Bourdieu Chamboredon Passeron

The Craft of Sociology

The Craft of Sociology
Epistemological Preliminaries

Pierre Bourdieu
Jean-Claude Chamboredon
Jean-Claude Passeron

Edited by Beate Krais
Translated by Richard Nice

Walter de Gruyter · Berlin · New York 1991

Pierre Bourdieu
Professor at the Collège de France, Director of Studies at the École des Hautes Études en Sciences Sociales, Director of the Center for European Sociology, Paris, France
Jean-Claude Chamboredon
Director of Studies at the École des Hautes Études en Sciences Sociales, Paris, France
Jean-Claude Passeron
Director of Studies at the École des Hautes Études en Sciences Sociales, Paris, France
Beate Krais
Max-Planck-Institut für Bildungsforschung, Berlin, Germany

Translated by Richard Nice, University of Surrey, Great Britain

Library of Congress Cataloging-in-Publication Data

Bourdieu, Pierre.
[Métier de sociologue. English]
the craft of sociology: epistemological preliminaries / Pierre Bourdieu, Jean-Claude Chamboredon, Jean-Claude Passeron: translated by Richard Nice. — English ed. / by Beate Krais. XVI, 272, p. 23 × 15 cm.
Translation of : Le métier de sociologue. Includes bibliographical references.
ISBN 0-89925-870-0 (U.S.) (alk. paper)
ISBN 0-89925-555-8 (U.S.) (pbk.)
1. Sociology—Methodology. I. Chamboredon, J.C. II. Passeron, Jean-Claude. III. Krais, Beate. IV. Title.
HM24.B67213 1991 90-24390
301'.01—dc20 CIP

Deutsche Bibliothek Cataloging in Publication Data

The craft of sociology: epistemological preliminaries / Pierre Bourdieu; Jean-Claude Chamboredon; Jean-Claude Passeron. Engl.ed. by Beate Krais. Transl. by Richard Nice. – Berlin; New York: de Gruyter, 1991
Einheitssacht.: Le métier de sociologue ‹engl.›
ISBN 3-11-011940-04
NE: Bourdieu, Pierre; Chamboredon, Jean-Claude; Passeron, Jean-Claude; Krais, Beate [Hrsg.]; EST

∞ Printed on acid-free paper which falls within the guidelines of the ANSI to ensure permanence and durability.

Printed in Germany
Typesetting: Wyvern Typesetting Ltd, Bristol, GB—Printing: WB-Druck GmbH, Rieden a/Forggensee—Binding: Lüderitz & Bauer GmbH, 1000 Berlin 61—Cover design: Johannes Rother, 1000 Berlin 21.

Preface to the English edition

This book was initially designed for teaching purposes, to enable students of sociology to become better equipped to cope with the pitfalls of sociological research. But it is not just a primer setting out the difficulties and risks of scientific work in sociology with the aid of illustrative texts drawn from the whole range of the human sciences; it is also a contribution in its own right to the epistemology of the social sciences. In accordance with the initial didactic intention, it was originally planned as the first part of a three-volume textbook, which was to deal not only with the epistemological foundations of the science of sociology but also with the major questions this science puts to itself and with the methodological resources of empirical sociological research. This original intention was not fulfilled, as is explained in the Preface to the second French edition and also in the interview with Pierre Bourdieu in this new edition.

The first French edition was published in 1968,[1] at a time when empirical social research in European sociology had already become an integral and recognized part of the discipline. Although it was far from being as developed and differentiated as it has now become, it was able to point to some interesting findings and was characterised by intensive discussion of its methods. In the context of Pierre Bourdieu's wide-ranging production, the epistemological reflection presented in this book can be situated at a precise point in his development as a sociologist — and much the same could be said of empirical social science in general. Bourdieu, like his co-authors, was already able to look back on a rich experience in sociological research. By 1968, some of his major works had been published — the studies on Algeria, the studies in the sociology of art (on photography and museums), and a number of works on the functioning of the educational system.[2] However, the major part of his oeuvre was still to come. Perhaps it is exactly that moment in the development of a discipline, when research routines, specializations, and the accompanying

[1] This translation is based on the second French edition of 1972, in which the first edition was slightly revised and the number of illustrative texts was reduced.

[2] P. Bourdieu, *The Algerians*, Boston: Beacon Press, 1962; P. Bourdieu, A. Darbel, J.-P. Rivet, & C. Seibel, *Travail et travailleurs en Algérie*, Paris and The Hague: Mouton, 1963; [partially translated as: *Algeria 1960*, Cambridge: C.U.P., & Paris: Maison des Sciences de l'Homme, 1978]; P. Bourdieu & A. Sayad, *Le Déracinement: la crise de l'agriculture traditionnelle en Algérie*, Paris, Éditions de Minuit, 1964; P. Bourdieu, L. Boltanksi, R. Castel, & J.-C. Chamboredon, *Un Art moyen: essai sur les usages de la photographie*, Paris: Éditions de Minuit, 1965; P. Bourdieu, A. Darbel, & D. Schnapper, *L'Amour de l'art: les musées d'art européens et leur public*, Paris: Éditions de Minuit, 1966; P. Bourdieu, J.-C. Passeron, *Les Héritiers: les étudiants et leurs études*, Paris and The Hague: Mouton, 1964 [translated as *The Inheritors: French Students and their Relation to Culture*, Chicago: Chicago University Press, 1979]; P. Bourdieu, J.-C. Passeron, M. de Saint-Martin, *Rapport pédagogique et communication*, Paris and The Hague: Mouton, 1965.

self-evidence of methods and techniques are not yet fully developed, that makes it a matter of particular urgency to take stock of one's own procedures. Thus, in West Germany, the 1960s were the years of the *Positivismusstreit*, a controversy which evolved from the need felt by sociologists to establish clearly the methodological position of a changing discipline. It has to be said that, at the 1961 conference of the German Sociological Association, neither the papers given by Adorno and Popper, nor the ensuing discussion, led to a more precise definition of epistemological positions, thus disappointing the expectations of many participants, as Ralf Dahrendorf writes in his report on the conference.[3] As regards the "clear establishment of the relationships of theory and empirical research, of construction, analysis and data-gathering",[4] it is perhaps not surprising that the speakers — as well as the opponents, Albert and Habermas, in the continuation of the controversy — achieved relatively little in their endeavour to resolve these problems. They were problems that were of little or no consequence for their research practice.

