In Memory of my Father

In Other Words

Essays Towards a Reflexive Sociology

Pierre Bourdieu

Translated by Matthew Adamson

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Contents

Preface

The spirit of the castle is in its drawbridge.

René Char

I have spoken often enough about the particular difficulties of sociological writing, and perhaps the texts the reader will find here refer to them only too frequently. But they justify, I believe, the publication of these transcriptions – relieved of the most flagrant repetitions and clumsy turns of phrase – of talks, interviews and papers. Written discourse is a strange product, which is created in a pure confrontation between the writer and 'what he or she has to say', outside any direct experience of a social relation, and outside the constraints and temptations of an immediately perceptible demand, which takes the form of a variety of signs of resistance or approval. I do not need to mention the irreplaceable advantages of being thus closed in on oneself: it is clear that, among other effects, this closure founds the autonomy of a text from which the author has as far as possible withdrawn, merely removing the rhetorical effects meant to display his intervention and involvement in his discourse (even if this goes no further than the use of the first person), as if to leave the reader's liberty intact.

But the presence of a listener, and especially an audience, has effects which are not all negative, especially when you have to convey an analysis and an experience at the same time, and to overcome obstacles to communication which very often have to do less with problems of understanding than with a disposition of the will: if the urgency and the linear nature of spoken discourse entail simplifications and repetitions (encouraged in addition by the fact that the same questions tend to recur), the facility of the spoken word, which enables you to go very quickly from one point to another, cutting the corners that a rigorous argument must negotiate one by one, means you can make compressions, abbreviations and

comparisons which convey an idea of the complex totalities that writing unfolds and develops in the interminable succession of paragraphs and chapters. The concern to communicate feelings or ideas that is imposed by the direct presence of attentive interlocutors prompts you to go from abstraction to example and back again, and encourages you to look for metaphors or analogies which, when you can point out their limits at the very moment you use them, enable you to give people a first approximate insight into the most complex models and thus to introduce your listeners to a more rigorous presentation. But above all, the juxtaposition of remarks that are very varied in circumstance and topic may, by demonstrating how the same theme is treated in different contexts, or the same model applied to different domains, show in action a mode of thought that the finished nature of the written work can convey only imperfectly, when it does not conceal it completely.

The logic of the interview which, in more cases than one, becomes a genuine dialogue, has the effect of removing one of the main forms of censorship which the fact of belonging to a scientific field can impose, one that may be so deeply internalized that its presence is not even suspected: that which prevents you from answering, in writing itself, questions which, from the professional's point of view, can only appear trivial or unacceptable. Furthermore, when a well-intentioned interlocutor puts forward, in all good faith, his reservations or resistances, or when he acts as the devil's advocate by voicing objections or criticisms he has read or heard, he can give you an opportunity either to state quite fundamental propositions that the elliptical style of academic dignity or the proprieties of scientific etiquette lead you to conceal, or to give explanations, denials or refutations that the disdain or the disgust aroused by the self-destructive over-simplifications of incomprehension and incompetence or by the stupid or base accusations of bad faith tempt you to reject. (I will not here indulge in the – somewhat narcissistic – cruelty of presenting an anthology of the criticisms made of me, in the form of political slogans and denunciations – determinism, totalitarianism, pessimism, etc. – and which shock me above all by their hypocrisy: it is so easy and so profitable to pose as the defender of fine feelings and good causes – art, freedom, virtue, disinterestedness – against someone who can be accused with impunity of hating them because he unveils, without even appearing to deplore the fact, all that it is a point of honour for the believer to conceal.) The fact of being questioned, which creates a certain demand, authorizes and encourages you to explain your theoretical intentions, and all the ways in

which they differ from other competing views, and to set out in greater detail the empirical operations, and the difficulties (often undetectable in the final record) they have had to overcome – all the information, in other words, which the perhaps exaggerated refusal to be indulgent and to spell things out as simply as possible often leads you to censor.

But the major advantage of an oral exchange is linked above all to the very content of the sociological message and to the resistances that it arouses. Many of the remarks presented here assume their full significance only if you refer to the circumstances in which they were pronounced and the audience to which they were addressed. Part of their effectiveness probably results from the effort of persuasion aimed at overcoming the extraordinary tension sometimes created by the clarification of a rejected or repressed truth. Gershom Scholem said to me one day: I don't talk about Jewish problems in the same way when I am talking to Jews in New York, Jews in Paris and Jews in Jerusalem. Likewise, the reply I can give to the questions I am most frequently asked varies with the interlocutors – sociologists or non-sociologists, French sociologists or foreign sociologists, specialists from other fields or ordinary laymen and women, and so on. This does not mean that there is not one true answer to each of these questions and that this truth does not always need to be stated. But when, like me, you feel that you owe it to yourself to concentrate in each case on the point where you expect the maximum resistance, which is the exact opposite of having any demagogic intentions, and to tell each audience, without being provocative but also without making any concessions, the aspect of the truth which it will find most difficult to accept, in other words what you think its truth to be, making use of the knowledge you think you have of its expectations so as not to flatter and manipulate it, but to 'get across', as they say, what it will find most difficult to accept or to swallow – in other words what disturbs its most trusted investments – you know that you always run the risk of seeing socio-analysis turn into a socio-drama.

