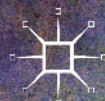


*Epistemology,  
Fieldwork, and  
Anthropology*

*Jean-Pierre  
Olivier de Sardan*



EPISTEMOLOGY, FIELDWORK,  
AND ANTHROPOLOGY

EPISTEMOLOGY, FIELDWORK,  
AND ANTHROPOLOGY

Jean-Pierre OLIVIER de SARDAN

Translated by Antoinette TIDJANI ALOU

palgrave  
macmillan



EPISTEMOLOGY, FIELDWORK, AND ANTHROPOLOGY

Copyright © Jean-Pierre Olivier de Sardan 2015

Softcover reprint of the hardcover 1st edition 2015 978-1-137-48849-7

All rights reserved.

First published in 2015 by PALGRAVE MACMILLAN® in the United States—a division of St. Martin's Press LLC, 175 Fifth Avenue, New York, NY 10010.

Where this book is distributed in the UK, Europe, and the rest of the world, this is by Palgrave Macmillan, a division of Macmillan Publishers Limited, registered in England, company number 785998, of Houndmills, Basingstoke, Hampshire RG21 6XS.

Palgrave Macmillan is the global academic imprint of the above companies and has companies and representatives throughout the world.

Palgrave® and Macmillan® are registered trademarks in the United States, the United Kingdom, Europe and other countries.

ISBN 978-1-349-69593-5 ISBN 978-1-137-47788-0 (eBook)

DOI 10.1057/9781137477880

Library of Congress Cataloging-in-Publication Data

Olivier de Sardan, Jean-Pierre.

Epistemology, fieldwork, and anthropology / Jean-Pierre Olivier de Sardan ; translated by Antoinette Tidjani Alou.

pages cm

Includes bibliographical references and index.

1. Anthropology—Methodology. 2. Anthropology—Fieldwork.  
3. Anthropology—Philosophy. 4. Social epistemology. I. Title.  
GN33.O48 2015  
301.072'3—dc23

2014044626

A catalogue record of the book is available from the British Library.

Design by Amnet.

First edition: May 2015

10 9 8 7 6 5 4 3 2 1

# CONTENTS

List of Figures and Tables	vii
1 Introduction: Empirical Adequacy, Theory, Anthropology	1
2 The Policy of Fieldwork: Data Production in Anthropology and Qualitative Approaches	21
3 Emic and the Actors' Point of View	65
4 From Observation to Description	83
5 The Methodological "I": Implication and Explicitation in Fieldwork	103
6 Methodological Populism and Ideological Populism in Anthropology	133
7 The Violence Done to Data: On a Few Figures of Overinterpretation	167
8 Conclusion: Common Sense and Scholarly Sense	195
Postface: Researcher and Citizen: Science and Ideology	211
Notes	215
References	243

## LIST OF FIGURES AND TABLES

### Figures

1.1	Empirical adequacy	3
2.1	Data production	25
2.2	Traces of data in the final text	61
3.1	One emic for two etics	78
5.1	The three levels of anthropological subjectivity	113

### Tables

2.1	Field inquiry and inquiry through questionnaires	22
3.1	The emic/etic oppositions	69
3.2	Harris final classification	69

“The results of scientific research in any branch of learning ought to be presented in a manner absolutely candid and above board . . . In ethnography, where a candid account of such data is perhaps even more necessary, it has unfortunately in the past not always been supplied with sufficient generosity . . . I consider that only such ethnographic sources are of unquestionable scientific value in which we can clearly draw the line between, on the one hand, the results of direct observations and of native statements and interpretations, and on the other the inferences of the author, based on his common sense and psychological insight.” (Bronislaw Malinowski, *Argonauts of the Western Pacific*, 1922)

“It’s a capital mistake to theorise before one has data. Insensibly one begins to twist facts to suit theories, instead of theories to suit facts.” (Sherlock Holmes, in Arthur Conan Doyle, *Scandal in Bohemia*, 1891)

## CHAPTER 1



# INTRODUCTION: EMPIRICAL ADEQUACY, THEORY, ANTHROPOLOGY<sup>1</sup>

### RIGOR AND APPROXIMATION IN ANTHROPOLOGY

#### Rigorous Approximation . . .

This expression is not as paradoxical as it may appear. What it highlights is a combination/conjunction rather than a simple contradiction. In fact, this book is entirely devoted to the following argument: On the one hand, an anthropological or sociological text must be rigorous (or otherwise abandon its scientific claims). On the other hand, it belongs to the realm of approximation as the statements it makes can claim only to be *plausible*; they can never claim to be “the truth.” One may assert that one feature of the necessary scientific rigor of anthropology is that it is inevitably approximative *while also maintaining* that the approximations produced derive their meaning from the absolute need to be rigorous “nevertheless.”

This inevitable approximation, a few properties of which will be examined below, makes the anthropological text all the more susceptible to interpretative biases and ideological excesses. But there is no reason to condone such biases and excesses or to abandon the *quest for rigor*, despite the challenges involved. Consequently, this work will also attempt to present various facets of this quest. To parody Gramsci’s saying, we must combine the pessimism of inevitable approximation with the optimism of the quest for scientific rigor.