They are, however, exactly the questions with which a sociologist as strongly influenced by empirical work as Bourdieu was, saw and still sees himself confronted. Thus the epistemological reflection in *The Craft of Sociology*, which is, above all, self-reflection, emphasizes quite different aspects than the German *Positivismusstreit*, despite its closeness in time. The question now is why a book such as this is still of interest today — an interest that goes beyond the fact that it expounds the epistemological foundation of the work of one of the most innovative contemporary sociologists, a sociologist who has shed new light on the nature of the social world. Since the book was first published, empirical social research has developed enormously, which has led to a significant change in sociology; and epistemological discussion has continued. What then are the reasons today for reading this book and translating it?

The epistemological position set out in *The Craft of Sociology* is in at least two respects still new for sociology. Firstly, a central concern of the book is an aspect of research logic which is seldom discussed, namely the logic of discovery as opposed to the logic of validation — so far as that distinction can be sustained. The rigorous demarcation of two distinct logics which the research process follows in its different stages, assigns a major aspect of scientific work — hypothesis-building, the scientific "idea", the generation of ideas guiding research, or whatever name one gives to these aspects of the research process that are generally placed outside the logic of validation, which is regarded as scientific in the strict sense — to the realm of chance, intuition, the purely individual and non-rational. One can safely assume a consensus within the

[3] Cf. R. Dahrendorf, "Anmerkungen zur Diskussion der Referate von Karl R. Popper und Theodor W. Adorno", in : T. W. Adorno et al., *Der Positivismusstreit in der deutschen Soziologie*. Darmstadt and Neuwied: Luchterhand 1980, pp. 145-153.

[4] R. Dahrendorf, *op. cit.*, p. 153.

scientific community that, as Max Weber wrote in "Science as a Vocation", the "inspiration", the "idea", is the decisive factor in winning accomplishments for science.[5] After adding that "normally . . an 'idea' is prepared only on the soil of very hard work", Weber dismisses the problem; and this aspect of the research process has been excluded from subsequent discussions of scientific procedures, which are based on the distinction between the context of validation and the context of discovery. More recent studies in the sociology of science, however, show that the actual research process does not manifest this distinction. The process of validating scientifically generated knowledge extends far back into the context of discovery and cannot be separated from it. Pierre Bourdieu and his co-authors are explicitly concerned to remove the scientific "notion", the "idea", the "process of hypothesis-building", from the realm of intuition, and to make it amenable to reason in an *ars inveniendi*. They speak of scientific "invention", thereby recalling a figure of classical rhetoric.

Central to this *ars inveniendi* is the construction of the scientific object, and this is the second, and essential, reason that makes this book so stimulating to read. Discussing the construction of the scientific object involves two aspects: on the one hand, the "constructedness" of knowledge; on the other, the construction of a specific scientific object. Scientifically grounded knowledge does not appear as a faithful reflection of a "reality", but as a fabricated knowledge, generated only through the work of the scientist, in other words a constructed knowledge. In the construction of the scientific object, it is of prime importance to break free from the representations, questions and problem formulations of common-sense understanding, i.e., as the authors themselves write, to break with "pre-notions" and "preconcepts", and to develop instead an autonomous object area which has systematic foundations and which formulates its own questions. A constructivist position such as this is by no means self-evident in contemporary sociology; even discussion about it has only just begun.[7] It is rather the case that sociology is to a large extent characterized by the fact that it simply takes over prescientific definitions of problems, often from the political sphere. In the interview included in this volume, Bourdieu cites examples of the simple taking-over of prescientific problem definitions. He points out that some "sociologies-of . . . ." mainly owe their existence to the public salience of certain social problems, and

[5] M. Weber, "Science as a Vocation", in *From Max Weber: Essays in Sociology*, trans. H. H. Gerth and C. W. Mills, London: Routledge & Kegan Paul, 1948, pp. 129–56. (The quotation is from p. 135.)

[6] Cf. K. D. Knorr, *The Manufacture of Knowledge: An Essay on the Constructivist and Contextual Nature of Science*, Oxford: Blackwell, 1980, and B. Latour and S. Woolgar, *Laboratory Life: The Social Construction of Scientific Facts*, London: Sage, 1979.

[7] In a recent article, written from the standpoint of the sociology of knowledge, K. Knorr-Cetina outlines the prospects for constructivism in sociology ("Spielarten des Konstruktivismus", *Soziale Welt*, Vol. 40, 1989, no. 1–2, pp. 86–96).

achieve no status beyond that of "public-concern" sociologies or technocratic management, so long as they remain within the confines of common-sense problem formulations. Only this break, the move beyond prescientific concepts and questions, can, incidentally, succeed in placing a systematic barrier against *ex-cathedra* prophesying, a temptation that sociologists cannot resist (except with the aid of their personal scruples) so long as they move in the same prescientific realm as the wider public. The authors discuss this problem in the first part of the book, which is devoted to the "break".

The above is not intended to bolster the misapprehension that scientificity is characterized above all by the use of complex technical terminologies, and by the distance between research and the problems of practice; and it does not claim that social agents are blind with regard to their social practice. The common-sense knowledge of the agents, their "practical sense",[8] is the starting point for any sociological understanding. This knowledge is, however, limited, and to that extent sociological knowledge, which strives to overcome these limitations, is always a clarification of social practice. But this clarification cannot be achieved simply by reproducing the promptings of practical sense or common-sense understanding. One needs analysis which shows — constructs — the relationships and connections that remain hidden within the wealth of appearances and actions of everyday life. In their argument, Bourdieu, Chamboredon and Passeron link up with an epistemological tradition which evolved mainly in France and is represented by names like Alexandre Koyré, Gaston Bachelard and Georges Canguilhem. With the exception of the work of Koyré, this tradition has so far largely been ignored in both Anglo-American and German sociological discussion. This volume includes several illustrative texts by Bachelard and Canguilhem. The conception of science put forward in this book was decisively influenced by these thinkers' work in the history of science, which is centred on the "epistemological obstacles", as Bachelard calls them — the social and mental obstacles standing in the way of scientific knowledge — and also on the construction of the autonomous scientific object.

