy epistemological privilege to have to force oneself, if one is not to refute one’s beliefs, to take on the whole package of historical scientificity. Most other social sciences are allowed, by virtue of stylistic habits consolidated over the course of their scientific history, to specialize in a preferred methodology – a position which releases them from the contortions between one extreme and the other of scientific reasoning that are required of sociology. The specialization or autonomy of the “particular” social sciences allows them a wider range of methodological choices to select from. They may use *modelling of empirical co-occurrences* which define a “state of affairs” – an overall historical configuration can always be simplified in a “simulated model”, as in economics, demography and linguistics. Conversely, they may use *chronological recounting* of the succession of global social configurations in order to then attempt to reconstruct series or systems of analogies revealed by historical comparison – as do history and historical anthropology, exchanging methods with one another. If we look closely it is only in sociology, whatever the name under which it is practised, that the two criteria for a complete social science have to be satisfied at the same time: it must both operate as an empirical “science”, in the demanding sense implied by any scientific handling of quantitative evidence, and must also remain a “historical” science of individual actions and events which does not cut out any actor or any pertinent fact which may have meaning for the explanation. Thus there is a double requirement, for “interpretation” (*Deutung*) of the singularity of actions in the Weberian sense, and “explanation” of social patterns in terms of cause and function, in the Durkheimian sense.

Put to the test of drawing a conclusion, I notice here the difficulty of introducing this book by Howard Becker, since its originality, like that of other ethnographic, interactionist or ethno-methodological texts, does not lend itself easily to an overall summary or a list of the main themes. At once a study of the writing of sociological texts and a theoretical reflection on the texture of practices and paradigms in the social sciences, the richness of Becker’s analysis of the social position of sociologists forced into the straight-jacket of precise writing in a “case-based science” suggests a vast range of possible developments, from the most theoretical to the most empirical. It would not be possible to replace Becker’s thoughtful overview with a preface summing up the content without losing much of its probatory richness, its convergences of arguments, its paradoxes unravelled and above all the pleasure the reader takes in its agreeable illustrative digressions. As in most of Becker’s other books, the reader also has the opportunity to become more fully personally acquainted with Howie and his anti-

academicism, his rigorous observation and his enthusiasms, his humour and his scientific indignation.

Writing for Social Scientists is not only a fully realised study, but also a crossroads opening up a multitude of other possible studies, conducted in similar style, using the acute methods of “symbolic interactionism”. The text deals with both the words and the wants of sociology, losing nothing of its aim to analyse a “case” as it traverses the vast ocean of disputes among sociologists. It clarifies both the social conditions of truthfulness of sociological assertions and the non-assertory element always contained in the sociologist’s utterances, never to be completely eliminated. To a greater or lesser degree, the sociologist is always in a double bind, between the quest for simplicity and the risk of banality, between informed and well-formed sociological reasoning and non-sense or rhetorically overburdened arguments. The epistemology of sociological reasoning proposed by Becker’s study explains perfectly why the work of deconstruction, reconstruction and “review” of evidence in a social science is never finished²² – like a Freudian analysis. It reveals the scientific emptiness of projects which seek to avoid, simply through integral formalism or through the author’s rhetoric, the sociological work of continual revision of sociological utterances. But this continuous, detailed work, adjusted to the descriptive patience of “non-monotonal logics” of reasoning, is the only approach which can enable us to overcome the difficulty of being a sociologist without verbal artifice or mathematical conjuring tricks. The words of sociology, congenitally too flexible and incapable of giving “absolutely definite descriptions”, do not in themselves explain the assertory ills from which sociological discourse suffers: the cunning or incompetence of some sociologists certainly play a role. But the texts of canny or clumsy sociologists are not the only ones to reveal the impossibility of containing all the words, concepts and assertions of scientific sociology in a single theoretical system: the uncontrolled use of sociological language is just one element in the failure of all attempts to reintegrate the Protean state of research in social sciences within a unified paradigm –albeit one whose rhetorical digressions aggravate this failure. The writing of the best sociologists encounters the same epistemological barrier, the same internal challenge, embedded in the language which must be spoken by a discipline whose central concern is to describe the singularity of contexts in ever more precise detail. The sociological study most devoted to exploring the *empirical* peculiarities of a “field” or a “case” is also a *theoretical* work of continually recasting the language of description of historical worlds, constantly replenished with new “facts” which could not have been observed if a new theoretical framework had not first made them conceivable.

²² See, for a recent use of this notion which has become a principle of logical analysis of “natural reasoning” in historical comparatism and ethical and juridical “casuistics”, Pierre Livet, “Formaliser l’argumentation en restant sensible au contexte [Formalising argument while remaining sensitive to context]”, *Enquête* II, op.cit., p. 49-66.