

An Invitation to Reflexive Sociology

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The University of Chicago Press

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The University of Chicago Press, Chicago 60637
The University of Chicago Press, Ltd., London
© 1992 by The University of Chicago
All rights reserved. Published 1992
Printed in the United States of America
01 00 99 98 97 96 95 94 93 92 5 4 3 2 1

ISBN (cloth): 0-226-06740-8

ISBN (paper): 0-226-06741-6

Library of Congress Cataloging-in-Publication Data

Bourdieu, Pierre.

An invitation to reflexive sociology / Pierre Bourdieu
and Loïc J. D. Wacquant.

p. cm.

Includes bibliographical references and index.

ISBN 0-226-06740-8 (cloth). — ISBN 0-226-06741-6

(pbk.)

1. Sociology. 2. Bourdieu, Pierre. I. Wacquant,
Loïc J. D. II. Title.

HM24.B669 1992

301—dc20

91-43725

CIP

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The Practice of Reflexive Sociology (The Paris Workshop)

Pierre Bourdieu

I am more than half-inclined to liken Descartes' rules to this precept of I-don't-remember-what chemist: take what you must and proceed as you must, you will then get what you wish to get. Do not admit of anything that is not truly obvious (that is, admit only that which you have to admit); divide the topic into the required parts (that is, do what you have to do); proceed according to order (in the order according to which you have to proceed); provide complete enumerations (that is, the ones you have to provide): here is precisely the manner of people who say that you must seek the good and shun the bad. All of this is surely appropriate, except that you lack the criteria of good and bad.

Leibniz, *Philosophische Schriften*

1 Handing Down a Trade

Today, to make an exception, I would like to try and explicate a little the pedagogical purposes that I pursue in this seminar. Next time I will ask each of the participants briefly to introduce themselves and to present the topic of their research in a few sentences—this, I insist, in a very casual manner, without any special preparation. What I expect is not a formal presentation, that is, a defensive discourse closed unto itself whose first aim (as is readily understandable) is to exorcize your fear of criticism, but rather a simple, unpretentious, and candid exposition of the work done, of the difficulties encountered, of the problems uncovered, etc. Nothing is more universal and universalizable than difficulties. Each of us will find considerable comfort in discovering that a good number of the difficulties that we attribute to our own idiosyncratic awkwardness or incompetence are universally shared; and all will benefit more fully from the apparently highly particularized advice that I may give.

I would like to say in passing that, among all the dispositions that I would wish to be capable of inculcating, there is the ability to *apprehend research as a rational endeavor* rather than as a kind of mystical quest, talked about with bombast for the sake of self-reassurance but also with the effect of increasing one's fear or anguish. Such a realistic stance (which does not mean that it is cynical) aims at maximizing the yield of your investments and is geared toward an optimal allocation of your resources, beginning with the time you have at your disposal. I know that this manner of experiencing scientific work is somewhat

disenchanted and disenchanting, and that I run the risk of damaging the image of themselves that many researchers like to keep. But it is perhaps the best and the only way of sheltering oneself from the much more serious disappointments that await the scholar who falls from on high after many a years of self-mystification during which he spent more energy trying to conform to the glorified image that he has of research, that is of himself as a researcher, than in simply doing his job.

A research presentation is in every respect the very opposite of an exhibition, of a *show*¹ in which you seek to show off and to impress others. It is a discourse in which you *expose yourself*, you take risks. (In order to be sure to defuse your defense mechanisms and to neutralize the strategies of presentation of self that you will likely use, I will not hesitate to give you the floor by surprise and to ask you to speak without forewarning and preparation.) The more you expose yourself, the greater your chances of benefiting from the discussion and the more constructive and good-willed, I am sure, the criticisms and advice you will receive. The most efficient way of wiping out errors, as well as the terrors that are oftentimes at their root, is to be able to laugh about them together, which, as you will soon discover, will happen quite often . . .

I will on occasion—I may do it next time—present the research work that I am presently conducting. You will then see in a state that one may call “becoming,” that is muddled, cloudy, works that you usually see only in their *finished* state. Homo academicus relishes the finished. Like the *pompier* (academic) painters, he or she likes to make the strokes of the brush, the touching and retouching disappear from his works. I have at times felt a great anguish after I discovered that painters such as Couture, who was Manet's master, had left behind magnificent sketches, very close to impressionist painting—which constructed itself against *pompier* painting—and that they had often “spoiled,” in a sense, these works by putting the finishing touches stipulated by the ethic of work well done and well polished whose expression can be found in the Academic aesthetic.² I will try to present this research work in progress in its fermenting confusion—

1. In English in the original.

2. See Bourdieu 1987i for a historical analysis of the symbolic revolution entailed in the emergence of impressionist painting in nineteenth-century France.

within limits, of course, for I am well aware that, for obvious social reasons, I am less entitled to confusion than you are, and that you will be less inclined to grant me that right than I would you, and in a sense rightly so (yet again, only in reference to an implicit pedagogical ideal which certainly deserves to be questioned, that which leads us for instance to assess the value of a course, its pedagogic yield, to the quantity and the clarity of the notes that one takes in it).

One of the functions of a seminar such as this one is to give you an opportunity to see *how research work is actually carried out*. You will not get a complete recording of all the mishaps and misfirings, of all the repetitions that proved necessary to produce the final transcript which annuls them. But the high-speed picture that will be shown to you should allow you to acquire an idea of what goes on in the privacy of the "laboratory" or, to speak more modestly, the workshop—in the sense of the workshop of the artisan or of the Quattrocento painter—i.e., it will include all the false starts, the wavering, the impasses, the renunciations, and so on. Researchers whose work is at various stages of advancement will present the objects they have tried to construct and will submit themselves to the questioning of all the others who, in the manner of old *compagnons*, fellow-workers of the trade, as they say in the traditional language of the *métiers*,³ will contribute the collective experience they have accumulated over all the trials and errors of the past.

The *summum* of the art, in the social sciences, is, in my eyes, to be capable of engaging very high "theoretical" stakes by means of very precise and often apparently very mundane, if not derisory, empirical objects. Social scientists tend too easily to assume that the sociopolitical importance of an object is in itself sufficient warrant for the importance of the discourse that addresses it. This is perhaps what explains why those sociologists who are most prone to equate their standing with that of their object (as do some of those who, today, concern themselves with the state or with power) often pay the least attention to method. What counts, in reality, is the rigor of the *con-*

3. William H. Sewell (1980: 19–39) offers a detailed historical exegesis of the notion of *métier* under the Old Regime. His capsule characterization of the corporate idiom in eighteenth-century France is worth quoting since it captures two key dimensions of the *métier* of the sociologist as Bourdieu conceives it: "Gens de *métier* could be defined as the intersection of the domain of manual effort or labor with the domain of art or intelligence."

struction of the object. The power of a mode of thinking never manifests itself more clearly than in its capacity to constitute socially insignificant objects into scientific objects (as Goffman did of the minutiae of face-to-face interaction),⁴ or, what amounts to the same thing, to approach a major socially significant object from an unexpected angle—something I am presently attempting by studying the effects of the monopoly of the state over the means of legitimate symbolic violence by way of a very down-to-earth analysis of what a certificate (of illness, invalidity, schooling, etc.) is and does. In this sense, the sociologist of today is, *mutatis mutandis*, in a position quite similar to that of Manet or Flaubert who, in order to realize fully the mode of construction of reality they were inventing, had to apply it to objects traditionally excluded from the realm of academic art, exclusively concerned with persons and things socially designated as important, which explains why they were accused of "realism." The sociologist could well make his or hers Flaubert's motto: "To write well about the mediocre."

We must learn how to translate highly abstract problems into thoroughly practical scientific operations, which presupposes, as we will see, a very peculiar relation to what is ordinarily called "theory" and "research" (*empirie*). In such an enterprise, abstract precepts such as the ones enunciated in *Le métier de sociologue* (Bourdieu, Chamboredon, and Passeron 1973; English translation 1991), if they have the virtue of arousing attention and putting us on notice, are not of much help. No doubt because there is no manner of mastering the fundamental principles of a practice—the practice of scientific research is no exception here—than by practicing it alongside a kind of guide or coach who provides assurance and reassurance, who sets an example and who corrects you by putting forth, *in situation*, precepts applied directly to the *particular case* at hand.

Of course, it may very well happen that, after listening to a two-hour discussion on the teaching of music, the logic of combat sports, the emergence of subsidized housing markets or Greek theology, you will wonder whether you have not wasted your time and if you have learned anything at all. You will not come out of this seminar with neat summaries on communicative action, on systems theory or even

4. See the epitaph written by Bourdieu (1983e) for *Le Monde* upon Goffman's sudden death. See also Boltanski 1974.

on the notions of field and habitus. Instead of giving a formal exposition of the notion of structure in modern mathematics and physics and on the conditions of applicability of the structural mode of thinking to sociology, as I used to do twenty years ago⁵ (this was undoubtedly more "impressive"), I will say much the same thing but in a practical form, that is, by means of very trivial remarks and elemental questions—so elemental indeed that we too often forget entirely to raise them—and by immersing myself, each time, into the detail of each particular study. One can really supervise a research, since this is what is involved here, only on condition of actually *doing it* along with the researcher who is in charge of it: this implies that you work on questionnaire construction, on reading statistical tables or interpreting documents, that you suggest hypotheses if necessary, and so on. It is clear that, under such conditions, one can supervise only a very small number of research projects and that those who pretend to supervise a large number of them do not really do what they claim they are doing.

Given that what is to be communicated consists essentially of a *modus operandi*, a mode of scientific production which presupposes a definite mode of perception, a set of principles of vision and division, there is no way to acquire it other than to make people see it in practical operation or to observe how this *scientific habitus* (we might as well call it by its name) "reacts" in the face of practical choices—a type of sampling, a questionnaire, a coding dilemma, etc.—without necessarily explicating them in the form of formal precepts.

The teaching of a *métier*, a craft, a trade, or, to speak like Durkheim (1956: 101), a social "art" understood as "pure practice without theory," requires a pedagogy which is completely different from that suited to the teaching of knowledge (*savoirs*). As can be clearly seen in societies without writing and schools—but this remains true of what is transmitted within societies with formal schooling and even within schools themselves—a number of modes of thinking and action, and oftentimes the most vital ones, are transmitted from practice to practice, through total and practical modes of transmission founded upon direct and lasting contact between the one who teaches and the one

5. See Bourdieu's (1968b) discussion in "Structuralism and Theory of Sociological Knowledge," where he sets out his debt to, and differences with, structuralism as a social epistemology.

who learns ("Do as I do").⁶ Historians and philosophers of science, and especially scientists themselves, have often observed that a good part of the craft of the scientist is acquired via modes of transmission that are thoroughly practical.⁷ And the part played by the pedagogy of silence, which leaves little room for explication of both the schemata transmitted and the schemata at work in the process of transmission itself, is surely all the greater in those sciences where the contents of knowledge and the modes of thinking and of action are themselves less explicit and less codified.