Those who know Pierre Bourdieu's work will easily recognize how his understanding of the social world parallels his understanding of science. Society, social practice, the social facts or social structures and institutions with which sociology deals, are something that the subjects themselves make, something that, as Bourdieu constantly points out, they constitute, realize, modify and transform through their activity: nothing social exists outside the action of the subjects. To avoid a subjectivist reading of this position, it should be noted that in his understanding of the social world Bourdieu echoes Marx: he does not confine the social to interactions, but insists on the autonomy of institutions and social structures as against the intentions and will of the

[8] Cf. P. Bourdieu, *The Logic of Practice*, Oxford: Polity Press, 1990 [*Le Sens pratique*, Paris: Éditions de Minuit, 1980].

subjects. However, his main concern, in contrast to Marx, is not with these autonomous entities, the consolidated product of the social activity of individuals, but rather with the "practical operators" which reproduce and transform through their practice the social constructions encountered by each generation.[9] Here the concept of the habitus, the key concept in Bourdieu's work, has its origin and its distinctive value: being itself structured through social relations, it functions as the practical operator through which the action of the subjects becomes social action, the practical construction of the social world. And sociology, as the science which is concerned with the understanding of the social world, is a product of construction, construction of knowledge about an object which is itself already a construct.

It is tempting, in the case of sociology, to speak of a double construction — construction at the level of the agents, and scientific meta-construction. But that approach would fail to grasp the understanding of sociology and sociological methods presented in this book. Sociologists do not stand outside the social world they analyse, or look down on it from above; they are themselves agents in the social process. What enables them — to some extent — to analyse this process, i.e. to constitute objects for scientific understanding — is quite simply scientific method, but not a fundamental difference in their position from that of other social agents. So Bourdieu and his co-authors are quite consistent when, in the final pages of their book, they emphasize the importance, for sociological knowledge, of a sociology of sociology. The authors are following Bachelard when they state that one of the basic elements of "epistemological vigilance" is reflection by the scientific subject on his or her own social relations, a reflection which not only applies to the fact that sociologists are subject to the ebbs and flows of the *Zeitgeist*, that they carry ways of thinking and prejudices that they owe to their social origin, the social position they have acquired, and their specific roles as intellectuals, but also to the social order within their own scientific community and their own position within it. Thus, the quality of sociological research depends, as it does in other sciences, on the organization and functioning of the scientific community, on the social practice of those who have science as a vocation.

[9] Whereas Marx and Engels, in their confrontation with the idealistic view of history, write that, in history, "at each stage there is found a material result: a sum of productive forces, an historically created relation of individuals to nature and to one another, which is handed down to each generation from its predecessor; a mass of productive forces, capital funds and conditions, which, on the one hand, is indeed modified by the new generation, but also prescribes for it its conditions of life and gives it a definite development, a special character [. . . .] circumstances make men as much as men make circumstances" (K. Marx & F. Engels, *The German Ideology*, in *Collected Works*, Vol. 5, London: Lawrence & Wishart, 1976, p. 50), Bourdieu stresses the mechanism or process through which, in practices, in "practical life", as Marx and Engels put it, those "circumstances" are constructed. While the meaning of social activity is objectified in institutions, it is the existence and functioning of the habitus, embodied history, "that makes it possible to inhabit institutions, to appropriate them practically, and so to keep them in activity, continuously pulling them from the state of dead letters, reviving the sense deposited in them, at the same time imposing on them the revisions and transformations that reactivation entails" (P. Bourdieu, *op. cit.*, 1990, p. 57).

Preface to the second French edition

This volume on the *Epistemological preliminaries* was originally intended to be followed by a second volume on the construction of the sociological object and a third volume giving a critical review of the conceptual and technical tools of research. The preparation of this abridged second edition has been an opportunity to reconsider that original intention. In the end, it seemed to us that in those other areas it was impossible to perform the work of construction that was made both possible and necessary by the non-existence of an epistemology of the social sciences. In such crowded, not to say over-crowded, domains, the stance of deliberate naïveté could not be sustained; but we were equally unwilling to resign ourselves to the even-handed discussion of prevailing theories and concepts which the university tradition has established as the preliminary to any theoretical discussion.

We might have been more tempted to revise these *Epistemological preliminaries* so as to bring the exposition more into line with the didactic intention, which is very imperfectly fulfilled in the present state of the work. Each of the principles might have been translated into precepts or, at least, into exercises designed to internalize the posture. For example, in order to draw out all the heuristic potential of a principle such as the primacy of relations, we would have had to show with concrete examples (as one can in a seminar, or better in a research group, by examining the construction of a sample, the design of a questionnaire or the analysis of a series of statistical tables) how this principle governs the technical choices made in research work (constructing series of populations separated by differences pertinent in respect of the relations being considered; devising questions which, while secondary as regards the sociography of the population itself, enable one to situate the particular case in a system of cases in which it takes on its full meaning; using graphic and mechanical techniques to provide a synoptic and exhaustive view of the system of relations linking the relations revealed by a set of statistical tables, etc.).

We were dissuaded from doing so, among other reasons by the fear that, owing to the limits of written communication, this effort at didactic clarification might lead to the very negation of the teaching of research conceived as the teaching of invention, by encouraging the canonization of the routinized precepts of a new methodology or, worse still, a new theoretical tradition. The risk is a real one: the critique of positivist empiricism and methodological abstraction, which was heretical in its day, now has every chance of being confused with the eternally preliminary discourses of a new vulgate that still manages to defer science by substituting the point of honour of theoretical purity for the obsession of methodological impeccability.