All science, social sciences included, entails the quest for rigor. In empirically grounded social science research, rigor is situated at two levels. There is, on the one hand, the *rigor of argumentation* (we have



to be convincing), which includes the *rigor of logic* (contradictory statements are not allowed) and the *rigor of theory* (statements are made in the framework of scholarly debate). But there is, in addition, a need for *empirical rigor*. This concerns the relation between mastering the skills of interpretation and the empirical underpinning that links theories to their “reference reality.” The latter is the tiny “fragment” of social space and social time with which the researcher engages, and which he has set out to understand and explain. This necessary combination of logical rigor and empirical rigor exists in any social science based on fieldwork.<sup>2</sup>

Of course, this does not imply acceptance of the classic positivist conception of an essential reality external to individuals and towering above cognizant subjects. Clearly, phenomenology, in particular, has abundantly stressed that knowledge of the world is always mediated by the knowledge and position of a subject, and that, in the final analysis, access to reality hinges on consciousness and experience. But the project of understanding the world that characterizes the social sciences cannot be content with such an observation. While the world (or its “fragments”) is, strictly speaking, unknowable and ultimately fuzzy or uncertain and philosophically unapproachable as an external reality, social science is nevertheless grounded on the bet that “despite everything” the world can in fact be the object of a certain type of shared and communicable rational knowledge. In other words, all researchers assume *in practice* that there exists a “reference reality” operating beyond our consciousness and individual experiences, although this is impossible to prove *in theory*. Social science is therefore based on what is sometimes called “the realist hypothesis,”<sup>3</sup> according to which other people’s reality (or the part of it that the researcher studies, what I call the “reference reality”) must be held to exist per se, irrespective of the subjectivity of the person speaking about it. In this sense, it may become the object of shareable intelligibilities and is subject to scientific debates. These concern, inter alia, the *empirical adequacy* of statements, i.e., the *fit between the reference reality, taken as a research object, and the interpretations and theories the researcher proposes concerning this reality*.

The realist hypothesis, advancing the existence of a reference reality that is relatively and partially knowable through field inquiry, should not be confused with the realist illusion. The latter believes in a direct, objective access to the reference reality, forgetting that it is a social construction. The realist illusion is an offshoot of classic positivism.<sup>4</sup> Although this illusion held sway for a long time and still sometimes prevails, it seems clear that the constructivist posture, positing “the

social construction of reality”<sup>5</sup> by actors and researchers, has long since triumphed in social science and is by no means incompatible with the realist hypothesis. It is therefore possible to contend that social science constructivism is a *realist constructivism*. In other words, it accepts the objective of veracity, contrary to the ultraconstructivist and relativist postures of postmodernism (or to the “epistemological anarchism” associated with Paul Feyerabend<sup>6</sup>).<sup>7</sup> The quest for empirical adequacy is one response to this demand for veracity embedded in realist constructivism; it has nothing to do with positivism.

In fact, this adequacy is mediated by the data produced through fieldwork. The empirical rigor of the anthropologist, and of the social scientist in general, is linked to a double relation of adequacy: (a) the *relation of adequacy between argumentation and the data produced through fieldwork*; (b) the *relation of adequacy between the data produced through fieldwork and the “reference reality.”*

This double relation is at the center of the present work. So, what can we say about the empirical constraints of anthropological interpretation? I will attempt to identify various methods of pursuing scientific rigor in the framework of a mental, institutional, and discursive

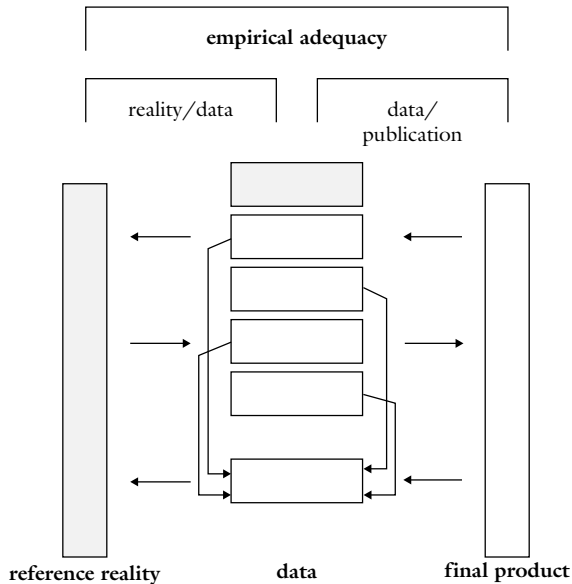


Figure 1.1 Empirical adequacy

space in which empirical fieldwork and interpretation continuously overlap, intermingle, and interact.

One of the problems facing anthropology in its quest for rigor regards its procedures of data production. Anthropology, *like any other social science*, operates within the Weberian register of *plausibility*—as opposed to the “Popperian” register of falsifiability.<sup>8</sup> But there is, moreover, the fact that the forms of empirical plausibility it employs are generally produced through “qualitative” inquiry. These require the immersion of the researcher in a “field” in which his or her interactions with the people being studied are decisive. The knowledge thus produced is simply plausible approximations, i.e., scholarly representations aimed at providing a rough, plausible account of the reference realities. These representations do not claim to be set in stone and rarely take on the burden of detailed statistics or precise percentages. This is so despite the fact that the ideal field inquiry, if such a thing exists, must combine qualitative and quantitative methods and can in no way present the two in opposition one to the other (the future belongs to mixed methods). Anthropological fieldwork or qualitative sociology involves a complex mixture of rough estimates, tendencies, descriptions, illustrations, significant cases, discourses and “local” representations, flexible hypotheses, cautious interpretations, local theories, and more or less confident generalizations. These elements are all subject to constant variations of scale and perspective. This relatively complex, mixed mosaic of commented upon and interpreted data clearly belongs to the realm of approximation. But this approximation does not (and should not) imply that anything goes.<sup>9</sup> This book will therefore make a point of analyzing some of the fundamental differences that, in anthropology, prevent approximation from being the equivalent of anything goes.

### ... in Anthropology

Yet anthropology is not really different from any other social science, and tracing a clear borderline between it and sociology is extremely tricky. Moreover, anthropology uses the same epistemology as other social sciences and is traversed by similar paradigms. It is subject to identical trends, is confronted with similar ideologies, and uses similar rhetorical and writing styles.