Sociology is a more advanced science than is ordinarily believed, even among sociologists. Perhaps a good criterion of the position of a social scientist in his or her discipline might be how high his idea is of what he must master in order to be abreast of the achievements of his science. The propensity to evolve an unpretentious grasp of your scientific capabilities cannot but increase as your knowledge of the most recent achievements in matters of method, techniques, concepts or theories, grows. But sociology is yet little codified and little formalized. Therefore one cannot, as much as is done elsewhere, rely on automatisms of thinking or on the automatisms that take the place of thinking (on the *evidentia ex terminis*, the "blinding evidence" of symbols that Leibniz used to oppose to Cartesian *evidence*), or yet on all these codes of proper scientific conduct—methods, protocols of observation, etc.—that constitute the law of the most codified scientific fields. Thus, in order to obtain adequate practices, one must count principally upon the embodied schemata of habitus.

The scientific habitus is a rule "made man," an embodied rule or, better, a scientific *modus operandi* that functions in a practical state according to the norms of science without having these norms as its explicit principle:⁸ it is this sort of scientific "feel for the game" (*sens du*

6. See Bourdieu 1990a. Connerton 1989 provides an effective and terse defense of this argument; also Jackson 1989: chap. 8.

7. See Kuhn 1970 and Latour and Woolgar 1979. This point is also supported by Rouse 1987 and Traweek 1989. Donald Schon (1983) shows in *The Reflective Practitioner* that professionals (in management and engineering, architecture, town planning and psychotherapy) know more than they can put into words; as competent practitioners, they "exhibit a kind of knowing-in-practice, most of which is tacit," and rely on improvisation learned in action rather than on formulas learned in graduate school.

8. See Bourdieu 1990g and Brubaker 1989a for an analysis of Bourdieu's theory as a working scientific habitus.

jeu) that causes us to do what we do at the right moment without needing to thematize what had to be done and still less the knowledge of the explicit rule that allows us to generate this conformable practice. Thus the sociologist who seeks to transmit a scientific habitus has more in common with a high-level sports coach than with a Professeur at the Sorbonne. He or she says very little by way of first principles and general precepts. Of course, she may set those forth as I did in *Le métier de sociologue*, but only if she knows that she cannot stop at that point: there is nothing worse, in a sense, than epistemology when it becomes a topic for society conversation and essays⁹ and a *substitute for research*. She proceeds by way of practical suggestions, and in this she looks very much like a coach who mimics a move ("if I were you I would do this . . .") or by "correcting" practices as they are executed, in the spirit of practice itself ("I would not ask this question, at least not in this form").

2 Thinking Relationally

None of this could be truer than when it comes to the construction of the object, no doubt the most crucial research operation and yet the most completely ignored, especially by the dominant tradition, organized as it is around the opposition between "theory" and "methodology." The paradigm (in the sense of exemplary instantiation) of theoreticist "theory" is offered by the work of Parsons, that conceptual melting pot¹⁰ produced by purely theoretical compilation (that is, entirely foreign to any application) of a select few grand oeuvres (Durkheim, Pareto, Weber, and Marshall—and, curiously, not Marx) reduced to their "theoretical" or, better, professorial dimension, or yet, closer to us, by the "neo-functionalism" of Jeffrey Alexander.¹¹ Born of the necessities of teaching, such eclectic and classificatory compilations are good for teaching, but for no other purpose. On the other side, we find "methodology," that catalogue of precepts that properly pertain neither to epistemology, understood as reflection aimed at uncovering the schemata of scientific practice apprehended

9. "Essay" does not capture the slightly pejorative connotation of the French *dissertation* as an empty and gratuitous discourse.

10. In English in the original.

11. See Parsons 1937, Alexander 1980–82, 1985, and Alexander's (1987b) *Twenty Lectures*, which originated in a series of course lectures to undergraduates.

in its failures as well as in its successes, nor to scientific theory. I think here of Paul Lazarsfeld. The couple formed by Parsons and Lazarsfeld (with Merton and his theories of the "middle range" standing midway between the two) has formed a sort of socially very powerful "scientific" holding that reigned over world sociology for the better part of three decades after World War II.¹² The division between "theory" and "methodology" establishes as an epistemological opposition an opposition that is in fact constitutive of the social division of scientific labor at a certain time (expressed by the opposition between professors and the staff of bureaus of applied research).¹³ I believe that this division into two separate instances must be completely rejected, as I am convinced that one cannot return to the concrete by combining two abstractions.

Indeed, the most "empirical" technical choices cannot be disentangled from the most "theoretical" choices in the construction of the object. It is only as a function of a definite construction of the object that such a sampling method, such a technique of data collection and analysis, etc., becomes imperative. More precisely, it is only as a function of a body of hypotheses derived from a set of theoretical presuppositions that any empirical datum can function as a proof or, as Anglo-American scholars put it, as *evidence*.¹⁴ Now, we often proceed as if what counts as evidence was evident because we trust a *cultural routine*, most often imposed and inculcated through schooling (the famous "methodology" courses taught at American universities). The fetishism of "evidence" will sometimes lead one to reject empirical works that do not accept as self-evident the very definition of "evidence." Every researcher grants the status of data only to a small fraction of the given, yet not, as it should be, to the fraction called forth by his or her problematics, but to that fraction vouchsafed and guaranteed by the pedagogical tradition of which they are part and, too often, by that one tradition alone.

It is revealing that entire "schools" or research traditions should develop around *one* technique of data collection and analysis. For ex-

12. For further elaboration, see Bourdieu 1988e. Pollak (1979, 1980) sketches an analysis of Lazarsfeld's activities aimed at the methodical exportation of positivist social science—canons and institutions—outside of the United States.

13. Coleman (1990a) offers rich biographical reminiscences on these two "poles" of Columbia sociology and on their rapprochement and mutual legitimation in the 1950s.

14. In English in the original.

ample, today some ethnomethodologists want to acknowledge nothing but conversation analysis reduced to the exegesis of a text, completely ignoring the data on the immediate context that may be called ethnographic (what is traditionally labeled the "situation"), not to mention the data that would allow them to situate this situation within the social structure. These "data," which are (mis)taken for the *concrete* itself, are in fact the product of a formidable *abstraction*—it is always the case since all data are constructions—but in this case an abstraction which ignores itself as such.¹⁵ Thus we will find monomaniacs of log-linear modeling, of discourse analysis, of participant observation, of open-ended or in-depth interviewing, or of ethnographic description. Rigid adherence to this or that one method of data collection will define membership in a "school," the symbolic interactionists being recognizable for instance by the cult of participant observation, ethnomethodologists by their passion for conversation analysis, status attainment researchers by their systematic use of path analysis, etc. And the fact of combining discourse analysis with ethnographic description will be hailed as a breakthrough and a daring challenge to methodological monotheism! We would need to carry out a similar critique in the case of techniques of statistical analysis, be they multiple regression, path analysis, network analysis, factor analysis, or event-history analysis. Here again, with a few exceptions, monotheism reigns supreme.¹⁶ Yet the most rudimentary sociology of sociology teaches us that methodological indictments are too often no more than a disguised way of making a virtue out of necessity, of feigning to dismiss, to ignore in an active way, what one is ignorant of in fact.

And we would need also to analyze the rhetoric of data presentation which, when it turns into an ostentatious display of data, often serves to mask elementary mistakes in the construction of the object, while at the opposite end, a rigorous and economical exposition of the *pertinent* results will, measured by the yardstick of such an exhibitionism of the *datum brutum*, oftentimes incur the *a priori* suspicion of

15. See Bourdieu's (1990d) analysis of the discursive interaction between house buyers and house sellers and, for contrast, compare his structural constructivism with the straightforward interactional discourse-analytic framework of Schegloff 1987.

16. "Give a hammer to a child," warns Abraham Kaplan (1964:112) "and you will see that that everything will seem to him to deserve to be hit with it." Everett C. Hughes's (1984) discussion of "methodological ethnocentrism" is relevant here.

the fetishizers of the *protocol* (in the twofold sense of the term) of a form of "evidence." Poor science! How many scientific crimes are committed in thy name! . . . To try to convert all these criticisms into a positive precept, I will say only that we must beware of all sectarian dismissals which hide behind excessively exclusive professions of faith. We must try, in every case, to mobilize all the techniques that are relevant and practically usable, given the definition of the object and the practical conditions of data collection. One can, for instance, utilize correspondence analysis for carrying out a discourse analysis, as I recently did in the case of the advertisement strategies of various firms involved in the construction of single-family homes in France (Bourdieu 1990c), or combine the most standard statistical analysis with a set of in-depth interviews or ethnographic observations, as I tried to do in *Distinction* (Bourdieu 1984a). The long and the short of it is, social research is something much too serious and too difficult for us to allow ourselves to mistake scientific *rigidity*, which is the nemesis of intelligence and invention, for scientific *rigor*, and thus to deprive ourselves of this or that resource available in the full panoply of intellectual traditions of our discipline and of the sister disciplines of anthropology, economics, history, etc. In such matters, I would be tempted to say that only one rule applies: "it is forbidden to forbid,"¹⁷ or, watch out for methodological watchdogs! Needless to say, the extreme liberty I advocate here (which seems to me to make obvious sense and which, let me hasten to add, has nothing to do with the sort of relativistic epistemological *laissez faire* which seems so much in vogue in some quarters) has its counterpart in the extreme vigilance that we must apply to the conditions of use of analytical techniques and to ensuring that they fit the question at hand. I often find myself thinking that our methodological "police" (*pères-la-rigueur*) prove to be rather unrigorous, even lax, in their use of the very methods of which they are zealots.

Perhaps what we will do here will appear to you insignificant. But, first, the construction of an object—at least in my personal research experience—is not something that is effected once and for all, with one stroke, through a sort of inaugural theoretical act. The program of observation and analysis through which it is effected is not a blue-

17. The reader will recognize here the famed May '68 French slogan, *il est interdit d'interdire*.

print that you draw up in advance, in the manner of the engineer. It is, rather, a protracted and exacting task that is accomplished little by little, through a whole series of small rectifications and amendments inspired by what is called *le métier*, the "know-how," that is, by the set of practical principles that orients choices at once minute and decisive. It is thus in reference to a somewhat glorified and rather unrealistic notion of research that some would express surprise at the fact that we should discuss at such length apparently negligible details such as whether the researcher ought to disclose his status as a sociologist or take the cover of a less threatening identity (say, that of ethnographer or historian) or hide it entirely, or whether it is better to include such questions in a survey instrument designed for statistical analysis or to reserve it for in-depth, face-to-face interviews with a select number of informants, and so on.