September 1972

Contents

Illustrative Texts

Introduction—Epistemology and methodology

Part One—The break

Part Two—Constructing the object

Part Three—Applied rationalism

Conclusion—Sociology of knowledge and epistemology

The illustrative texts that make up the second part of this book (pp. 79–245) should be read in conjunction with the analyses in the course of which they are used or explained. Cross-references to these texts, in the first part of the book (in italics, in square brackets), give the author's name and the number of the text.

Introduction
Epistemology and methodology

"Method," wrote Auguste Comte, "does not admit of being studied apart from the research in which it is used; or, at all events, it is only a lifeless study, incapable of fertilizing the mind which resorts to it. Looking at it in that abstract way, the only real information you can give about it amounts to no more than a few general propositions, so vague that they can have no influence on mental habits. When we have thoroughly established as a logical thesis that all our knowledge must be founded upon observation, that we must proceed sometimes from facts to principles, at other times from principles to facts, and some other similar aphorisms, we still know method far less clearly than he who, even without any philosophical purpose in view, has studied at all completely a single positive science. It is because they have failed to recognize this essential fact that our psychologists have been led to take their reveries for science, in the belief that they understood the positive method because they have read the precepts of Bacon or the discourse of Descartes. I do not know if, in the future, it will become possible to construct by *a priori* reasoning a genuine course on method, wholly independent of the philosophical study of the sciences; but I am quite convinced that it cannot be done at present, for the great logical methods cannot be explained with sufficient precision apart from their applications. I venture to add, moreover, that, even if such an enterprise could be carried out eventually, which is conceivable, it would nevertheless be only through the study of regular applications of scientific methods that we could succeed in forming a good system of intellectual habits; this is, however, the essential object to be gained by studying method."[1]

There would be nothing to add to this text, which refuses to dissociate method from practice and rejects in advance all discourses on method, if there were not already a whole discourse around method which, in the absence of any serious challenge, is liable to force on researchers a split image of scientific work. Whether they are prophets who fulminate against the original impurity of the empirical—though it is not clear whether they regard the menial tasks of scientific routine as offensive to the dignity of the object they assign to them-

[1] A. Comte, *Introduction to Positive Philosophy*, ed. with introdn. and rev. transln. by F. Ferré, Indianapolis: Bobbs-Merrill, 1970, p. 23. As Georges Canguilhem points out, it is not easy to resist the impulses of vocabulary, which "constantly lead us back to an idea of method as something that can be separated from the research in which it is implemented: Comte teaches in the first lesson of the *Cours de philosophie positive* that 'method cannot be studied apart from the research in which it is *used*'—which implies that use of a method presupposes prior possession of the method" (G. Canguilhem, "Théorie et technique de l'expérimentation chez Claude Bernard", *Colloque du centenaire de la publication de l'Introduction à l'étude de la médecine expérimentale*, Paris: Masson, 1967, p. 24).

selves or of the scientific mind they seek to incarnate—or high priests of method who would gladly make all researchers spend the rest of their lives on the benches of the methodological catechism, those who pontificate on the art of being a sociologist or the scientific way of conducting sociological science often have in common the fact that they dissociate method, or theory (not to mention the theory of method or the theory of theory) from the operations of research. Our aim in this book, arising from our experience of research and its everyday difficulties, is simply to make explicit, for the purpose in hand, a "system of intellectual habits". It is addressed to those who have embarked on the practice of empirical sociology and have no need to be reminded of the necessity of measurement and all its theoretical and technical apparatus. They will immediately grant us what we ourselves assume, because it goes without saying: for example, the need to neglect none of the conceptual or theoretical tools that enable one to give experimental verification its full rigour and its full force. Only those who do not have or do not want experience of research will see this work, which seeks to put sociological practice to the question, as calling empirical sociology into question.[2]

If it is true that the teaching of research requires both its teachers and its students to make constant and direct reference to personal experience of practice, then "the vogue in unapplied methodology, programmes for hypothetically superior research, evaluatory surveys of work done by others... and all such instruments and occasions for methodological pronouncements"[3] can be no substitute for reflection on the correct relation to techniques and for even a risky effort to transmit principles that cannot present themselves as *a priori* truths because they are the principle of the search for truths. And if it is true that methods are distinguished from techniques at least insofar as they are "sufficiently general to be common to all sciences, or to a significant part of them",[4] then this reflection on method must also take the risk of retracing the most classic analyses of the epistemology of the natural sciences: but perhaps sociologists need to agree on the elementary principles that are truisms for

[2] The division of the intellectual field in accordance with the logic of opposing couples (see Part Three) and the intellectual traditions which treat all reflection as pure speculation, making it impossible to see the technical function of a reflection on the relation to techniques, give a very high probability to the misunderstanding which we are here trying to prevent. In this dualistic organization of epistemological positions, any attempt to reinsert technical operations into the hierarchy of epistemological acts will almost inevitably be interpreted as an attack on technique and technicians. Whether we like it or not—and we would acknowledge here the important contribution which the methodologists, and in particular Paul F. Lazarsfeld, have made to the rationalization of sociological practice—, we realize that we are likely to be classified among the "Fads and Foibles of American Sociology" rather than "The Language of Social Research".

[3] R. Needham, *Structure and Sentiment: A Test-case in Social Anthropology*, Chicago & London: Chicago University Press, 1962, p. vii.

[4] A. Kaplan, *The Conduct of Inquiry: Methodology of Behavioral Science*, San Francisco: Chandler, 1964, p. 23. The same author expresses regret that "technology" already has a specialized meaning, since it could otherwise well be used to describe a good number of "methodological studies" (*ibid.*, p. 19).

natural scientists or philosophers of science in order to escape from the conceptual anarchy to which they are condemned by their indifference to epistemological reflection. In reality, the effort to interrogate a particular science with the aid of the general principles that are provided by this epistemological heritage is particularly justified and necessary in the case of sociology. Here everything encourages neglect of that heritage, from the humanist stereotype of the irreducibility of the human sciences, to the characteristics of the recruitment and training of researchers, not to mention the existence of a corps of methodologists specializing in selective reinterpretation of the heritage of the other sciences. So we must subject the operations of sociological practice to the polemics of epistemological reason in order to define and, if possible, inculcate an attitude of vigilance that can use adequate knowledge of error and the mechanisms that can induce it as one of the means of overcoming it. The intention of giving the researcher the means of taking on the oversight of his own scientific work is quite different from the calls to order by censors whose peremptory negativism can only inspire the mortal fear of error and a resigned recourse to a technology invested with the function of exorcism.