This does not prevent anthropological work from having its own unique trademark: a certain “style,” a certain “smell,”<sup>10</sup> although there is a persistent tendency to overestimate this tiny whiff of something special that “nevertheless” sets the anthropologist apart. This

slight distinction is enough to prevent us from saying that there is absolutely no difference between anthropology and the related disciplines of history and sociology.

This book will attempt to grasp the fundamental anchorage of anthropology as a social science *and* to highlight its discrete yet undeniable specificity. But it will definitely refuse the temptation of attributing this distinction to involvement with “exotic” research objects.

The present introduction focuses on the five topics that will be examined in the ensuing chapters: the relations between anthropology and the social sciences; the relations between anthropological epistemology and fieldwork; the relations between anthropological epistemology and ethics; the ethnographic pact; and the choice of a nonculturalist anthropology.

### ANTHROPOLOGY AND THE SOCIAL SCIENCES

Emphasizing the unity and epistemological specificity of the social sciences is nothing new, but Jean-Claude Passeron is indisputably the author to whom we owe the most contemporary, systematic, and rigorous expression of this unity and of this specificity.<sup>11</sup> His main focus is on anthropology, history, and sociology as disciplines at the “historical heart” of the social sciences.<sup>12</sup> Passeron, by demonstrating the existence of an epistemological space specific to these disciplines, grounded on the specific historicity of social phenomena and on the predominant use of natural language to describe and interpret them, opposed the positivist excesses that deem all science to be of an experimental or formal nature. He also objected to certain radical hermeneutic tendencies that hastily discard the empirical and comparative dimension of the social sciences.

Passeron’s central thesis may be divided into three interrelated, fundamental statements: (a) social sciences share a similar mode of scientificity; (b) this mode of scientificity specific to social sciences differs from that of the natural or physical sciences, i.e., it is not related to a “Popperian” epistemology of “falsification”; (c) it nevertheless pursues “scientific” objectives in that it attempts to produce an accurate knowledge of the world, a knowledge that is empirically grounded and subject to certain conditions of vigilance.

I consider that statements (b) and (c) regulate, *nolens volens*, the greater part of research strategies in anthropology as in neighboring disciplines. As Weber himself put it, “Empirical disciplines . . . deal with the qualitative aspect of reality—and history as well as all ‘cultural sciences’ of whatever kind belongs to this group.”<sup>13</sup> These statements

will be taken for granted and will serve as points of entry whose validity, as demonstrated by Passeron, need not be repeated. The following chapters offer extensive analyses of the various consequences arising from these theses, considered in the specific conditions of anthropological practice. Of course, these two statements are not unanimously accepted. In anthropology and elsewhere there are scholars who dispute the idea that the social sciences are specific and who would prefer to include them in the same epistemic order as the natural or experimental sciences.<sup>14</sup> Likewise, proponents of postmodernist-type epistemological anarchism<sup>15</sup> or radical hermeneutism do exist. These two extremes will, however, be considered set aside as marginal.

Statement (a), which raises the question of multidisciplinary within the social sciences, is more problematic. It is fairly obvious that strong disciplinary cleavages do exist. It is also clear that they cannot be wished away by an epistemological decree. Consequently, it may be useful to clear up a few misunderstandings.

Because the social sciences have a common scientific regime, their interpretative approaches, theoretical problems, heuristic postures, paradigms, and ways of constructing research objects are essentially shared, overlapping, or transversal. This is clearly visible in the way in which populism, Marxism, structuralism, or systemic analysis, for instance, has traversed history, sociology, and anthropology. The same is true when we consider how individualism (methodological or ideological) and holism (methodological or ideological) endow sociology and anthropology with structuring heuristic postures, or the degree to which linguistics, semiology, or interactionism has at times inspired other social sciences. The various social sciences also use similar techniques of narration or rhetoric of interpretation. Chapters 6 and 7 will study diverse effects of some of the “biases” (overinterpretation, populism) that threaten anthropology and other social sciences.

But this is no reason to disregard differences among the social sciences. Dissimilarities may be classified, for the sake of convenience, as methodological, institutional, and “cultural.” Despite epistemological commonalities, specific methodological, institutional, or cultural configurations point to the respective personality or autonomy of sociology, anthropology, or history and entertain relations of “selective affinity” with each discipline.

To begin with methodology, anthropology, history, and sociology do not necessarily produce their data in the same way and seem to favor relatively distinct forms of empirical inquiry. Archives for the historian, questionnaires for sociology, and “fieldwork” for anthropology are three distinct modes of data production, three specific

methodological configurations that tend to identify them. The specific mode of data production in anthropology will be covered in Chapter 2, devoted to the policy of fieldwork. Two fundamental characteristics of this mode of data production will then be analyzed (the emic in Chapter 3 and description in Chapter 4).

Institutional constraints, generally speaking, should not be underestimated. Each discipline has its courses, diplomas, commissions and scholarly associations, methods of recruitment and networks, trademarks, journals, and symposiums. Each has its own constraints, corporatist reflexes, and external reputation. Consequently, history has a multitude of secondary school teachers and a large audience outside the academic world. Anthropology continues to bear the burden of its exoticist legacy, which exerts an ambiguous fascination. And sociology is frequently seen as connected to consultancy or expertise in analyzing statistics or explaining “social” problems.