This constant attention to the details of the research procedure, whose properly social dimension (how to locate reliable and insightful informants, how to present yourself to them, how to describe the aims of your research and, more generally, how to "enter" the world under study, etc.) is not the least important, should have the effect of putting you on notice against the fetishism of concepts, and of "theory," born of the propensity to consider "theoretical" instruments—habitus, field, capital, etc.—in themselves and for themselves, rather than to put them in motion and to make them *work*. Thus the notion of field functions as a conceptual shorthand of a mode of construction of the object that will command, or orient, all the practical choices of research. It functions as a *pense-bête*, a memory-jogger: it tells me that I must, at every stage, make sure that the object I have given myself is not enmeshed in a network of relations that assign its most distinctive properties. The notion of field reminds us of the first precept of method, that which requires us to resist by all means available our primary inclination to think the social world in a substantialist manner. To speak like Cassirer (1923) in *Substance and Function*: one must *think relationally*. Now, it is easier to think in terms of realities that can be "touched with the finger," in a sense, such as groups or individuals, than in terms of relations. It is easier for instance to think of social differentiation in the form of groups defined as populations, as with the realist notion of class, or even in terms of antagonisms between these groups, than in the form of a space of re-

lations.¹⁸ The ordinary objects of research are realities which are pointed out to the researcher by the fact that they "stand out," in a sense, by "creating problems"—as, for instance, in the case of "teenage welfare mothers in Chicago's black ghetto." Most of the time, researchers take as objects of research the problems of social order and domestication posed by more or less arbitrarily defined populations, produced through the successive partitioning of an initial category that is itself pre-constructed: the "elderly," the "young," "immigrants," "semi-professions," or the "poverty population," and so on. Take for instance "The youth of the western housing project of Villeurbanne."¹⁹ The first and most pressing scientific priority, in all such cases, would be *to take as one's object the social work of construction of the pre-constructed object*. That is where the point of genuine rupture is situated.

To escape from the realist mode of thinking, however, it does not suffice to employ the grand words of Grand Theory. For instance, concerning power, some will raise substantialist and realist questions of location (in the manner of those cultural anthropologists who wandered in an endless search for the "locus of culture"); others will ask where power comes from, from the top or from the bottom ("Who Governs?"), as did those sociolinguists who worried about where the locus of linguistic change lies, among the petty bourgeois or among the bourgeois, etc.²⁰ It is for the purpose of breaking with this substantialist mode of thinking, and not for the thrill of sticking a new label on old theoretical wineskins, that I speak of the "field of power" rather than of the dominant class, the latter being a realist concept designating an actual population of holders of this tangible reality that we call power. By field of power, I mean the relations of force that obtain between the social positions which guarantee their occupants a

18. See Bourdieu 1985a, 1987b, 1989e for elaborations. Bourdieu draws on the work of logician Peter F. Strawson (1959) to ground his relational conception of social space and of the epistemological status of individuals in it.

19. A structural equivalent for the United States would be something like the "gang members of Chicago's South Side housing projects."

20. On the search for the locus of power, see Robert Dahl's (1961) *Who Governs*, and the "community power structure" debate for the view "from above." The view "from below" is represented by the tradition of proctological historiography and recent anthropology (e.g., Scott 1985). On the locus of linguistic change, see Labov 1980.

quantum of social force, or of capital, such that they are able to enter into the struggles over the monopoly of power, of which struggles over the definition of the legitimate form of power are a crucial dimension (I think here in particular of the confrontation between "artists" and "bourgeois" in the late nineteenth century.)²¹

This being said, one of the main difficulties of relational analysis is that, most of the time, social spaces can be grasped only in the form of distributions of properties among individuals or concrete institutions, since the data available are attached to individuals or institutions. Thus, to grasp the subfield of economic power in France, and the social and economic conditions of its reproduction, you have little choice but to interview the top two hundred French CEOs (Bourdieu and de Saint Martin 1978; Bourdieu 1989a: 396–481). When you do so, however, you must beware at every moment of regression to the "reality" of preconstructed social units. To guard against it, I suggest that you use this very simple and convenient instrument of construction of the object: the *square-table of the pertinent properties of a set of agents or institutions*. If, for example, my task is to analyze various combat sports (wrestling, judo, aikido, boxing, etc.), or different institutions of higher learning, or different Parisian newspapers, I will enter each of these institutions on a line and I will create a new column each time I discover a property necessary to characterize one of them; this will oblige me to question all the other institutions on the presence or absence of this property. This may be done at the purely inductive stage of initial locating. Then I will pick out redundancies and eliminate columns devoted to structurally or functionally equivalent traits so as to retain all those traits—and only those traits—that are capable of discriminating between the different institutions and are thereby analytically relevant. This very simple instrument has the virtue of forcing you to think relationally both the social units under consideration and their properties, which can be characterized either in terms of presence and absence (yes/no) or gradationally (+, 0, –, or 1, 2, 3, 4, 5).

It is at the cost of such a work of construction, which is not done in one stroke but by trial and error, that one progressively constructs so-

21. On the field of power see Bourdieu 1989a and above, part 1, sec. 3; on the clash between "artists" and "bourgeois" at the close of the nineteenth century in France, see Bourdieu 1983d and 1988d, and Charle 1987.

cial spaces which, though they reveal themselves only in the form of highly abstract, objective relations, and although one can neither touch them nor "point to them," are what makes the whole reality of the social world. I will refer you here to the work I recently published (Bourdieu 1989a) on the *Grandes écoles*²² and in which I recount, by means of a very condensed chronicle of a research project spread over the better part of two decades, how one moves from monography to a genuinely constructed scientific object, in this case the field of the academic institutions entrusted with the reproduction of the field of power in France. It becomes all the more difficult to avoid falling into the trap of the preconstructed object in that I am here dealing with an object in which I am by definition interested without clearly knowing what the veritable principle of that "interest" is. It could be, for example, the fact that I am an alumnus of the *Ecole normale supérieure*.²³ The first-hand knowledge I have of it, which is all the more pernicious when it is experienced as demystified and demystifying, generates a whole series of supremely naive questions that every *normalien* will find interesting because they immediately "come to the mind" of the *normalien* who wonders about his or her school, that is, about himself or herself: for example, does the ranking upon entry into the school contribute to determining the choice of disciplines, mathematics and physics or literature and "philo"? (The spontaneous problematic, in which a considerable measure of narcissistic complacency enters, is in fact ordinarily much more naive than this. I can refer you here to the myriad volumes claiming scientific status published over the last twenty years on this or that *Grande école*.) One can end up writing a voluminous book packed with facts that have every appearance of being perfectly scientific but which misses the root of the mat-

22. The French *Grandes écoles* are elite graduate schools that are separate from the regular university system. They include the *Ecole nationale d'administration* (ENA), which prepares higher civil servants, created in 1945; the *Ecole des hautes études commerciales* (HEC, est. 1881), which trains executives and business experts; the *Ecole polytechnique* and the *Ecole Centrale* (for engineers, 1794); and the *Ecole normale supérieure* (1794), which produces top teachers and university professors. Entrance to these schools is by highly selective national competitive examinations after one to four years of special post-high school preparatory education.

23. Pierre Bourdieu graduated from the *Ecole normale supérieure* (thereby becoming a *normalien*) in 1954, three years after Foucault, one year before Jacques Derrida, and along with historian Le Roy Ladurie and literary theorist Gérard Genette.

ter, if, as I believe, the Ecole normale supérieure, to which I may be tied by affective attachments, positive or negative, produced by my prior investments, is in reality but a point in a space of objective relations (a point whose "weight" in the structure will have to be determined); or if, to be more precise, the truth of this institution resides in the network of relations of opposition and competition which link it to the whole set of institutions of higher learning in France, and which link this network itself to the total set of positions in the field of power which these schools grant access to. If it is indeed true that the real is relational, then it is quite possible that I know nothing of an institution about which I think I know everything, since it is nothing outside of its relations to the whole.

Whence the problems of strategy that one cannot avoid, and which will crop up again and again in our discussions of research projects. The first of these may be posed as follows: is it better to conduct an extensive study of the totality of the relevant elements of the object thus constructed or to engage in an intensive study of a limited fragment of that theoretical ensemble devoid of theoretical justification? The choice most often approved of socially, in the name of a naively positivist idea of precision and "seriousness," is the second one, that which consists of "studying exhaustively a very precise and well-circumscribed object," as thesis advisors like to say. (It would be too easy to show how such typically petty bourgeois virtues as "prudence," "seriousness," "honesty," and so on, which would be as apposite in the management of a small business or in a mid-level bureaucratic position, are here transmuted into "scientific method"; and also how a socially approved nonentity—the "community study" or the organizational monograph—can accede to recognized scientific existence as a result of a classical effect of social magic.)

In practice, we shall see that the issue of the boundaries of the field, apparently a positivist question to which one can give a theoretical answer (an agent or an institution belongs to a field inasmuch as it produces and suffers effects in it), will come up time and again. Consequently you will almost always be confronted with this alternative between the intensive analysis of a practically graspable fragment of the object and the extensive analysis of the true object. The scientific profit to be gained from knowing the space from which you have isolated the object under study (for instance a particular elite school) and that you must try to map out even roughly, with second-

ary data for lack of better information, resides in that, by knowing what you do and what the reality from which the fragment has been *abstracted* consists of, you can at least adumbrate the main force lines that structure the space whose constraints bear upon the point under consideration (in a manner similar to those nineteenth-century architects who drew wonderful charcoal sketches of the totality of the building inside of which the part that they wanted to represent in detail was located). Thus you will not run the risk of searching (and "finding") in the fragment studied mechanisms or principles that are in reality external to it, residing in its relations to other objects.

To construct a scientific object also demands that you take up an active and systematic posture vis-à-vis "facts." To break with empiricist passivity, which does little more than ratify the preconstructions of common sense, without relapsing into the vacuous discourse of grand "theorizing," requires not that you put forth grand and empty theoretical constructs but that you tackle a very concrete empirical case with the purpose of *building a model* (which need not take a mathematical or abstract form in order to be rigorous). You must link the pertinent data in such a manner that they function as a self-propelling program of research capable of generating systematic questions liable to be given systematic answers, in short, to yield a coherent system of relations which can be put to the test *as such*. The challenge is *systematically* to interrogate the particular case by constituting it as a "particular instance of the possible," as Bachelard (1949) put it, in order to extract general or invariant properties that can be uncovered only by such interrogation. (If this intention is too often lacking in the work of historians, it is no doubt because the definition of their task inscribed in the social definition of their discipline is less ambitious, or pretentious, but also less demanding, on this count, than that thrust upon the sociologist.)