As the whole *œuvre* of Gaston Bachelard shows, epistemology differs from abstract methodology inasmuch as it strives to grasp the logic of error in order to construct the logic of the discovery of truth as a polemic against error and as an endeavour to subject the approximated truths of science and the methods it uses to methodical, permanent rectification [*Canguilhem, text no. 1*]. But the polemical action of scientific reason cannot be given its full force unless the "psychoanalysis of the scientific mind" is taken further by an analysis of the social conditions in which sociological works are produced: the sociologist may find an exceptionally valuable instrument of epistemological vigilance in the sociology of knowledge, a means of enhancing and clarifying knowledge of error and the conditions that make it possible and sometimes inevitable [*Bachelard, text no. 2*]. It follows that any residues of what may seem to be *ad hominem* polemics that remain here are simply due to the limits of sociological understanding of the conditions of error. An epistemology that appeals to a sociology of knowledge is less entitled than any other to impute errors to subjects who are never entirely the authors of those errors. If, to paraphrase a famous text by Marx, "we have not painted a rosy picture" of the empiricist, the intuitionist, or the methodologist, we have never thought of the "persons except insofar as they are the personification" of epistemological positions that can only be fully understood in the social field in which they are put forward.

Teaching research

The form and content of this work are defined by its function. Research training which aims to set out the principles of professional practice and

simultaneously to inculcate a particular relationship to that practice, i.e. to give both the tools that are indispensable for the sociological treatment of the object and an active disposition to use them adequately, has to break with the routines of pedagogic discourse and restore their full heuristic force to the concepts and operations that have been most completely "neutralized" by the ritual of canonical exposition. That is why this work, which aims to teach the most practical acts of sociological practice, starts with a reflection that seeks to systematize and underline the implications of all practice, good or bad, and to specify the principle of epistemological vigilance in the form of practical precepts (Volume 1).[5] We shall then be in a position to try to define the function and conditions of application of the theoretical schemes to which sociology has to resort in order to construct its object—without claiming to present these first principles of specifically sociological inquiry as a complete theory of sociological knowledge, still less as a general and universal theory of the social system (Volume 2).[5] Empirical research has no need to implement such a theory in order to escape from empiricism, provided that, in each of its operations, it effectively realizes the principles that constitute it as a science by giving it an object endowed with a degree of theoretical coherence. On that condition, concepts or methods can be treated as *tools* that can be removed from their original context and put to new uses (Volume 3).[5] By associating the presentation of each intellectual instrument with examples of its use, we shall seek to prevent sociological knowledge from appearing as a catalogue of techniques, or a bank of concepts, separate or separable from their use in research.

If we have taken it upon ourselves to extract both the theoretical principles and the technical procedures bequeathed by the history of sociological science from the "order of reasons" to which they belonged, this was not done simply to break the linkages of the didactic order which only renounces academic indulgence towards the history of doctrines or concepts in order to grant diplomatic recognition to the values consecrated by tradition or fashion. Nor was it done merely to liberate heuristic potentialities that are often greater than academic usages would suggest. We have done so, above all, for the sake of a conception of the theory of sociological knowledge that makes this theory the system of principles defining the conditions of possibility of all distinctively sociological acts and discourses and only these, whatever the theories of the social system specific to those who produce or have produced sociological works in the name of these principles. The question of the affiliation of a piece of sociological research to a particular theory of the social system, that of Marx, or Weber, or Durkheim, for example, is always secondary to the question of whether that research belongs to sociological science. The only criterion of this is whether it implements the fundamental principles of the theory of sociological knowledge which, as such, in no way separates authors

[5] See above, Preface to the second French edition.

who differ in every respect as regards their theory of the social system. Even if most authors have been led to identify their particular theory of the social system with the theory of sociological knowledge that they involve, implicitly at least, in their sociological practice, the epistemological project can nonetheless use this preliminary distinction as a basis for juxtaposing authors whose doctrinal oppositions mask an epistemological agreement.

It might be feared that this undertaking will lead to an amalgam of principles borrowed from different theoretical traditions or to the establishment of a set of recipes dissociated from the principles that underlie them. But this would be to forget that the reconciliation whose principles we seek to make explicit really occurs in the authentic exercise of the sociologist's practice, or rather his "craft"—the *habitus*, which, as a system of more or less well-assimilated and more or less transposable schemes of thought, is nothing other than the internalization of the principles of the theory of sociological knowledge. The endlessly renewed temptation to transform the precepts of method into scientific recipes or laboratory gadgets can only be resisted by constant training in the scientific vigilance which, by subordinating the use of techniques and concepts to an examination of the conditions and limits of their validity, rules out the short cuts offered by automatic application of tried and tested procedures, and which teaches that even the most routine operation has to be rethought, both in itself and in relation to the particular case. Only a magical reinterpretation of the demands of measurement can lead one simultaneously to overestimate the importance of operations that are ultimately no more than tricks of the trade and—transforming methodological prudence into sacred reverence—never to use instruments that ought to be judged only by their use, or to use them only with trembling hands, for fear of falling short of one of the ritual conditions. Those who push methodological concern to the point of obsession are like Freud's patient who spent all his time cleaning his spectacles and never put them on.

To take seriously the project of methodically transmitting an *ars inveniendi* requires one to see that it implies something more than and different from the *ars probandi* proposed by those who confuse the mechanical logic of validations and proofs, dismantled after the event, with the real processes of invention. It also requires one to see, with equal clarity, that the paths, or rather short cuts, that a reflection on research can now trace are a far cry from the unwavering, unrepentant course that would be marked out by a genuine discourse on sociological method.