Then there are the “cultural” configurations. Each discipline tends to have its own scholarly culture, theoretical framework, and “library.” The “library” of the ordinary anthropologist, be it “real” (the books he owns or reads) or “mental” (knowledge retained from reading or the authors he likes to quote), is not identical to that of the average historian or sociologist, despite common ancestors. Of course, each library has books from neighboring disciplines: French social scientists have all read Duby, Lévi-Strauss, or Bourdieu, regardless of their professional identity. But such references, usually borrowed in keeping with intellectual trends, tend to caricature the neighboring discipline and often fail to tap into its innovative resources. To take the example of anthropology, in France, Lévi-Strauss is still frequently viewed as its icon by scholars from other disciplines, leading to an extremely skewed image of anthropology that disregards the profound changes that have occurred over the last 30 or 40 years.

In fact, anthropology is plagued with the “cultural” liabilities of its past (and these are unfortunately still obvious in certain contemporary works). These inherited cultural burdens often project an extremely reductive and highly inaccurate image of anthropology. Old school ethnology, with its traditionalist, colonial, culturalist, and patrimonialist biases, has had its day. It still has a few nostalgic followers, but it no longer reflects an accurate image of today’s anthropology and has nothing to do with the type of anthropology this book is about (we will return to this later).<sup>16</sup> Misinformed or misleading fascination with “primitive societies,” the idealization of societies that allegedly refuse the state or goods, exoticist exaggerations and indulgence, and the systematic referencing of ancestral “culture” still exist (see Chapters 6,

7, and 8). Such biases appear not only in the work of certain sociologists or historians who claim to draw on anthropology but also in that of anthropologists themselves.

The opposite tendency—diving into modernistic essayism—is one reaction to this backward-looking ethnology whose image still dogs the discipline. Hence, some scholars, discovering the myriad facets of “globalization” or “modernity,” hasten to publish their brilliant insights on a brave new world. Whereas old-hat ethnography was meticulously backward looking, postmodern or ultramodern anthropology is more often than not superficially futuristic and globalized.<sup>17</sup> So, is it possible to do anthropology in a way that is both rigorous and up-to-date? This work will provide throughout an affirmative answer to this question.

It is quite easy to identify the evils of nostalgic traditionalism or anecdotic modernism in other people’s work, yet we have all been tempted by such offenses and have sometimes yielded to temptation. Those who most readily succumb to such ills are scholars who have loosened their ties with empirical research. There is no rigorous anthropology without empirical fieldwork. This is true at every stage, not only at the beginning of one’s career. Fieldwork is necessary at every stage; it is not merely a professional ritual of initiation, nor is it simply a prerequisite for preparing a dissertation. Can we imagine a historian, no matter how famous, who no longer needs to go to the archives?

### EPISTEMOLOGY, THEORY, AND FIELDWORK

Let me dispel a possible misunderstanding: I am not saying that every anthropologist should serve a term of hard labor “in the field.” Nor am I stigmatizing scholars who have drifted away from fieldwork over the years. This is simply a reminder that fieldwork is the cornerstone of anthropological legitimacy. It is in the name of fieldwork that anthropology claims the right to an audience, and in its name that such an audience is acquired. Numerous publications that are “distant from fieldwork” obviously gravitate around the empirical core of the discipline, and many of these are useful or even indispensable. Works derived from secondhand materials large-scale comparative analyses, scholarly syntheses, and research related to the history or the epistemology of the discipline *are* important. But this should not obscure the fact that empirical validity is the ultimate requirement on which anthropological legitimacy is founded.

Seen in this perspective, anthropology as an empirical discipline is quite different from Kant’s philosophical anthropology and from the

definition of Lévi-Strauss, whose three-tiered diagram attributes to ethnography and ethnology the task of producing and interpreting empirical materials and to anthropology that of constructing a general theory of human societies.<sup>18</sup>

The epistemology proposed in this work therefore claims to be first and foremost an *epistemology of fieldwork*, one that is centered on the relations between fieldwork data and the scholarly interpretations they generate. Following Glaser and Strauss's "grounded theory"<sup>19</sup> we may speak in terms of a *grounded epistemology*, i.e., an epistemology "grounded in fieldwork." Seen in this light, the border between epistemology and methodology is thin, porous, and relatively easy to cross. Consequently, my focus on the empirical adequacy of anthropological interpretations places this work in a space in which both registers overlap.

The purpose of this work is *not* to present outcomes of empirical fieldwork. But it is nonetheless the result of more than 40 years of practicing fieldwork, teaching about fieldwork, and supervising doctoral candidates faced with their first field inquiry. Despite its abstract approach, which may appear "far from data," this work constantly draws on this long-standing, invisible experience.

Fieldwork is central to the production and, to a great extent, to the interpretation of data in anthropology. The field report provides a large share of anthropological knowledge and intelligibility. In anthropology, fieldwork is the specific embodiment of the exigency of empirical rigor on which the social sciences are grounded.

Yet this work is not about promoting or defending a fieldwork mystique. Fieldwork is just *one* form of social science data production. It has its advantages and its disadvantages, as any method of inquiry. They are simply different from those encountered when going through dusty archives or managing a huge team of inquirers and keyboard operators. The adventures or misfortunes of the field anthropologist portrayed as a hero are of no interest here. Our point of departure is that there is almost no epistemological difference between going down a highway in Florida and rowing up the Congo, between living in a low-cost flat on the outskirts of Paris or in a Fulani camp in the bush. Well, yes, there is a tiny difference, but it has nothing to do with the fortunes and misfortunes of the anthropologist. It resides, instead, in the level of exoticism to which he is likely to succumb or which he might be tempted to exploit. Exoticism (like narcissism) might be a wonderful resource in literature but is ill-advised in anthropology (see Chapter 8), where our job, like that of the sociologist or historian, is to make the subjects of our investigation familiar and intelligible irrespective of their greater or lesser cultural proximity. In anthropology,



sociology, or history, the hero is the person about whom we are speaking, *not* the scholar who is speaking. From an epistemological point of view, the scholar is of no interest to us unless what he has to say about his personal position aids our understanding of what he has to say about others (see Chapter 5).