Analogical reasoning, based on the reasoned intuition of homologies (itself founded upon knowledge of the invariant laws of fields) is a powerful instrument of construction of the object. It is what allows you to immerse yourself completely in the particularity of the case at hand without drowning in it, as empiricist idiography does, and to realize the intention of *generalization*, which is science itself, not through the extraneous and artificial application of formal and empty conceptual constructions, but through this particular manner of thinking the particular case which consists of actually thinking it as

such. This mode of thinking fully accomplishes itself logically in and through the *comparative method* that allows you to think relationally a particular case constituted as a "particular instance of the possible" by resting on the structural homologies that exist between different fields (e.g., between the field of academic power and the field of religious power via the homology between the relations professor/intellectual, bishop/theologian) or between different states of the same field (the religious field in the Middle Ages and today for instance).²⁴

If this seminar functions as I want, it will offer a practical social re-actualization of the method I am trying to advance. In it, you will listen to people who are working on very different objects and who will submit themselves to a questioning constantly guided by the same principles, so that the *modus operandi* of what I wish to transmit will be transmitted in a sense *practically*, through its repeated application to various cases, without need for explicit theoretical explication. While listening to others, each of us will think about his or her own research, and the situation of institutionalized comparison thereby created (as with ethics, this method functions only if it can be inscribed in the mechanisms of a social universe) will oblige each participant, at once and without contradiction, both to particularize her object, to perceive it as a particular case (this, against one of the most common fallacies of social science, namely the universalization of the particular case), and to generalize it, to discover, through the application of general questions, the invariant properties that it conceals under the appearance of singularity. (One of the most direct effects of this mode of thinking is to forbid the kind of semigeneralization that leads one to produce abstract-concrete concepts born of the smuggling, into the scientific universe, of unanalyzed native words or facts.) During the time when I was a more guiding supervisor, I strongly advised researchers to study at least *two* objects, for instance to take, in the case of historians, besides their principal object (say, a publisher under the Second Empire), the contemporary equivalent of this object (a Parisian publishing house). The study of the present has at least the virtue of forcing the historian to objectivize and to control the prenotions that he is likely to project onto the past, if only by the fact that he uses words of the present to name past practices, such as the

24. See Bourdieu 1971b and "Maxwell's Devil: The Structure and Genesis of the Religious Field" in Bourdieu forthcoming a.

word "artist" which often makes us forget that the corresponding notion is an extraordinarily recent invention (Bourdieu 1987d, 1987j, 1988d).²⁵

3 A Radical Doubt

The construction of a scientific object requires first and foremost a break with common sense, that is, with the representations shared by all, whether they be the mere commonplaces of ordinary existence or official representations, often inscribed in institutions and thus present both in the objectivity of social organizations and in the minds of their participants. *The preconstructed is everywhere*. The sociologist is literally beleaguered by it, as everybody else is. The sociologist is thus saddled with the task of knowing an object—the social world—of which he is the product, in a way such that the problems that he raises about it and the concepts he uses have every chance of being the product of this object itself. (This is particularly true of the classificatory notions he employs in order to know it, common notions such as names of occupations or scholarly notions such as those handed down by the tradition of the discipline.) Their self-evident character arises from the fit between objective structures and subjective structures which shields them from questioning.

How can the sociologist effect in practice this radical doubting which is indispensable for bracketing all the presuppositions inherent in the fact that she is a social being, that she is therefore socialized and led to feel "like a fish in water" within that social world whose structures she has internalized? How can she prevent the social world itself from carrying out the construction of the object, in a sense, through her, through these unself-conscious operations or operations unaware of themselves of which she is the apparent subject? To not construct, as positivist hyperempiricism does when it accepts without critical examination the concepts that offer themselves to it ("achievement" and "ascription," "profession," "actor," "role," etc.)

25. Similarly, Charle (1990) has shown that "intellectuals," as a modern social group, schema of perception, and political category, are a recent "invention," which took place in France in the late nineteenth century and crystallized around the Dreyfus affair. For him, as for Bourdieu (1989d), to apply the notion indiscriminately to thinkers and writers of prior epochs results in either anachronism or presentist analyses that end up obfuscating the historical singularity of "intellectuals."

is still to construct, because it amounts to recording—and thus to ratifying—something already constructed. Ordinary sociology, which bypasses the radical questioning of its own operations and of its own instruments of thinking, and which would no doubt consider such a *reflexive intention* the relic of a philosophic mentality, and thus a survival from a prescientific age, is thoroughly suffused with the object it claims to know, and which it cannot really know, because it does not know itself. A scientific practice that fails to question itself does not, properly speaking, know what it does. Embedded in, or taken by, the object that it takes as its object, it reveals something of the object, but something which is not really objectivized since it consists of the very principles of apprehension of the object.

It would be easy to show that this half-scholarly science²⁶ borrows its problems, its concepts, and its instruments of knowledge from the social world, and that it often records as a datum, as an empirical given independent of the act of knowledge and of the science which performs it, facts, representations or institutions which are the product of a prior stage of science. In short, it records itself without recognizing itself . . .

Let me dwell on each of these points for a moment. Social science is always prone to receive from the social world it studies the *issues* that it poses about that world. Each society, at each moment, elaborates a body of *social problems* taken to be legitimate, worthy of being debated, of being made public and sometimes officialized and, in a sense, *guaranteed by the state*. These are for instance the problems assigned to the high-level commissions officially mandated to study them, or assigned also, more or less directly, to sociologists themselves via all the forms of bureaucratic demand, research and funding programs, contracts, grants, subsidies, etc.²⁷ A good number of ob-

26. In French *science demi-savante*.

27. A prime example would be the field of poverty research in the United States, whose creation is largely a by-product of the 1960s "War on Poverty" and of the subsequent demands of the state for knowledge on populations it had failed to domesticate. The official redefinition of the problem effected by the Office of Economic Opportunity in 1964 turned what was hitherto a sociopolitical issue into a legitimate area of "scientific" inquiry, thereby drawing scores of scholars—especially economists—to new research centers, journals, and conferences devoted to poverty and its public management, eventually leading to the institutionalization of the highly technical (and highly ideological) discipline of "public policy analysis." This entailed not only the uncritical adoption by social scientists of bureaucratic categories and government mea-

jects recognized by official social science and a good many titles of studies are nothing other than social problems that have been smuggled into sociology—poverty, delinquency, youth, high school drop-outs, leisure, drunken driving, and so on—and which vary with the fluctuations of the social or scholarly consciousness of the time, as an analysis of the evolution over time of the main realist divisions of sociology would testify (these can be grasped through the subheadings used in mainstream journals or in the names of research groups or sections convening periodically at the World Congress of Sociology).²⁸ Here is one of the mediations through which the social world constructs its own representation, by using sociology and the sociologist

surements (such as the famed federal "poverty line" which continues to define the boundaries of discourse despite its oft-revealed and growing conceptual inadequacies) and concerns (Does welfare receipt make poor people work less? Do public aid recipients share a culture or engage in behaviors that violate "mainstream" norms? What are the most economical means to make them "self-sufficient"—i.e., socially and politically invisible?) which has reified the moralistic and individualistic perception of poverty by the dominant into "scientific facts" (Katz 1989: 112–23). Haveman (1987) makes a good case that, in the process, the federal government also reshaped the face of social science *in toto*: in 1980, poverty-related research absorbed fully 30 percent of all federal research expenditures compared to .6 percent in 1960. The recent spread of discourse on the "underclass" is a further illustration of how a major influx of funding triggered by foundations can redefine the terms of social scientific debate without critical discussion of the premises built into the new demand.

28. This can also readily be seen in the evolution of the categories used to sort out books in the journal of reviews *Contemporary Sociology*, or in changes in the chapter headings of handbooks (e.g., Smelser 1988) and in the entries of encyclopedias of social science. The taxonomy of topics used by the *Annual Review of Sociology* is a good example of a mix of commonsensical, bureaucratic, and plainly arbitrary divisions inherited from the (academic) history of the discipline: it is a rare mind who can retrospectively impart a degree of (socio)logical coherence to the way it parcels out its subject matter. Opening each volume is the category "Theory and Methods," as always made into a self-contained topic. Then come "Social Processes," a category so broad that it is hard to see what could possibly not fall under it, and "Institutions and Culture," which hypostatizes culture into a distinct object. Why "Formal Organizations" have been separated from "Political and Economic Sociology" is unclear; how they can in turn be distinguished from "Stratification and Differentiation" is also moot. "Historical Sociology" has the dubious privilege of being reified into a separate specialty (presumably on the basis of method, but then should it not be regrouped with "Theory and Methods," and why do other approaches not have "their" sections?). Just why "Sociology of World Religion" has a rubric all to itself is a mystery. "Policy" is a direct offshoot of bureaucratic state demand for social knowledge. And, crowning all the other categories in its sanctification of common sense, the rubric "Individual and Society."

for this purpose. For a sociologist more than any other thinker, to leave one's thought in a state of unthought (*impensé*) is to condemn oneself to be nothing more than the *instrument* of that which one claims to think.

How are we to effect this rupture? How can the sociologist escape the underhanded persuasion which is exercised on her every time she reads the newspapers or watches television or even when she reads the work of her colleagues? The mere fact of being on the alert is important but hardly suffices. One of the most powerful instruments of rupture lies in the social history of problems, objects, and instruments of thought, that is, with the history of the work of social construction of reality (enshrined in such common notions as role, culture, youth, etc., or in taxonomies) which is carried out within the social world itself as a whole or in this or that specialized field and, especially, in the field of the social sciences. (This would lead us to assign to the teaching of the social history of the social sciences—a history which, for the most part, remains to be written—a purpose entirely different from the one it presently serves.) A good part of the collective work that finds an outlet in *Actes de la recherche en sciences sociales* deals with the social history of the most ordinary objects of ordinary existence. I think for instance of all those things that have become so common, so taken for granted, that nobody pays any attention to them, such as the structure of a court of law, the space of a museum, a voting booth, the notion of “occupational injury” or of “cadre,” a two-by-two table or, quite simply, the act of writing or taping.²⁹ History thus conceived is inspired not by an antiquarian interest but by a will to understand why and how one understands.