In contrast to the tradition which draws the line at the logic of proof, refusing on principle to enter into the arcana of invention, thereby condemning itself to oscillate between a rhetoric of formal exposition and a literary psychology of discovery, we try here to provide the means of acquiring a mental disposition which is the precondition for both invention and proof. If this reconciliation is not made, one must give up any hope of aiding the work

of discovery, and one is reduced, along with so many methodologists, to invoking the miracles of creative insight, as purveyed by the hagiography of scientific discovery, or the mysteries of depth psychology.[6]

Though it goes without saying that acquired automatic behaviours can reduce the need for permanent invention, it should not be supposed that the subject of scientific invention is an *automaton spirituale* guided by the built-in mechanisms of a methodological programming, set up once and for all. This would lock the researcher into a blind submission to the programme which rules out reflexive re-examination of the programme, the precondition for the invention of new programmes.[7] Methodology, said Weber, "is no more the precondition of fruitful intellectual work than the knowledge of anatomy is the precondition for 'correct' walking".[8] But, while it is futile to hope to discover a science of the way to do science or to expect logic to provide more than a way of controlling science in progress or of validating science that has been done, the fact remains that, as J. S. Mill observed, "invention can be cultivated". It follows that even a partial spelling-out of the logic of invention can help to rationalize the teaching of the capacity to invent.

Epistemology of the social sciences and epistemology of the natural sciences

Most of the errors that tend to arise both in sociological practice and in reflection on it have their roots in a misconceived view of the epistemology of

[6] When defining the object of scientific logic, the methodological literature always takes care explicitly to "leave aside... the question of *ways of discovery*": "For our purposes it will suffice to consider the scientific *ways of validation*" (C. Hempel, *Aspects of Scientific Explanation and Other Essays in the Philosophy of Science*, New York: Free Press; London: Collier–Macmillan, 1965, p. 83). Popper often returns to this dichotomy, which for him seems to correspond to the distinction between public and private life: "The question, 'How did you first *find* your theory?' relates, as it were, to an entirely private matter, as opposed to the question, 'How did you *test* your theory?'" (K. R. Popper, *The Poverty of Historicism*, London: Routledge & Kegan Paul, 2nd ed. 1960, p. 135). Or again: "There is no such thing as a logical method of having new ideas, or a logical reconstruction of this process. My view may be expressed by saying that every discovery contains 'an irrational element', or 'a creative intuition', in Bergson's sense" (K. R. Popper, *The Logic of Scientific Discovery*, London: Hutchinson, 1959, p. 32). By contrast, as soon as, exceptionally, one explicitly considers the "context of discovery" (as opposed to the "context of justification"), one is forced to break with a number of routine conceptions of the epistemological and methodological tradition, in particular with the image of the process of research as a succession of distinct, predetermined stages (see P. E. Hammond [ed.], *Sociologists at Work: Essays on the Craft of Social Research*, New York: Basic Books, 1964).

[7] Consider, for example, the ease with which research can reproduce itself without producing anything, in accordance with the logic of "pump-handle research".

[8] M. Weber, "Critical Studies in the Logic of the Cultural Sciences", in *The Methodology of the Social Sciences*, trans. and ed. E. A. Shils and H. A. Finch, New York: Free Press, 1949, p. 115.

the natural sciences and its relationship to the epistemology of the social sciences. Epistemologies as different in their manifest assertions as Diltheyan dualism, which is able to posit the specificity of the method of the social sciences only by contrasting it with an image of the natural sciences that springs from a pure concern for distinction, and positivism, which endeavours to mimic an image of the natural sciences that is devised for the purposes of imitation, are unequally unaware of the exact philosophy of the exact sciences. This misperception has led some people to invent artificial distinctions between the two methods in order to indulge humanistic nostalgia or pious wishes; led others to herald discoveries that are mere rediscoveries; and still others into a positivism that naively copies a reductive image of experience as a copy of the real.

But it is all too obvious that positivism only takes over a caricature of the method of the exact sciences, without *ipso facto* achieving an exact epistemology of the social sciences. Indeed, it is a constant feature of the history of ideas that the critique of mechanical positivism serves to strengthen the subjective character of social facts and their irreducibility to the rigorous methods of science. Thus, having observed that "the methods which scientists or men fascinated by the natural sciences have so often tried to force upon the social sciences were not always necessarily those which the scientists followed in their own field, but rather those which they believed that they employed",[9] Hayek immediately concludes that the facts of the social sciences "differ from the facts of the physical sciences in being beliefs or opinions held by particular people", and therefore "must not be defined in terms of what we might find out about them by the objective methods of science, but in terms of what the person acting thinks about them".[10] Any questioning of automatic imitation of the natural sciences is so automatically associated with the subjectivist critique of the objectivity of social facts that every effort to deal with the specific problems raised by transposing the epistemological heritage of the natural sciences to the social sciences is always liable to be seen as a reaffirmation of the imprescriptible rights of subjectivity.[11]

[9] F. A. Hayek, *The Counter-Revolution of Science: Studies on the Abuse of Reason*, Glencoe (Ill.): Free Press, 1952, p. 14.

[10] *ibid.*, pp. 28 and 30.

[11] And yet Durkheim's whole enterprise would be sufficient to show that it is possible to escape from the dilemma of blind imitation and the equally blind refusal to imitate: "Sociology had its origin in the shadow of the natural sciences, and in intimate contact with them... . To be sure, some of the first sociologists made the mistake of exaggerating this close connection to the point that they ignored the originality of the social sciences and the autonomy which they must enjoy vis-à-vis the sciences that preceded them. But such excesses must not make us forget how much there is that is fruitful in the natural sciences, those principal forges of scientific thought" (É. Durkheim, "Sociology and its Scientific Field", in *Émile Durkheim, 1858–1917*, ed. K. H. Wolff, Columbus (Ohio): Ohio State University Press, 1960, p. 373).