Fieldwork is the ultimate authoritative tool when it comes to talking about others and making them talk; it is on this fact that the lifeworld of anthropologists is grounded. Other lifeworlds such as poetry, painting, music, psychoanalysis, love, and football have very different rules of the game and—fortunately—are not submitted to constraints akin to the empirical demands of anthropological work and discourse. But, in the field of anthropology, it is mandatory to report other people's actions and words as "truthfully" as possible and to offer proofs of credibility. The "emic" (in other words, the attention paid to actors' point of view; see Chapter 3) and the "descriptive" (in other words, the use of observation; see Chapter 4) are fundamental properties of anthropological work. They bear witness, through deliberate methods of qualitative inquiry, to the fact that our interpretations are based on a securely empirical foundation.<sup>20</sup>

Fieldwork, with its resources and constraints, is therefore the main definer of anthropological specificity. But these constraints and resources set out to stimulate the anthropological imagination,<sup>21</sup> not to bridle it. Opposing theory and fieldwork one to the other makes no sense and generally boils down to a *de facto*, systematic (and unwarranted) valorization of theory (seen as conceptual virtuosity) at the expense of fieldwork (viewed as crude empiricism). Yet eminent theoreticians are often condescending with regard to the drudgery of descriptive investigation. So, is it possible to suggest a more satisfactory opposition between "theories based on theories" and "theories based on fieldwork"? Not really, insofar as such a formulation implies that theory based on fieldwork disregards other theories, which is simply outrageous. *Theoretical interpretations grounded on fieldwork simply labor under greater constraints than others, but they also take into account other theories.* They are equally "theoretical," but in different ways. Far from interpretations constructed simply on the basis of scholarly reading and thinking, anthropological theories are also (and often primarily) constructed on the basis of fieldwork (though this is unfortunately not always the case in modern globalized anthropology). In other words, grounded theories are more demanding, which often makes them less popular in academic circles. If there is opposition, it is, *de facto*, between "theories based on theories" and "theories based on fieldwork and theories." The scientific and academic

markets tend to privilege the first, which is less demanding in terms of intellectual and personal investments. But there is no reason for armchair anthropology to disqualify fieldwork anthropology on the charge of empiricism. Theories grounded in fieldwork are often (but not always) less general and abstract than theories unconstrained by empirical rigor. But this does not make them any less “theoretical” or interesting. Excessive generalization and abstraction (a characteristic of the structuralist French anthropology of the 1960s and deconstructionist American anthropology of the 1980s) is not in itself a sign of theoretical excellence.<sup>22</sup> Likewise, interpretative relevance is not enhanced by pretentiousness and disregard for space-time constraints or counterexamples. The world of research is cluttered with hasty generalizations<sup>23</sup> skydiving to universal meanings without the safety net of the contextualized case study (see Chapter 7). When anthropological virtuosity loses sight of empirical constraints, yielding to the temptation of gorging itself on philosophy, it produces theories that are liable to gain in glamor what they inevitably lose in terms of rigor.

The term *grounded theory* has the advantage of connecting rather than opposing fieldwork and theory. It highlights the production of theory based on data from the field.<sup>24</sup> Such a practice might appear to be inductive, but this should not lead us to think that anthropology never operates on the level of deduction. In fact, all social sciences combine the two procedures, but in variable proportions.<sup>25</sup>

At all events, the aim of anthropology grounded in fieldwork is not to renounce skillful interpretation, gratifying intellectual exploration, and scholarly debate. But it subjects them to situations of increased vigilance to make the challenge more stimulating and interesting.<sup>26</sup> Fieldwork, in this regard, is also an intellectual playing field, on which we must refuse to cheat and where we must take risks (discerningly).

This playing field is not fenced off, and the anthropologist does not remain there forever. Monographic exercises or focused inquiries, while necessary, are not enough. Hence, anthropology, like history and sociology, and maybe even more than these disciplines, practices reasoned comparison. Comparative anthropology may remain empirical and grounded, as within the framework of multisite research, or venture out into other scholarly times and spaces. There is no social science without comparison (see Chapter 8): the problem is determining what kinds of comparison to make. It is true that such “departures from the field,” however crucial, are prone to excesses, in anthropology as elsewhere. There are too many wild or uncontrolled comparisons going around. Clearly, it is not always easy to do comparative analysis rigorously. But this is all the more reason to advocate rigor rather than abandon it.

### A MORAL EPISTEMOLOGY?

Some readers might perceive moral connotations in the preceding remarks. Indeed, the term “rigor” in itself, while associated with the “imagination,” also resonates vaguely with puritanism. Instead of denying this ethical dimension of epistemology, I would like to take on a few of its characteristics while indicating certain limits.

To cite an extreme situation, it is obviously just as possible to cheat in anthropology as in any other social science. As in other types of fraud, this is liable to occur unwittingly and in good faith. Yet it does not follow that fraud should be accepted, justified, or tolerated. There are innumerable forms of cheating, or, to use a less excessive, less moral, and less aggressive terminology, there are numerous forms of “lack of vigilance.” Lack of vigilance in data production goes from the simple refusal to take stock of counterexamples or obstacles that challenge completed interpretations to the discrete, convenient tweaking of descriptions or translations. Lack of vigilance also affects data interpretation through the multiple modes of “overinterpretation” (see Chapter 7), which sometimes include the wild comparisons and hasty generalizations already mentioned.