To avoid becoming the object of the problems that you take as your object, you must retrace the history of the *emergence* of these problems, of their progressive constitution, i.e., of the collective work, oftentimes accomplished through competition and struggle, that proved necessary to make such and such issues to be known and recognized (*faire connaître et reconnaître*) as *legitimate problems*, problems that are allowable, publishable, public, official. One thinks here of the problem of “work accidents” or occupational hazards studied by Rémi Lenoir (1980) or of the invention of the “elderly” (*troisième âge*) scru-

29. See, respectively, Lenoir 1980, Boltanski 1979, Garrigou 1988, Bourdieu 1977a: 36–38, 188, and Sayad 1985.

tinized by Patrick Champagne (1979) and, more generally, to such staples of the sociology of “social problems” as family, divorce, delinquency, drugs, and female labor force participation. In all these cases we will discover that the problem that ordinary positivism (which is the first inclination of every researcher) takes for granted has been *socially produced*, in and by a *collective work of construction of social reality*,³⁰ and that it took meetings and committees, associations and leagues, caucuses and movements, demonstrations and petition drives, demands and deliberations, votes and stands, projects, programs, and resolutions to cause what was and could have remained a *private*, particular, singular problem to turn into a *social problem*, a public issue that can be publicly addressed (think of the fate of abortion or homosexuality)³¹ or even an official problem that becomes the object of official decisions and policies, of laws and decrees.

Here one would need to analyze the particular role of the political field (Bourdieu 1981a) and especially of the bureaucratic field. Through the very peculiar logic of the *administrative commission*, a logic that I am currently investigating in the case of the elaboration of the public policy of individual housing assistance in France around 1975,³² the bureaucratic field contributes decisively to the constitution, and to the consecration, of “universal” social problems. The imposi-

30. While Bourdieu's position may appear akin to the “social constructionist” approach to social problems (e.g., Schneider 1985, Gusfield 1981, Spector and Kitsuse 1987), it differs substantially from the latter in that it grounds the social work of symbolic and organizational construction in the objective structure of the social spaces within which the latter takes place. This grounding operates at the level of the positions and the dispositions of claim makers and claim takers. Bourdieu advocates neither a “strict” nor a “contextual” constructionist position (as defined by Best 1989: 245–89) but a “structural constructivism” which causally relates claims-making and their products to objective conditions. See Champagne 1990 for an analysis of the social construction of “public opinion” along those lines.

31. Kristin Luker (1984) and Faye Ginsburg (1988) offer detailed historical and ethnographic accounts of the social construction of abortion as a public issue at the political and grass-roots level. Pollak (1988a) sketches an analysis of the public framing of the link between AIDS and homosexuality in recent French political discourse. Boltanski unravels the conditions of efficacy of strategies designed to transform personal incidents and outrage into socially accepted issues and injustices in his important article on “Denunciation” (Boltanski with Daré and Schültz 1984, and Boltanski 1990).

32. See the entire March 1990 issue of *Actes de la recherche en sciences sociales* devoted to “The Economics of Housing” (Bourdieu 1990b, 1990c, 1990d; Bourdieu and de Saint Martin 1990; Bourdieu and Christin 1990).

tion of *problématique* that the sociologist—as every other social agent—suffers and of which he becomes a relay and support every time he takes up on his own account questions which are an expression of the sociopolitical mood of the times (for instance by including them in his survey questionnaires or, worse, by designing his survey around them) is all the more likely when the problems that are *taken for granted* in a given social universe are those that have the greatest chances of being allocated *grants*,³³ material or symbolic, of being, as we say in French, *bien vus*, in high favor with the managers of scientific bureaucracies and with bureaucratic authorities such as research foundations, private firms, or governmental agencies. (This explains why public opinion polls, the “science without scientist,” always begot the approval of those who have the means of commissioning them and who otherwise prove so critical of sociology whenever the latter breaks with their demands and commands.)³⁴

I will only add, to complicate things still a bit more, and to make you see how difficult, indeed well-nigh desperate, the predicament of the sociologist is, that the work of production of official problems, that is, those problems endowed with the sort of universality that is granted by the fact of being guaranteed by the state, almost always leaves room for what are today called *experts*. Among those so-called experts are sociologists who use the authority of science to endorse the universality, the objectivity, and the disinterestedness of the bureaucratic representation of problems. This is to say that any sociologist worthy of the name, i.e., who does what, according to me, is required to have some chance of being the *subject* of the problems she can pose about the social world, must include in her object the contribution that sociology and sociologists (that is, her own peers) make, in all candor, to the production of official problems—even if this is very likely to appear as an unbearable mark of arrogance or as a betrayal of professional solidarity and corporatist interests.

33. In English in the text: here Bourdieu plays on the words “grants” and “for granted” to emphasize the organic link between the material and the cognitive imposition of problematics.

34. Ever since the introduction of public opinion polls in French political life in the 1960s, Bourdieu has been a persistent and often caustic critic of their social uses. His 1971 paper provocatively entitled “Public Opinion Does Not Exist” (Bourdieu 1979e) has been reprinted in numerous collections and journals and translated into six languages. This issue is broached again in “A Science Without Scientist” (Bourdieu 1987a: 217–24).

In the social sciences, as we well know, epistemological breaks are often social breaks, breaks with the fundamental beliefs of a group and, sometimes, with the core beliefs of the body of professionals, with the body of shared certainties that found the *communis doctorum opinio*. To practice radical doubt, in sociology, is akin to becoming an outlaw. This was no doubt acutely felt by Descartes, who, to the dismay of his commentators, never extended the mode of thinking that he so intrepidly inaugurated in the realm of knowledge to politics (see the prudence with which he talks of Machiavelli).

I now come to the concepts, the words, and the methods that the “profession”³⁵ employs to speak about, and to think, the social world. Language poses a particularly dramatic problem for the sociologist: it is in effect an immense repository of naturalized preconstructions,³⁶ and thus of preconstructions that are ignored as such and which can function as unconscious instruments of construction. I could take here the example of *occupational taxonomies*, whether it be the names of occupations that are in currency in daily life or the socioeconomic categories of INSEE (the French National Institute of Economic and Statistical Research), an exemplary instance of *bureaucratic conceptualization*,³⁷ of the bureaucratic universal, and, more generally, the example of all the taxonomies (age groups, young and old, gender categories, which we know are not free from social arbitrary) that sociologists use without thinking about them too much because they are the social categories of understanding shared by a whole society.³⁸ Or,

35. In English in the original, as Bourdieu prepares to critique the Anglo-American sociological concept of “profession.”

36. Or, in Wittgenstein’s (1977: 18) words, “Language sets everyone the same traps; it is an immense network of easily accessible wrong turnings.” This view is shared by Elias (1978a: 111) who counts “inherited structures of speech and thought” among the most serious obstacles to a science of society: “The means of speaking and thinking available to sociologists at present are for the most part unequal to the task we ask them to perform.” He points out in particular, following Benjamin Lee Whorf, that Western languages tend to foreground substantives and objects at the expense of relations and to reduce processes to static conditions.

37. Another example would be the bureaucratic invention, and subsequent reification, of the “poverty line” in U.S. social “science” (Beeghley 1984; Katz 1989: 115–17).

38. Maurice Halbwachs (1972: 329–48) showed long ago that there is nothing “natural” about the category of age. Pialoux (1978), Thévenot (1979), Mauger and Fossé-Polliak (1983), and Bourdieu’s (1980b: 143–54) “Youth is Nothing But a Word” carry that argument further in the case of youth. Champagne (1979) and Lenoir (1978) apply it to

as in the case of what I called the "categories of professorial judgment" (the system of paired adjectives used to evaluate the papers of students or the virtues of colleagues [Bourdieu 1988a: 194–225]), they belong to their professional corporation (which does not exclude their being founded, in the final analysis, upon homologies between structures, i.e., upon the fundamental oppositions of social space, such as rare/banal, unique/common, etc.).

But I believe that one must go further and call into question not only classifications of occupations and the concepts used to designate classes of jobs, but the very concept of occupation itself, or of *profession*, which has provided the basis for a whole tradition of research and which, for some, stands as a kind of methodological motto. I am well aware that the concept of "profession" and its derivatives (professionalism, professionalization, etc.) has been severely and fruitfully questioned in the works of Magali Sarfatti Larson (1977), Randall Collins (1979), Elliott Friedson (1986), and Andrew Abbott (1988) in particular, who have highlighted, among other things, the conflicts endemic to the world of professions. But I believe that we must go beyond this critique, however radical, and try, as I do, to *replace* this concept with that of field.

The notion of profession is all the more dangerous because it has, as always in such cases, all appearance of neutrality in its favor and because its use has been an improvement over the theoretical jumble (*bouillie*) of Parsons. To speak of "profession" is to fasten on a true reality, onto a set of people who bear the same name (they are all "lawyers" for instance); they are endowed with a roughly equivalent economic status and, more importantly, they are organized into "professional associations" endowed with a code of ethics, collective bodies that define rules for admission, etc. "Profession" is a folk concept which has been uncritically smuggled into scientific language and which imports into it a whole social unconscious. It is the *social product* of a historical work of construction of a group and of a

the sociopolitical construction of the "elderly." Countless historical studies of gender relations have, in recent years, demonstrated the arbitrariness of the categories of male and female; perhaps the most incisive of these is Joan Scott's (1988); see also several of the articles published in the two issues of *Actes de la recherche en sciences sociales* on "Male/Female" (June and September 1990). For an extended discussion of the struggles over the definition of "natural" categories, see Lenoir (in Champagne et al. 1989: 61–77).

representation of groups that has surreptitiously slipped into the science of this very group. This is why this "concept" works so well, or too well in a way: if you accept it to construct your object, you will find directories on hand, lists and biographies drawn up, bibliographies compiled, centers of information and data bases already constituted by "professional" bodies, and, provided that you be a bit shrewd, funds to study it (as is very frequent in the case of lawyers for instance). The category of profession refers to realities that are, in a sense, "too real" to be true, since it grasps at once a mental category and a social category, socially produced only by superseding or obliterating all kinds of economic, social, and ethnic differences and contradictions which make the "profession" of "lawyer," for instance, a space of competition and struggle.³⁹

Everything becomes different, and much more difficult if, instead of taking the notion of "profession" at face value, I take seriously the work of *aggregation* and symbolic imposition that was necessary to produce it, and if I treat it as a field, that is, as a structured space of social forces and struggles.⁴⁰ How do you draw a sample in a field? If, following the canon dictated by orthodox methodology, you take a random sample, you mutilate the very object you have set out to construct. If, in a study of the juridical field, for instance, you do not draw the chief justice of the Supreme Court, or if, in an inquiry into the French intellectual field of the 1950s, you leave out Jean-Paul Sartre, or Princeton University in a study of American academia, your field is destroyed, insofar as these personas or institutions alone mark a crucial position. There are positions in a field that admit only one occupant but command the whole structure.⁴¹ With a random or representative sample of artists or intellectuals conceived as a "profession," however, *no problem*.⁴²

39. See the two issues of *Actes de la recherche en sciences sociales* on law and legal experts, no. 64 (September 1986), and no. 76/77 (March 1989, particularly the articles by Yves Dezalay, Alain Bancaud, and Anne Boigeol).