Methodology and the displacement of vigilance

The way to move beyond these academic debates, and beyond the academic way of moving beyond them, is to subject scientific practice to a reflection which, unlike the classical philosophy of knowledge, is applied not to science that has been done—*true* science, for which one has to establish the conditions of possibility and coherence or the claims to legitimacy—but to science in progress. This specifically epistemological task consists in discovering, within scientific practice itself, which is constantly confronted with error, the conditions in which one can extract the true from the false, moving from a less true to a more true knowledge, or rather, as Bachelard puts it, an "approximated, that is to say, rectified, knowledge". Transposed to the social sciences, this philosophy of scientific work as the "unceasing polemical action of reason" can yield the principles of a reflection capable of inspiring and controlling the concrete acts of a truly scientific practice, by defining the specificity of the principles of the "regional rationalism" characteristic of sociological science. The fixist rationalism that inspired the inquiries of the classical philosophy of knowledge is more often expressed nowadays in the endeavours of some sociologists who tend to reduce reflection on method to a formal logic of the sciences. However, as Paul Feyerabend points out, "any form of meaning invariance is bound to lead to difficulties when the task arises... to give a proper account of the growth of knowledge, and of discoveries contributing to this growth".[12] More precisely, an exclusive concern with the atemporal relationships between abstract propositions, at the expense of the processes through which each proposition or concept has been established and has given rise to others, can be of no help to those who are involved in the drama of scientific work, because all the action takes place behind the scenes and only the denouement is brought on to the stage. Entirely occupied with the search for an ideal logic of research, the methodologists can only address themselves to a researcher abstractly defined by the capacity to achieve these standards of perfection—an impeccable, i.e. impossible or infertile, researcher. Unconditional obedience to an *organon* of logical rules tends to produce an effect of "premature closure" by removing, as Freud puts it, "the elasticity of definitions" or what Carl Hempel calls the "openness of meaning" of scientific terms[13]—which, at least in certain phases in the history of a science or the unfolding of research, is one of the conditions of invention.

This is not to deny that logical formalization, treated as a means of testing the ongoing logic of research and the coherence of its results, is one of the

[12] P. K. Feyerabend, "Explanation, Reduction and Empiricism", in H. Feigl and G. Maxwell (eds.), *Scientific Explanation, Space, and Time*, (Minnesota Studies in the Philosophy of Science, Vol. III,) Minneapolis: University of Minnesota Press, 1962, p. 31.

[13] C. G. Hempel, *Fundamentals of Concept Formation in Empirical Science*, Chicago and London: Chicago University Press, 1952, p. 29.

most effective instruments of epistemological control. But this legitimate usage of logical instruments is too often exploited to justify the perverse passion for methodological exercises which have no discernible purpose beyond that of displaying the arsenal of available means. Faced with some research designed for the sake of the logical or methodological cause, one is reminded of Abraham Kaplan's story of the "drunkard searching under a street lamp for his house key, which he had dropped some distance away. Asked why he didn't look where he had dropped it, he replied 'It's lighter here!'"[14] [*Kaplan, text no. 3*].

The cult of technological rigour, based on faith in a rigour defined once and for all and for all situations, i.e. on a fixist representation of truth or, equally, of error as the transgression of unconditional norms, is diametrically opposed to the search for *specific rigours*, which is based on a theory of truth as rectified error. "The knowing," says Bachelard, "has to evolve with the known." It is therefore futile to seek a logic prior to and external to the history of science in progress. To understand the processes of research, one has to examine how it proceeds, instead of enclosing it in the observance of a decalogue of procedures that may appear to be in advance of real practice only because they are defined in advance.[15] "Fascinated by the fact that in mathematics the avoidance of error is a matter of technique, some people claim to define truth as the product of an intellectual activity that complies with certain norms; they want to treat experimental data as mathematicians treat the axioms of geometry; they hope to define rules of thought that would fulfil the same role that logic fulfils in mathematics. Starting from a limited experiment, they want to define its theory in a single operation. Calculus was very slowly established, the idea of number took two and a half millennia to be clarified. The procedures that establish rigour arise as answers to questions which cannot be posed *a priori* and which only the development of science brings to light. Naïveté is lost very slowly. This is true in mathematics and it is true *a fortiori* in sciences based on observation, in which every disproved theory suggests new demands of rigour. It is therefore futile to seek to lay down *a priori* the conditions for truly scientific thought."[16]

At a deeper level, the insistent calls for methodological perfection are likely to lead to a *displacement* of epistemological vigilance. Instead, for example, of questioning the object of measurement and asking whether it is worth measuring, and how much accuracy is desirable and legitimate in view of the particu-

[14] A. Kaplan, *op. cit.*, p. 11.

[15] The authors of a long study of the functions of the statistical method in sociology eventually admit that their "suggestions regarding the possibility of applying statistical theory to empirical research only characterize the present state of methodological discussion, *with practice lagging behind*" (E. K. Scheuch and D. Rüschemeyer, "Soziologie und Statistik. Über den Einfluß der modernen Wissenschaftslehre auf ihr gegenseitiges Verhältnis", *Kölner Zeitschrift für Soziologie und Sozialpsychologie*, Vol. 8, pp. 272–291, 1956).

[16] A. Régnier, *Les infortunes de la raison*, Paris: Seuil, 1966, pp. 37–38.

lar conditions of measurement, or even, more simply, examining whether the instruments measure what they are supposed to measure, one may be carried away by the wish to translate the pure idea of methodological rigour into realizable tasks and, obsessed by the decimal, pursue the contradictory ideal of an intrinsically indefinable accuracy. This would be to forget that, as A. D. Ritchie points out, "it is really just as bad technique to make a measurement more accurately than is necessary as it is to make it not accurately enough",[17] or that, as N. R. Campbell observes, when it is established that all propositions contained within certain limits are equivalent and that the proposition, defined approximately, lies within these limits, "approximation is completely justified".[18] It is clear that, by generating a casuistry of technical errors, the ethic of methodological duty can lead, indirectly at least, to a procedural ritualism which may be the caricature of methodological rigour but which is certainly the exact opposite of epistemological vigilance.[19] It is particularly significant that statistics, the science of error and of approximated knowledge, which, in such standard procedures as calculating error or the limits of reliability, implements a philosophy of critical vigilance, is so frequently used as a scientific alibi for blind submission to technical instruments.