The problem is twofold. On the one hand, it is difficult to detect inattention (or even out-and-out fraud). On the other, this difficulty is due, *inter alia*, to the absence of precise, clear, recognized standards of anthropological rigor (there is no written “code” of good anthropological conduct).

First, it is almost impossible to verify the field data referenced by the anthropologist (it is a lot easier to check the historian’s sources, to refute the questions posed by a survey institute, or to debate its sampling procedures). We are more or less obliged to resort to the unhealthy expedient of taking the anthropologist at his word.

This raises a second difficulty: there are no hard-and-fast rules on which to call. The fact is that anthropology has no formalized methodology; there is no rule against which to judge conformity or non-conformity. Fieldwork results are approximative. This is not such a bad thing after all and in itself poses no insurmountable problems. But the methods used to produce results are also reasonably approximative, thus making it a lot more difficult to evaluate rigor and reliability. As a result, the anthropologist’s work is inextricably linked to his personal skills, *i.e.*, to the know-how acquired through practice (see Chapter 2). It therefore follows that anthropology is the social science that relies the most on personal “sleight of hand.”

However, this does not imply a total repudiation of control, methodological evaluation, and epistemological vigilance. If this were

the case, anthropology would be no more than a literary exercise. A position of this type is not unprecedented, and the anthropologist, in moments of depression, is sometimes tempted to give up. But to reduce anthropology to pure subjectivity is to disregard its decades-old quest for greater veracity. Thanks to debates and polemics, critiques and states of the art, reports and calls for papers, conferences and scholarly associations, PhD juries and research supervision, anthropology has gradually tooled modes of functioning based on “tacit agreements” about fieldwork (rather than definitive “rules of the game”). These agreements are “soft,” latent, fuzzy, interlaced with dissensions and criticisms. Though approximative, they are nonetheless real. To return to the craft metaphor, there is usually no handbook defining the “rules” of skillful craftsmanship, but this does not prevent the existence of “quasi standards” and practical norms. These are rules of thumb, which often—but not always—make allowances for flexible and variable “quasi consensuses” concerning the excellence or acceptability of the empirical value of a given paper. Likewise, over the course of countless analyses of ethnographic procedures, of copious definitions of fieldwork, of myriad reflections on data production, anthropology has laid the groundwork of its methodology. Obviously, this methodology is neither stabilized nor codified and remains open to debate. This book will present a few key elements of a substantial literature attesting to extensive debate.

Furthermore, and this brings us to the concept of “moral epistemology,” ethical barriers function “nevertheless.” We cannot afford to overlook this dimension. While it is true that the anthropologist, more than any other researcher, has the possibility of simply inventing his data (no one is likely to go out into the field to check if what he claims informants have said is really the case<sup>27</sup>), we still have every reason to believe that “this does not happen” in the overwhelming majority of cases. Obviously, every researcher has personal biases (in the same way that every field inquiry has its own), and we should never take what anyone says for granted. But researchers rarely introduce biases systematically and deliberately. In other words, few are likely to make a practice of cheating. Consequently, analyzing these biases, providing ways of managing them as rigorously as possible (i.e., never completely), and pinpointing every conceivable means of supporting methodological vigilance “in the field” may enhance the professional ethics of anthropology.

Regardless of our opinion concerning the effectiveness of this advocacy of scientific ethics, we are forced to admit that plausibility in anthropology reposes significantly on a peculiar mix comprising the critical glance—indispensable in anthropological work—and

confidence in the anthropologist, based on an unspoken moral contract binding him to his peers and readers. The “ethnographic pact” is also an ethical pact.

### THE ETHNOGRAPHIC PACT

In their writing, anthropologists all make constant use of “effects of reality” to legitimate their discourse. This is the primary principle of the ethnographic enterprise: other people exist, I have met them, and I am asking you to believe what I say. *Having been there* is at the heart of the matter.

Of course, an anthropological statement, of whatever type, is neither the reference reality nor its reflection; it is simultaneously a “production,” a “performance,” and a “hypothesis.” However, “other people’s reality” exists independently of the anthropologist. His reference to this “outside reality” underpins anthropological writing. In other words, while the anthropological text can never really “reflect” reality and therefore cannot “speak on its behalf,” it nevertheless “speaks about reality” and attempts to describe and understand it in the least inaccurate way possible. The “effects of reality” make up the groundwork of anthropological rhetoric. Saying “I assure you that over there this is how it happens” is our bread and butter. In a way, as anthropologists, we sign with our reader what one may call an “ethnographic pact.” This is a gauge of our seriousness and deontology: what I am describing really happened, the statements I am quoting were really made, the reality I am narrating is true; it is not a figment of my imagination. This ethnographic pact arises from the fact that ethnographic data are principally produced via the interactions of the researcher with the subjects of his inquiry: his personal testimony is the only guarantee of veracity.<sup>28</sup>

This concept of the “ethnographic pact” draws direct inspiration from what Lejeune calls the “autobiographical pact.”<sup>29</sup> What Lejeune points out in essence is that nothing—no narrative technique, grammatical or stylistic device, editorial or typographical device, in short, no objective indicator—allows us to differentiate between the autobiography and the novel. And yet, they are not the same. It is because the author of an autobiography promises the reader that his work is in fact an autobiography (this is the autobiographical pact) that we read it as an autobiography and not as a novel, trusting the author as far as he does not confuse us by using techniques that are liable to disrupt this tacit agreement.

It is easier to understand what the ethnographic pact entails by considering a particular brand of ethnographic product, namely the

ethnographic film, seen as a documentary subtype. We all know that the images are constructed, that each frame is a subjective division of reality selected from among many other possibilities, that editing is invariably a deliberate “manipulation” of the meaning of images. But this does not prevent the filmmaker from stating, implicitly and necessarily, that these artifacts and artifices, at the end of the day, remain a “true” picture of reality (in the minimal sense of not false, not invented, of this is really how things happen). Hence the scandal sometimes caused by the revelation of “doctored” images in a documentary film:<sup>30</sup> the pact, in such cases, has been disrespected.