40. The concept of field is explained at length in part 2, sec. 3, above. See Boltanski 1984a and 1987 for an in-depth examination of the organizational and symbolic invention of the category of "cadres" in French society, and Charle 1990 on that of "intellectuals" proceeding along the same analytical lines.

41. For example, Sartre both dominated, and was in turn dominated by his own domination in, the French intellectual field of the 1950s (see Boschetti 1988 and Bourdieu 1980e, 1984b).

42. In English in the original.

If you accept the notion of profession as an instrument, rather than an object, of analysis, none of this creates any difficulty. As long as you take it as it presents itself, the given (the hallowed *data* of positivist sociologists) gives itself to you without difficulty. Everything goes smoothly, everything is taken for granted. Doors and mouths open wide. What group would turn down the sacralizing and naturalizing recording of the social scientist? Studies of bishops or corporate leaders that (tacitly) accept the church or business problematic will enroll the support of the Episcopate and of the Business Council, and the cardinals and corporate leaders who zealously come to comment on their results never fail to grant a certificate of objectivity to the sociologist who succeeds in giving objective, i.e., public, reality to the subjective representation they have of their own social being. In short, as long as you remain within the realm of socially constituted and socially sanctioned appearances—and this is the order to which the notion of “profession” belongs—you will have all appearances with you and for you, even the appearance of scientificity. On the contrary, as soon as you undertake to work on a genuine constructed object, everything becomes difficult: “theoretical” progress generates added “methodological” difficulties. “Methodologists,” for their part, will have no difficulty finding plenty to nit-pick about in the operations that have to be carried out in order to grasp the constructed object as best one can. (Methodology is like spelling of which we say in French: *c’est la science des ânes*, “it is the science of the jackasses.” It consists of a compendium of errors of which one can say that you must be dumb to commit most of them.) Among those difficulties, there is the question I touched upon earlier, of the boundaries of the field. The most daring of positivists solve that question—when they do not purely and simply neglect to pose it by using preexisting lists—by what they call an “operational definition” (“In this study, I shall call ‘writer’ . . .”; “I will consider as a ‘semiprofession’ . . .”), without seeing that the question of the definition (“So and so is not a true writer!”) is at stake within the object itself.⁴³ There is a struggle

43. Peter Rossi’s (1989: 11–13) strenuous effort to pass off a socially arbitrary definition of “homelessness” as grounded in “scientific” considerations is exemplary in its degree of positivist ingenueness and notable for its blindness to its own presuppositions (including that of the existence of a sort of Platonic essence of homelessness). Instead of (at minimum) showing how different definitions produce populations of different sizes, compositions and trajectories and of analyzing the political and scientific inter-

within the object over who is part of the game, who in fact deserves the title of writer. The very notion of writer, but also the notion of lawyer, doctor, or sociologist, despite all efforts at codification and homogenization through certification, is at stake in the field of writers (or lawyers, etc.): the struggle over the legitimate definition, whose stake—the word definition says it—is the boundary, the frontiers, the *right of admission*, sometimes the *numerus clausus*, is a universal property of fields.⁴⁴

ests involved in the contention opposing their various advocates, Rossi is content to assert *ex cathedra* his definition tailored to existing data and preconceptions. In his struggle to “operationalize” a notion borrowed from everyday discourse in a way that will not challenge but reinforce the latter, Rossi seeks to cumulate congruity with ordinary common sense, with scholarly common sense and with the practical constraints of bureaucratic survey research. Noting that “it is easy to get bogged down in academic exercises in definition,” he explains: “I will use a definition of homelessness that covers the *essence of that term* and is also *practical to use in actual research*. Although my ultimate conception is that homelessness is a matter of degree, I am *constrained to use the definition most common in the social science studies* of homelessness that I rely on. . . . There are some very *persuasive logistical reasons* why most studies of the homeless have adopted this definition in practice” (emphasis added). The construction—in this case, it might be more appropriate to talk of destruction—of his object follows neither the main observable articulations of the phenomenon nor a theoretically guided problematic of its causes and variations. It ends up yielding a “fairly narrow definition” which basically borrows and ratifies that of state bureaucracies whose interest in normalizing and minimizing the phenomenon is amply documented: it centers “mainly on the most accessible of the homeless, clients of agencies that provide services, such as shelters, food kitchens, and medical clinics set up to serve the homeless.” It excludes all those that the state does not want to recognize as bona fide homeless (inhabitants of hospitals, jails, prisons, nursing homes, and all the “precariously housed,” including people forced to rent or occupy rooms in the dwelling of parents or friends, etc.).

This positivist tour de force climaxes when Rossi replaces the ordinary, commonsensical category of “homelessness” by another category of the current “sociological vernacular” (Merton), that of “extreme poverty,” defined here, with the same sense of self-evidence (and the same self-assured arbitrariness), as having income below 75 percent of the “official poverty line,” another bureaucratic construct. Homelessness and poverty are thus transmogrified from a sociopolitical condition—a set of *historical relations* and *categories* resulting from struggles over the production and allocation of social wealth—to a *state* measured by neat, clear-cut atomistic variables that allow one to count, divide, and discipline individuals.

44. On recent changes in the social definition and functions of legal experts, see Dezalay 1989; on the struggle to define what a writer is in seventeenth-century France, Viala 1985; on the dilemmas of women writers to be recognized as such, de Saint Martin 1990b.

Empiricist resignation has all appearances going for it and receives all approvals because, by avoiding self-conscious construction, it leaves the crucial operations of scientific construction—the choice of the problem, the elaboration of concepts and analytical categories—to the social world *as it is*, to the established order, and thus it fulfills, if only by default, a quintessentially conservative function of ratification of the *doxa*. Among all the obstacles that stand in the way of the development of a scientific sociology, one of the most formidable is the fact that genuine scientific discoveries come at the highest *costs* and with the lowest profits, not only in the ordinary markets of social existence but also, too often, in the academic market, from which greater autonomy could be expected. As I tried to argue concerning the differential social and scientific costs and benefits of the notions of *profession* and *field*, it is often necessary, in order to produce science, to forego the appearances of scientificity, even to contradict the norms in currency and to challenge ordinary criteria of scientific rigor. Appearances are always in favor of the apparent. True science, very frequently, isn't much to look at, and, to move science forward, it is often necessary to take the risk of not displaying all the outward signs of scientificity (we often forget how easy it is to simulate them). Among other reasons because the half-wits or *demi-habiles*, as Pascal calls them, who dwell on superficial violations of the canons of elementary "methodology," are led by their positivist confidence to perceive as so many "mistakes" and as effects of incompetence or ignorance what are methodological choices founded upon a deliberate refusal to use the escape hatches of "methodology."

I need not say that the obsessive reflexivity which is the condition of a rigorous scientific practice has nothing in common with the false radicalism of the questioning of science that is now proliferating. (I am thinking here of those who introduce the age-old philosophical critique of science, more or less updated to fall in line with the reigning fashion in American social science, whose immune system has paradoxically been destroyed by several generations of positivist "methodology.") Among these critiques, one must grant a special place to those of ethnomethodologists, even though, in some of their formulations, they converge with the conclusions of those who reduce scientific discourse to rhetorical strategies about a world itself reduced to the state of a text. The analysis of the logic of practice, and of the spontaneous theories with which it arms itself in order to make

sense of the world, is not an end in itself—no more so than the critique of the presuppositions of ordinary (i.e., unreflexive) sociology, especially in its uses of statistical methods. It is an absolutely decisive moment, but only a moment, of the rupture with the presuppositions of lay and scholarly common sense. If one must objectivize the schemata of practical sense, it is not for the purpose of proving that sociology can offer only one point of view on the world among many, neither more nor less scientific than any other, but to *wrench scientific reason from the embrace of practical reason*, to prevent the latter from contaminating the former, to avoid treating as an instrument of knowledge what ought to be the object of knowledge, that is, everything that constitutes the practical sense of the social world, the presuppositions, the schemata of perception and understanding that give the lived world its structure. To take as one's object commonsense understanding and the primary experience of the social world as a nonthetic acceptance of a world which is not constituted as an *object* facing a subject is precisely the means of avoiding being "trapped" *within the object*. It is the means of submitting to scientific scrutiny *everything that makes the doxic experience of the world possible*, that is, not only the preconstructed representation of this world but also the cognitive schemata that underlie the construction of this image. And those among the ethnomethodologists who rest content with the mere description of this experience without questioning the social conditions which make it possible—that is, the correspondence between social structures and mental structures, the objective structures of the world and the cognitive structures through which the latter is apprehended—do nothing more than repeat the most traditional questionings of the most traditional philosophy on the reality of reality. To assess the limitations of this semblance of radicalism that their epistemic populism imparts to them (due to their rehabilitation of ordinary thinking), we need only observe that ethnomethodologists have never seen the *political implications* of the doxic experience of the world which, as fundamental acceptance of the established order situated outside the reach of critique, is the most secure foundation of a conservatism more radical than that which labors to establish a political orthodoxy.⁴⁵

45. See above, part 2, sec. 1, for further discussion. It is easy to understand how such conservatism can, under definite historical circumstances, turn into its opposite: as Calhoun (1979) has shown in his revisionist critique of Thompson's analysis of the

4 Double Bind and Conversion

The example I just gave with the notion of profession is but a particular instance of a more general difficulty. In point of fact, it is the whole scholarly tradition of sociology that we must constantly question and methodically distrust. Whence the sort of *double bind* in which every sociologist worthy of the name is inescapably caught: without the intellectual instruments bequeathed by her scholarly tradition, she or he is little more than an amateur, an autodidactic, self-taught, spontaneous sociologist (and certainly not the best equipped of all lay sociologists, given the evidently limited span of the social experiences of most academics); but at the same time these instruments constantly put one in danger of simply substituting for the naive doxa of lay common sense the no less naive doxa of scholarly common sense (*sens commun savant*) which parrots, in technical jargon and under the official trappings of scientific discourse, the discourse of common sense (this is what I call the "Diafoirus effect").⁴⁶

It is not easy to escape the horns of this dilemma, this alternative between the disarmed ignorance of the autodidact deprived of instruments of scientific construction and the half-science of the half-scientist who unknowingly and uncritically accepts categories of perception tied to a definite state of social relations, semi-constructed concepts more or less directly borrowed from the social world. This contradiction is never felt more strongly than in the case of ethnology where, owing to the difference in cultural traditions and to the resulting estrangement, one cannot live, as in sociology, under the illusion of immediate understanding. In this case, either you see nothing or you are left with the categories of perception and the mode of thinking (the legalism of anthropologists) received from your predecessors, who oftentimes themselves received them from another scholarly tradition (that of Roman law, for instance). All this inclines us toward a sort of *structural conservatism* leading to the reproduction of the scholarly *doxa*.⁴⁷

formation of the English working class, a doxic worldview, that is, an unquestioned and unified cultural "tradition," can, when challenged, provide the cognitive mechanism necessary for radical collective action.