Similarly, whenever theoreticians summon empirical research and its conceptual tools to the bar of a theory whose constructions they refuse to measure against the heritage of the science it claims to theorize and rule over, it is only the prestige indiscriminately attached to any theoretical undertaking that wins them the forced and purely verbal homage of the practitioners. And whenever the intellectual climate enables the pure theoreticians to foist on scientists their own logical or semantic ideal of complete, universal coherence of the system of concepts, they can even paralyse research, inasmuch as they manage to instil the obsession of conceptualizing everything, in every way, and in all respects at once. In the real situations of scientific practice, the construction of new problematics or theories requires one to renounce the impossible ambition of saying everything about everything, in the right order.[20]

[17] A. D. Ritchie, *Scientific Method: An Inquiry into the Character and Validity of Natural Laws*, London: Kegan Paul, Trench, Trubner, 1923, p. 113. Analysing this pursuit of "ill-founded precision", which consists in believing that "the quality of the solution is measured by the number of decimal places", Bachelard points out that "when accuracy in a result is greater than the accuracy of the experimental data, it is quite precisely the determination of nothing... . As Dulong said ironically of an experimenter, 'He is quite sure of the third figure after the decimal point, it's the first one that worries him'" (G. Bachelard, *La formation de l'esprit scientifique*, 4th edn., Paris: Vrin, 1965, p. 214).

[18] N. R. Campbell, *An Account of the Principles of Measurement and Calculation*, London & New York: Longmans Green, 1928, p. 186.

[19] Anxious interest in diseases of the scientific mind can have as depressive an effect as hypochondriac addiction to reading medical dictionaries.

[20] Some theoretical dissertations on everything that is known or knowable no doubt perform a function of pre-emptive annexation similar to that of astrological prophecies, which always manage to subsume events retrospectively. As Claude Bernard put it: "There are some people who, on any given question, say all that can be said in order to be able to stake a claim when an

The epistemological order of reasons

But these sociological or psychological analyses of the methodological perversion or the speculative diversion are no substitute for the specifically epistemological critique to which they point. The warnings issued by the methodologists have to be strongly warned against, because, by focusing attention exclusively on formal controls of experimental procedures and operational concepts, they are likely to divert vigilance from more serious dangers. The sometimes powerful instruments and assistance that methodological reflection provides for vigilance turn against vigilance whenever the preconditions for their use are not fulfilled. The science of the formal conditions of rigour in scientific operations, presenting itself in the guise of an "operational" version of epistemological vigilance, may seem to be grounded in the claim to provide automatic implementation of the principles and precepts that define methodological vigilance, so that additional vigilance is required in order to prevent it from automatically producing this displacement effect.

As Saussure put it, "the linguist needs to be shown what he is doing."[21] To ask what it means to practise science, or rather, to try to know what the scientist does (whether or not he knows what he is doing) is not only to inquire into the efficacy and formal rigour of the available theories and methods but also to question the methods and theories at the very moment at which they are implemented, in order to determine what they do to objects and what objects they make. The order in which this inquiry has to be conducted is imposed as much by epistemological analysis of the obstacles to knowledge as by sociological analysis of the epistemological implications of present-day sociology, which define the hierarchy of epistemological dangers and therefore the order of priorities.

Bachelard's premise that *the scientific fact is won, constructed, and confirmed* calls into question both the empiricism which reduces the scientific act to one of validation and the conventionalism which sets against it only the preliminary of construction. By so much emphasizing the imperative of validation, in opposition to the whole speculative tradition of the social philosophy from which it seeks to break away, the sociological community is now tending to forget the epistemological hierarchy of scientific acts which subordinates validation to construction and construction to the break with self-evident appearances. In an experimental science, a simple appeal to experimental proof is a mere tautology if one does not, at the same time, explicate the theoretical

experiment is subsequently carried out on it. They are like astronomers who plot planets all over the sky so as to claim that any new one was the planet they had predicted" (*Principes de médecine expérimentale*, Paris: P.U.F., 1947, p. 255).

[21] É. Benveniste, "Lettres de Ferdinand de Saussure à Antoine Meillet", *Cahiers Ferdinand de Saussure*, Vol. 21, pp. 92–135, 1964.

principles which are the basis of genuine experimentation; and this explication itself has no heuristic value if one does not, at the same time, make explicit the epistemological obstacles which present themselves in a specific form in each scientific practice.

Part One
The break

1 The social fact is won against the illusion of immediate knowledge

Epistemological vigilance is particularly necessary in the social sciences, where the separation between everyday opinion and scientific discourse is more blurred than elsewhere. Though it is readily acknowledged that their concern for political and moral reform of society may have led the 19th century sociologists to fall short of scientific neutrality, and even that 20th century sociology may have renounced the ambitions of social philosophy without thereby escaping ideological contaminations of another sort, the very critics who are so quick to point this out all too often fail to recognize and realize the full implications of the fact that, for the sociologist, familiarity with his social universe is the epistemological obstacle *par excellence*, because it continuously produces fictitious conceptions or systematizations and, at the same time, the conditions of their credibility. The sociologist's struggle with spontaneous sociology is never finally won, and he must conduct unending polemics against the blinding self-evidences which all too easily provide the illusion of immediate knowledge and its insuperable wealth. The separation between perception and science, which is expressed for the physicist in the opposition between the laboratory and daily life, is even harder for the sociologist to make, because his theoretical heritage does not provide him with the tools that would make it possible to radically challenge ordinary language and everyday notions.

1.1 Prenotions and techniques for breaking with them

Because they have the function of reconciling everyday consciousness with itself at all costs by offering explanations (even if they are mutually exclusive) of the same fact, spontaneous opinions on social facts present themselves as a spuriously systematized collection of judgements for alternating use. These preconceptions or "prenotions"—"schematic, summary representations" that are "formed by and for experience"—derive their self-evidence and their "authority", as Durkheim observes, from the social functions that they fulfil [*Durkheim, text no. 4*].

Everyday notions are so tenacious that all the techniques of objectification have to be applied in order to achieve a break that is more often proclaimed than performed. Thus, the results of statistical measurement may have at least