We may consider two core operations, *description* (of scenes observed by the anthropologist; see Chapter 4) and *transcription* (of the words of “informants,” recorded and even translated by the anthropologist; see Chapter 3), through which the ethnographic pact functions in anthropological writing. These two operations are closest to the empirical pole of anthropology and always claim to be “realistic.” Although they propose data “produced” partly on the basis of the anthropologist’s subjectivity, they nevertheless set out to convince the reader that the practices presented really occurred or that the statements reported are authentic notwithstanding the constraints of artificiality and subjectivity, and despite the preconceptions at work in social science inquiry and writing. To renounce this quest for veracity, to relinquish this objective of realism, would amount to abandoning all hope of “faithfulness” to the reference reality, and the ethnographic pact would thus be broken.

Obviously, ethnographic descriptions inevitably include interpretative postures and entail the unavoidable risks of interpretative bias. Whereas descriptions all operate in this manner, some do so more than others, and some openly acknowledge it. Geertz’s<sup>31</sup> “thick description” is a good illustration of ethnographic descriptions that are deliberately saturated with meanings constructed by the researcher (see Chapter 4). Geertz nevertheless proposes a tacit “ethnographic pact.” For instance, in a well-known and abundantly commented text<sup>32</sup> the reader is implicitly requested to believe that the cockfights Geertz depicts are “true” and that the gestures he describes are “real” regardless of the influence of Balinese culture on his interpretations. The problem of this method has more to do with the influence of the empirical level on the interpretative one than the inverse (which has, however, received greater attention from commentators). The “truth” of the fundamentally “descriptive” elements of his “quick description” tends to be surreptitiously reinjected into the fundamentally “interpretative” elements with which this “description” is spiked.

This leads the insufficiently keen or underinformed reader to confuse what is “possible” (Geertz’s highly culturalist interpretations) with what “really exists” (the cockfights whose reality is attested thanks to the ethnographic pact). But the opposite never occurs: more or less “veiled” elements of interpretation never question the veracity of elements of description. The attentive or informed critic will never accuse Geertz of having invented his cockfight scenes or police raids, despite the scantiness of details provided. The ethnographic pact is not broken.<sup>33</sup>

Accusations challenging the “truthfulness” of the “data” collected by an anthropologist are definitely graver than debates over “interpretations” or “theorizations,” since the first is a breach of the ethnographic pact. Van Beek’s study on the work of Griaule,<sup>34</sup> based on the inquiries he later conducted on the same field sites in Mali, clearly raises the issue of the “nonexistence” of what Griaule presented as “real.” This analysis is infinitely more devastating than the numerous criticisms addressing Griaule’s ideological and theoretical assumptions, glaring though they may appear.

The same applies to translation—strictly speaking—between distinct languages, i.e., the operation through which the statements, narratives, and discourses of actors, collected by the researcher in the language of these actors, are reframed in the language of his readers. Such a translation poses the same problem as description: it is a complex and unstable compromise between a mandatory empirical objective and inescapable interpretative projections (or, if we prefer, between the emic and the etic; see Chapter 3). Even in the event of “similar” languages, belonging to neighboring or similar cultures, “translation” is never entirely “faithful” or “true” owing to the fact that the semantic fields in question are never exactly the same. Yet the purpose of translation is to serve actors’ discourses and to be as faithful to them as possible. In other words, the work of the translator in anthropology is also based on the “ethnographic pact”: the translator “by definition” commits himself to relaying to the reader, to the best of his ability, what “others really said.”

But translation may also be viewed in a broader, quasi-metaphorical sense, one which considers anthropology to be basically the translation of one culture (that of the social groups being studied) into another (that of the academic community, or even of the neighboring intelligentsia).<sup>35</sup> Such a translation may be more or less skillful, explicit, brilliant, or resourceful. But its legitimacy is still based on the objective of getting as close as possible to the “reality of others,” an objective that is never fully attained. However, the problem with the

metaphor of translating one culture into another has less to do with what we understand by “translation” than with what we understand by “culture.”

### A NONCULTURALIST ANTHROPOLOGY

It may be clear by now that what is being defended here is a non-culturalist conception of anthropology, inasmuch as culturalism is a scientific ideology that is continuously at work in the social sciences, an ideology whose presence calls for constant vigilance. *Culturalism as a scientific ideology* is directly and typically associated with anthropology. From the beginning of the twentieth century onward, the concept of culture was decisive in the establishment of ethnology and remains, for many authors, central to its definition.<sup>36</sup> It has played an indisputably positive role in the history of the social sciences, beginning with Boas, by refuting dominant evolutionary theories, in the name of the equality of cultures, and by rehabilitating the cultures of dominated peoples.<sup>37</sup> But culturalism, as a homogenizing and essentializing approach, has since become one of the main obstacles in the way of the quest for anthropological rigor (see Chapters 6 and 7). This is not to say that the concept of “culture” should be thrown away with the bathwater of culturalism. The use of this concept is obviously not to be proscribed. Used in a limited and cautious manner, to describe a set of significantly shared representations and/or behaviors by a specific unit of social actors in a given context, this concept remains indispensable and has its place on the agenda of any social science research. But caution obliges us to use only restricted and empirically validated acceptations of this term (such as local cultures, professional cultures, organizational culture, specific subcultures) and to strictly avoid overgeneralizations (such as national culture, ethnic culture, cultural identity).