46. After the name of Molière's physician, who speaks a pretentious and falsely scholarly Latin in *Le Bourgeois gentilhomme*.

47. This point is argued more fully in Bourdieu 1986a and 1986c.

Thence the peculiar antinomy of the pedagogy of research: it must transmit both tested instruments of construction of reality (problematics, concepts, techniques, methods) and a formidable critical disposition, an inclination to question ruthlessly those instruments—for instance the occupational taxonomies of INSEE or others, which are neither given as a godsend nor issued ready for use out of reality. It goes without saying that, as with every message, the chances that this pedagogy will be successful vary substantially with the socially constituted dispositions of its recipients. The most favorable situation for its transmission is with people who combine an advanced mastery of scientific culture and a certain revolt against, or distance from, that culture (most often rooted in an estranged experience of the academic universe) that pushes them not to "buy it" at its face value or, quite simply, a form of resistance to the aseptized and derealized representation of the social world offered by the socially dominant discourse in sociology. Aaron Cicourel is a good illustration of this: he had hung around with "delinquents" in the slums of Los Angeles long enough in his youth to be spontaneously inclined to question the official representation of "delinquency." It is no doubt this intimate familiarity with that universe, joined with a solid knowledge of statistics and of statistical practices, that prompted him to ask of "delinquency" statistics questions that all the methodological precepts in the world would have been incapable of generating (Cicourel 1968).

At the risk of seeming to push radical doubt to its breaking point, I would like to evoke again the most pernicious forms that lazy thinking can take in sociology. I have in mind that very paradoxical case where a critical thought such as Marx's functions in a state of unthought (*impensé*), not only in the minds of researchers (and this applies to both the advocates and the critics of Marx), but also within the reality that they record as a matter of pure observation. To conduct surveys on social classes without any further reflection on their existence or their nonexistence, on their size, and on whether they are antagonistic or not, as has often been done, especially with the aim of discrediting Marxist theory, is unknowingly to take as one's object the traces, within reality, of the effects wielded by Marx's theory, in particular via the activities of parties and unions who worked to "raise class consciousness."

What I am saying about the "theory effect" that the theory of class may have exerted, and of which "class consciousness" as we measure

it empirically is in part the product, is but a particular illustration of a more general phenomenon. Due to the existence of a social science, and of social practices that claim kinship with this science, such as opinion polls, media advising, publicity, etc.,⁴⁸ but also pedagogy and even, more and more often, the conduct of politicians or government officials, businessmen, and journalists, there are, within the social world itself, more and more agents who engage scholarly, if not scientific, knowledge in their practices and more importantly in their work of production of representations of the social world and of manipulation of these representations. So that science increasingly runs the risk of inadvertently recording the outcome of practices that claim to derive from science.

Finally, and more subtly, surrendering to habits of thought, even those that can exert a powerful effect of rupture under other circumstances, can also lead to unexpected forms of naiveté. I will not hesitate to say that Marxism, in its most common social uses, often constitutes the form *par excellence* of the *scholarly preconstructed* because it stands above all suspicion. Let us suppose that we set out to study "legal," "religious," or "professorial" ideology. The word ideology itself purports to mark a break with the representations that agents claim to give of their own practice; it signifies that we should not take their statements to the letter, that they have interests, and so on. But, in its iconoclastic violence, the word leads us to forget that the domination from which one must tear away in order to objectivize it is exercised in large part because it is misrecognized as such. Therefore it makes us forget that we need to bring back into the scientific model the fact that the objective representation of practice had to be constructed against the primary experience of practice, or, if you prefer, that the "objective truth" of this experience is inaccessible to experience itself. Marx allows us to smash open the doors of *doxa*, of the doxic adherence to primary experience. But behind this door lies a trap and the *demi-habile* who trusts scholarly common sense forgets to return to the primary experience that scholarly construction had to bracket and to set aside. "Ideology" (really, by now, we would be better off calling it something else) does not appear as such, to us and to itself, and it is this misrecognition that gives it its symbolic efficacy.

48. See Champagne 1988 and 1990, on the uses of social science and pseudo-social science in the "new political space" of France.

In sum, it does not suffice to break with ordinary common sense, or with scholarly common sense in its ordinary form. *We must also break with the instruments of rupture which negate the very experience against which they have been constructed.* This must be done to build more complete models, models which encompass both the primary naiveté and the objective truth that this naiveté conceals and at which the *demi-habiles*, those who think they are smarter than everybody else, stop by falling for another form of naiveté. (I cannot refrain from saying here that the thrill of feeling smart, demystifying and demystified, of playing the role of the disenchanted disenchanter, is a crucial ingredient in a good number of sociological vocations . . . And the sacrifice that rigorous method demands is all the more costly for that.)

There is no risk of overestimating difficulty and dangers when it comes to thinking the social world. The force of the preconstructed resides in the fact that, being inscribed both in things and in minds, it presents itself under the cloak of the self-evident which goes unnoticed because it is by definition taken for granted. Rupture in fact demands a *conversion of one's gaze* and one can say of the teaching of sociology that it must first "give new eyes," as initiatory philosophers sometimes phrased it. The task is to produce, if not a "new person," then at least a "new gaze," a *sociological eye*. And this cannot be done without a genuine conversion, a *metanoia*, a mental revolution, a transformation of one's whole vision of the social world.

What is called "epistemological rupture,"⁴⁹ that is, the bracketing of ordinary preconstructions and of the principles ordinarily at work in the elaboration of these constructions, often presupposes a rupture with modes of thinking, concepts, and methods that have every appearance of *common sense*, of ordinary sense, and of good scientific sense (everything that the dominant positivist tradition honors and hallows) going for them. You will certainly understand that, when one is convinced, as I am, that the most vital task of social science and thus of the teaching of research in the social sciences is to establish as a fundamental norm of scientific practice the conversion of thought,

49. The notion of "epistemological rupture" (like that of "epistemological profile"), which many Anglo-American readers associate with Althusser (or with Foucault), originates with Gaston Bachelard and was used quite extensively by Bourdieu well before the heyday of structuralist Marxism (note the pivotal status it is given in Bourdieu, Chamboredon, and Passeron 1973, originally published in 1968).

the revolution of the gaze, the rupture with the preconstructed and with everything that buttresses it in the social order—and in the scientific order—one is doomed to be forever suspected of wielding a prophetic *magisterium* and of demanding personal conversion.

Being acutely aware of the specifically social contradictions of the scientific enterprise as I have tried to describe it, when I consider a piece of research submitted for my judgment, I am often compelled to ask myself whether I should try to impose the critical vision which seems to me to be the necessary condition of the construction of a genuine scientific object by launching into a critique of the preconstructed object that is always likely to appear as a *coup de force*, as a kind of intellectual *Anschluss*. This difficulty is all the more serious because in the social sciences the principle of mistakes is almost always rooted, at least in my experience, in socially constituted dispositions as well as in social fears and social fantasies. So that it is often difficult to state publicly a critical judgment which, beyond scientific practices, touches on the deepest dispositions of habitus, those intimately linked to social and ethnic origins, gender, and also to the degree of prior academic consecration. I have in mind here the exaggerated humility of some researchers (more frequent among women than among men, or among people of “modest” social background, as we sometimes say) which is no less fatal than arrogance. In my view, the right posture consists of a highly unlikely combination of definite ambition, which leads one to take a broad view (*à voir grand*), and the great modesty indispensable in burying oneself in the fullest detail of the object. Thus the research director who truly wants to fulfill his function would sometimes have to take up the role of the confessor or guru (in French, we say “director of consciousness”), a role that is quite dangerous and has no justification, by bringing back to reality the one who “sees too big” and by instilling more ambition in those who let themselves be trapped in the security of humble and easy undertakings.

In fact, the most decisive help that the novice researcher can expect from experience is that which encourages him or her to take into account, in the definition of her project, the real conditions of its realization, that is, the means she has at her disposal (especially in terms of time and of specific competence, given the nature of her social experiences and her training) and the possibilities of access to informants and to information, documents and sources, etc. Oftentimes, it

is only at the conclusion of a protracted work of socioanalysis, through a whole sequence of phases of overinvestments and divestments, that the ideal match between a researcher and “her” object can be made.

The sociology of sociology, when it takes the very concrete form of the sociology of the sociologist, of his scientific project, of his ambitions and his failures, of his audacities and his fears, is not a *supplément d'âme* or a kind of narcissistic luxury: the bringing to awareness (*prise de conscience*) of the dispositions, favorable or unfavorable, associated with your social origins, academic background, and gender offers you a chance, if a limited one, to get a grip on those dispositions. Yet the ruses of social pulsions are countless, and to do a sociology of one's own universe can sometimes be yet another, most perverse, way of satisfying such repressed impulses in a subtly roundabout way. For instance, a former theologian turned sociologist who undertakes to study theologians may undergo a sort of regression and start talking like a theologian or, still worse, use sociology as a weapon to settle his past theologian's accounts. The same may be true of an ex-philosopher: she will also risk finding in the sociology of philosophy a covert way of waging philosophical wars by other means.

5 Participant Objectivation

What I have called *participant objectivation* (and which is not to be mistaken for participant observation)⁵⁰ is no doubt the most difficult exercise of all because it requires a break with the deepest and most unconscious adherences and adhesions, those that quite often give the object its very “interest” for those who study it—i.e., everything about their relation to the object they try to know that they least want to know. It is the most difficult but also the most necessary exercise because, as I tried to do in *Homo Academicus* (Bourdieu 1988a), the work of objectivation in this case touches on a very peculiar object within which some of the most powerful social determinants of the very principles of apprehension of any possible object are implicitly inscribed: on the one hand, the specific interests associated with

50. On this notion, see *The Logic of Practice* (Bourdieu 1990a), *Homo Academicus* (Bourdieu 1988a), Bourdieu 1978a, and part 2, sec. 1, above.

being a member of the academic field and with occupying a specific position in that field; on the other hand, the socially constituted categories of perception of the academic world and of the social world, those categories of professorial understanding which, as I said earlier, can furnish the underpinnings of an aesthetics (e.g., the *art pompier*, academic art) or of an epistemology (as with the epistemology of resentment which, by making a virtue out of necessity, always values the petty prudences of positivist rigor as against all forms of scientific audacity).

Without trying to explicate here all the teachings that a reflexive sociology can draw from such an analysis, I would like to suggest only one of the best concealed presuppositions of the scientific enterprise that work on such an object forced me to uncover and whose immediate consequence—proof that the sociology of sociology is a necessity, not a luxury—is a better knowledge of the object itself. In a first phase of my work, I had built a model of the academic space as a space of positions linked by specific relations of force, as a field of forces and a field of struggles to preserve or transform this field of forces. I could have stopped there but observations I had made in the past, in the course of my ethnographic work in Algeria, had sensitized me to the “epistemocentrism” associated with the scholarly viewpoint. Moreover, I was forced to look back upon my enterprise by the uneasiness that filled me, upon publication, by the feeling I had of having committed a kind of disloyalty by setting myself up as observer of a game I was still playing. I thus experienced in a particularly acute manner what was implicated in the claim to adopt the stance of the impartial observer, at once ubiquitous and invisible because dissimulated behind the absolute impersonality of research procedures, and thus capable of taking up a quasi-divine viewpoint on colleagues who are also competitors. By objectivizing the pretension to the regal position that turns sociology into a weapon *in* the struggles internal to the field instead of an instrument of knowledge of these struggles, and thus of the knowing subject himself who, no matter what he does, never ceases to wage them, I gave myself the means of reintroducing into the analysis the consciousness of the presuppositions and prejudices associated with the local and localized point of view of someone who constructs the space of points of view.

Awareness of the limits of objectivist objectivation made me discover that there exists, within the social world, and particularly

within the academic world, a whole nexus of institutions whose effect is to render acceptable the gap between the objective truth of the world and the lived truth of what we are and what we do in it—everything that objectivized subjects bring up when they oppose objectivist analysis with the idea that “things are not that way.” In this case, there exists for instance collective systems of defense which, in universes where everyone struggles for the monopoly over a market in which all of one’s customers are also one’s competitors and where life is therefore very hard,⁵¹ enable us to accept ourselves by accepting the subterfuges or compensatory gratifications offered by the milieu. It is this *double truth*, objective and subjective, which constitutes the whole truth of the social world.

Although I hesitate a bit to do it, I would like to evoke as a final illustration a presentation made here some time ago on a post-election television debate,⁵² an object which, due to its apparent easiness (everything about it is immediately given to immediate intuition), presents many of the difficulties that a sociologist can encounter. How are we to move beyond intelligent description, of the kind always exposed to “being redundant with the world” (*faire pléonasme avec le monde*), as Mallarmé used to say? The danger is great, indeed, to restate in a different language what agents involved have already said or done and to bring out meanings of the first degree (there is a dramatization of the wait for the results, there is a struggle between the participants over the meaning of the result, etc.), or simply (or pompously) to identify meanings that are the product of conscious intentions and which agents themselves could state, if they had the time, and if they did not fear giving the show away. For the latter know very well—at least in practice and, more and more often today, in a conscious mode—that, in a situation whose stake is to impose the most favorable representation of one’s own position, public admis-

51. This is what Bourdieu (1985d) calls the “market of restricted production,” in opposition to the “generalized market” in which cultural producers submit their works to the public at large.

52. On the night of each national election, the main television channels in France organize special programs where prominent politicians, political scientists, journalists, and political commentators interpret and debate the estimated results of the vote and their significance for the political evolution of the country. Such programs are nearly universally identifiable by French television spectators and constitute an increasingly influential means of political action.

sion of failure, as an act of re-cognition, is *de facto* impossible. They also know that, to speak properly, figures and their meanings are no universal “facts,” and that the strategy which consists in “denying the obvious” (54 percent is greater than 46 percent), although apparently doomed to fail, retains a degree of validity (party X won but party Y didn’t really lose: X won but not as cleanly as in previous elections or by a smaller margin than predicted, etc.).

But is this what really matters? The problem of the break is raised with a special salience here because the analyst is contained within the object of his or her competitors in the interpretation of the object, and these competitors may also call upon the authority of science. It is raised in a particularly acute form because, in contradistinction to what happens in other sciences, a mere description, even a constructed description—i.e., one bent on capturing the relevant traits and only those—does not have the intrinsic value that it assumes in the case of the description of a secret ritual ceremony among the Hopis or of the coronation of a king in the Middle Ages: the scene has been seen and *understood* (at a certain level and up to a certain point) by twenty million television spectators and the recording gives a read-out of it that no positivist transcription could match.

In fact, we cannot escape the indefinite series of mutually refutable interpretations—the hermeneuticist is involved in a struggle among hermeneuticists who compete to have the last word about a phenomenon or an outcome—unless we actually construct the space of objective relations (*structure*) of which the communicative exchanges we directly observe (*interaction*) are but the expression. The task consists in grasping a hidden reality which veils itself by unveiling itself, which offers itself to observers only in the anecdotal form of the interaction that conceals it. What does this all mean? Under our eyes we have a set of individuals, designated by surnames, Mr. Amar, journalist, Mr. Rémond, historian, Mr. X, political scientist, and so on, who exchange, as we say, utterances that apparently are liable to a “discourse analysis” and where all visible “interactions” apparently provide all the necessary tools for their own analysis. But in fact the scene that unravels on the television set, the strategies that agents deploy to win the symbolic struggle over the monopoly of the imposition of the verdict, for the recognized ability to tell the truth about the stake of the debate, are the expression of objective relations of force between the agents involved or, to be more precise, between the dif-

ferent fields in which they are implicated and in which they occupy positions of various standing. In other words, *the interaction is the visible and purely phenomenal resultant of the intersection of hierarchized fields.*

The space of interaction functions as a situation of linguistic market and we can uncover the principles that underlie its conjunctural properties.⁵³ First, it consists of a preconstructed space: the social composition of the group of participants is determined in advance. To understand what can be said and especially *what cannot be said* on the set, one must know the laws of formation of the group of speakers—who is excluded and who exclude themselves. The most radical censorship is absence. We must thus consider the ratios of representation (in both the statistical and the social sense) of the various categories (gender, age, occupation, education, etc.), and thus the chances of access to speech, measured by how much time is used up by each. A second characteristic is the following: the journalist wields a form of domination (conjunctural, not structural) over a space of play that he has constructed and in which he finds himself in the role of referee imposing norms of “objectivity” and “neutrality.”

We cannot, however, stop here. The space of interaction is the locus where the intersection between several different fields is realized. In their struggle to impose the “impartial” interpretation, that is, to make the viewers recognize their vision as objective, agents have at their command resources that depend on their membership in objectively hierarchized fields and on their position within their respective fields. First we have the political field (Bourdieu 1981a): because they are directly implicated in the game and thus directly interested and seen as such, politicians are immediately perceived as judges and judged (*juges et parties*) and therefore are always suspected of putting forth interested, biased, and hence discredited interpretations. They occupy different positions in the political field: they are situated in this space by their membership in a party but also by their status in the party, their notoriety, local or national, their public appeal, etc. Then we have the journalistic field: journalists can and must adopt a rhetoric of objectivity and neutrality, with the assistance of “politologists” when needed. Then we have the field of “political

53. The concept of linguistic market is explicated in Bourdieu 1990f and part 2, sec. 5, above.

science" within which "media politologists" occupy a rather unglamorous position, even if they enjoy considerable prestige on the outside, especially among journalists whom they structurally dominate. Next is the field of political marketing, represented by advertisers and media advisors who dress up their evaluations of politicians with "scientific" justifications. Last is the university field proper, represented by specialists in electoral history who have developed a specialty in the commentary of electoral results. We thus have a progression from the most "engaged" to the most detached, structurally or statutorily: the academic is the one who has the most "hindsight," "detachment." When it comes to producing a *rhetoric of objectivity* which is as efficacious as possible, as is the case in such post-election news programs, the academic enjoys a structural advantage over all the others.

The discursive strategies of the various agents, and in particular rhetorical effects aimed at producing a front of objectivity, will depend on the balance of symbolic forces between the fields and on the specific resources that membership in these fields grants to the various participants. In other words, they will hinge upon the specific interests and the differential assets that the participants possess, in this particular symbolic struggle over the "neutral" verdict, by virtue of their position in the system of invisible relations that obtain between the different fields in which they operate. For instance, the politologist will have an edge, as such, over the politician and the journalist, due to the fact that he is more readily credited with objectivity, and because he has the option of calling upon his specific competence, i.e., his command of electoral history to make comparisons. He can ally himself with the journalist, whose claims to objectivity he will thereby reinforce and legitimize. The resultant of all these objective relations are relations of symbolic power which express themselves in the interaction in the form of rhetorical strategies. It is these objective relations that determine for the most part who can cut somebody off, ask questions, speak at length without being interrupted, or disregard interruptions, etc., who is condemned to strategies of denegation (of interests and interested strategies) or to ritual refusals to answer, or to stereotypical formulas, etc. We would need to push further by showing how bringing objective structures into the analysis allows us to account for the particulars of discourse and of rhetorical strategies, complicities, and antagonisms, and for the moves at-

tempted and effected—in short, for everything that discourse analysis believes it can understand on the basis of discourse alone.

But why is the analysis especially difficult in this case? No doubt this is because those whom the sociologist claims to objectivize are competitors for the monopoly over objective objectivation. In fact, depending on what object she studies, the sociologist herself is more or less distant from the agents and the stakes she observes, more or less directly involved in rivalries with them, and consequently more or less tempted to enter the game of metadiscourse under the cloak of objectivity. When the game under analysis consists, as is the case here, in delivering a metadiscourse about all other discourses—those of the politician who cheerfully proclaims electoral victory, of the journalist who claims to provide an objective report on the spread between the candidates, of the "politologist" and the specialist in electoral history who claim to offer us an objective explanation of the result by drawing on comparison of the margins and trends with past or present statistics—where it consists, in a word, in placing oneself *meta*, above the game, through the sole force of discourse, it is tempting to use the science of the strategies that the different agents develop to assure victory to their "truth" in order to tell the truth of the game, and thus to secure victory in the game for yourself. It is still the objective relation between political sociology and "media-oriented politology" or, to be more precise, between the positions that the observers and the observed occupy in their respective, objectively hierarchized, fields that determines the perception of the observer, especially by imposing on him blind spots indicative of his own vested interests.

Objectivation of the relation of the sociologist to his or her object is, as we can clearly see in this case, the necessary condition of the break with the propensity to invest in her object which is no doubt at the root of her "interest" in the object. One must in a sense renounce the use of science to intervene in the object in order to be in a position to carry out an objectivation which is not merely the partial and reductionist view that one can acquire, from within the game, of the other player(s), but rather the all-encompassing view that one acquires of a game that can be grasped as such because one has retired from it. Only the sociology of sociology—and of the sociologist—can give us a definite mastery of the social aims that can be pursued via the scientific goals we immediately seek. Participant objectivation, ar-

guably the highest form of the sociological art, is realizable only to the extent that it is predicated on as complete as possible an objectivation of the interest to objectivize inscribed in the fact of participating, as well as on a bracketing of this interest and of the representations it sustains.