In fact, to move away from this pragmatic and inevitable use of “culture” is to drift rapidly in the direction of an ideological use, which loads the term with misunderstandings, stereotypes, hasty interpretations, and overinterpretations and moreover projects a series of preconceptions onto the object of study.<sup>38</sup> In the culturalist perspective, the important (relevant) representations and behaviors of a social group (an “ethnic group” in classic ethnology) are necessarily held in common. Yet, finding out which representations and behaviors are shared remains an empirical research problem. For culturalism, shared representations and behaviors do not differ according to context. Yet, finding out which representations and behaviors are



shared in a given context and which ones are not remains an empirical research problem. For culturalism, shared representations and behaviors stem from common “values” (not to mention “worldviews”). But these are merely speculative assumptions, which no empirical research can currently guarantee or validate, given the uncertain nature of the conceptual realm of “values,” which is ambivalent and saturated with ideology.<sup>39</sup>

Culturalism, with its prejudices about what is shared and its essentialist or catchall explanation of this alleged commonality, pollutes the work of data production and interpretation. It is the manifestation of *ideological holism* (seen as an ideology of totality; see Chapter 6) in anthropology, whereas a reasoned and careful use of the concept of culture is more in line with *methodological holism* (viewed as a concern with transversality). To forget that culture is a convenient fiction is to transform it into an overarching essence, into a naturalized category that goes without saying,<sup>40</sup> one that towers above social actors and forcefully determines their representations and behaviors. Calling on culture is a cheap expedient for lending explanatory credence to a convenient fiction, thereby sparing the pains of empirical demonstration. Interestingly, deconstructionist and postmodern currents, while virulently refuting some founding principles and other dimensions of anthropology, especially its scientific claim, have rarely attacked the basis of culturalist ideology. On the contrary, they have often inspired cultural studies or cultural anthropology.<sup>41</sup> In fact, not only have they reproduced culturalist ideology in its “naturalized” form, they have moreover expanded it by applying the term to many collective referents stretched to include social class, gender, sexual identities, “modern tribes,” and multiple marginalities. As soon as a social entity seems ripe for cultural analysis, in other words, as soon as it is deemed to have a “culture,” it becomes an epistemic community producing its own “text,” its own “speech,” its own worldview, and its own knowledge.<sup>42</sup> Admittedly, in this adventure the concept of culture has evolved. It has been voided of most of its traditionalist-patrimonialist meanings, and hybridization is now a key term, but culture remains, more than ever, a discrete, homogenized, and all-powerful totality: culturalism as an ideology is still alive and kicking.

But despite these recent developments, culturalism, which has spread in the meantime to all of the other social sciences, continues to court the old demons of ethnology, such as traditionalism or ethnum, especially where Africa<sup>43</sup> is concerned. We will not belabor this point. The problem with these embarrassing ideological legacies is that some sociologists and anthropologists still fall for them, while others spend

a considerable amount of time showing that they have nothing to do with them or trying to break away from them by tackling anthropology itself, portrayed as the eternal culprit responsible for these evils.

My position on this issue is clear. Undeniably, anthropology has fostered scientific ideologies such as traditionalism, ethnism, and culturalism, which currently appear outdated or unacceptable. But it is absolutely possible to separate it from ideologies of this type. In spite of (and sometimes through) these ideologies, it has, moreover, produced knowledge and irreplaceable methodologies, in particular. Hence, it is hardly a question here of buying wholesale into the extremely debatable legacy of anthropology. Nor is this, to the contrary, about championing an epistemological revolution or calling for a radical overhauling of the discipline itself. All social sciences stand in need of a balanced, clearheaded, sorting-through of the legacy of the past, a process that needs to be conducted without unwarranted reverence for founding fathers or systematic belittling of real accomplishments.

But this sorting-through will be tackled herein from an exclusively methodological or epistemological point of view, i.e., as regards the problems arising from the production and interpretation of empirical data in qualitative research (in anthropology or other disciplines where fieldwork is practiced). It will not hazard a substantive overall evaluation of the discipline itself. Hence, this book does not propose a conceptual, theoretical, or paradigmatic assessment of anthropology; this would be almost impossible besides given the increasing diversification of its research topics and problematics.

Furthermore, the majority of these new topics and problematics are increasingly shared, or exploited in common, with sociology or history or political science. I am personally in favor of this trend. Anthropological rigor should not be equated with narrow corporatism; to the contrary, it implies an ever-increasing collaboration with the other social sciences. Hence, serious anthropology is always framed diachronically. The generally founded (but sometimes excessive) critique identifying an immoderate and illegitimate use of the "ethnographic present"<sup>44</sup> by classic ethnology blithely disregards the fact that the historiographical posture is part and parcel of the anthropological tradition.

In French, I frequently use the term "socioanthropology" as an equivalent of "anthropology" in response to various concerns. First, it is a manner of stressing the fact that sociology and anthropology largely partake of common objects, postures, and problems. Then, there is the concern of releasing anthropology from its exoticist

excesses. Lastly, and above all, I use this term to highlight a convergent, double *methodological* heritage: that of ethnology (beginning with Boas and Malinowski), currently called anthropology,<sup>45</sup> and that of the Chicago School (starting with Park), which developed, in the midst of modern American society,<sup>46</sup> a fieldwork sociology verging on ethnographic methods, which is sometimes called “qualitative sociology.” In a way, the Manchester School was the forerunner in the successful unification of these two traditions in the 1950s and 1960s. This was accomplished through the well-known studies it conducted in southern Africa (on cities, migrations, networks, conflicts, the colonial context) and through a series of long-term inquiries in English factories.<sup>47</sup> We owe much to these studies, which were highly innovative when compared to the themes of classic Africanist ethnology.