The uncertainties and imprecisions of this deliberately foolhardy discourse thus have their counterpart in the quavering of the voice which is the mark of risks shared in any honest exchange of ideas and which, if it can still be heard, however faintly, through its written transcription, seems to me to justify its publication.

Part I

Pathways

1

'Fieldwork in philosophy'

Q. What was the intellectual situation like when you were a student
– Marxism, phenomenology and so on?

A. When I was a student in the fifties, phenomenology, in its
existentialist variety, was at its peak, and I had read *Being and
Nothingness*[1] very early on, and then Merleau-Ponty and Husserl;
Marxism didn't really exist as an intellectual position, even if people
like Tran-Duc-Thao managed to give it a certain profile by raising
the question of its relation with phenomenology. However, I did
read Marx at that time for academic reasons; I was especially
interested in the young Marx, and I had been fascinated by the
'Theses on Feuerbach'. But this was the period of Stalinist ascend-
ancy. Many of my fellow students who these days have become
violently anti-communist were then in the Communist Party. The
pressure exerted by Stalinism was so exasperating that, around 1951,
we had founded at the École normale (with Bianco, Comte, Marin,
Derrida, Pariente and others) a Committee for the Defence of
Freedom, which Le Roy Ladurie denounced in the communist cell at
the École . . .
  Philosophy as taught in the University was not very inspiring –
even if there were some very competent people, like Henri Gouhier,
under whose supervision I wrote a dissertation (a translation and
commentary of the *Animadversiones* of Leibniz), Gaston Bachelard
and Georges Canguilhem. Outside the Sorbonne, and especially at
the École des hautes études and the Collège de France, there were
Éric Weil, Alexandre Koyré, and Martial Guéroult, whose classes I

1 J.-P. Sartre, *Being and Nothingness*, tr. H. E. Barnes (New York, 1956).

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followed once I was at the École normale. All these people were outside the usual syllabus, but it's pretty much thanks to them and to what they represented – a tradition of the history of the sciences and of rigorous philosophy (and thanks also to my reading of Husserl, who was still little translated in those days) – that I tried, together with those people who, like me, were a little tired of existentialism, to go beyond merely reading the classical authors and to give some meaning to philosophy. I studied mathematics and the history of the sciences. Men like Georges Canguilhem, and also Jules Vuillemin, were for me, and for a few others, real 'exemplary prophets' in Weber's sense. In the phenomenologico-existentialist period, when they weren't very well known, they seemed to point to the possibility of a new path, a new way of fulfilling the philosopher's role, quite different from just vaguely holding forth about the big problems. There was also the review *Critique*, then in the middle of its best years, with Alexandre Koyré, Éric Weil and others writing for it; in it, you could come across both wide-ranging and precise information on work being done in France and, especially, abroad. I was, doubtless for sociological reasons, less attracted than other people (for instance, Foucault) to the Bataille–Blanchot side of *Critique*. The desire for a clean break, rather than for some 'transgression', was in my case directed against institutional power, and especially against the institution of the university and all the violence, imposture and sanctified stupidity that it concealed – and, behind that institution, against the social order. This may have been because I didn't have any accounts to settle with the bourgeois family, as did others, and so I was less inclined to the symbolic breaks dealt with in *The Inheritors*.[2] But I think that the concern to *nicht mitmachen*, as Adorno put it – the refusal to compromise with institutions, beginning with intellectual institutions – has never left me.

Many of the intellectual leanings that I share with the 'structuralist' generation (especially Althusser and Foucault) – which I do not consider myself to be part of, firstly because I am separated from them by an academic generation (I went to their lectures) and also because I rejected what seemed to me to be a fad – can be explained by the need to react against what existentialism had represented for them: the flabby 'humanism' that was in the air, the complacent

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2 P. Bourdieu and J. C. Passeron, *Les héritiers, les étudiants et la culture* (Paris, 1964); trans. as *The Inheritors: French Students and their Relation to Culture*, tr. R. Nice (Chicago, 1979).

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appeal to 'lived experience' and that sort of political moralism that lives on today in *Esprit*.[3]

Q. Were you never interested in existentialism?

A. I read Heidegger, I read him a lot and with a certain fascination, especially the analyses in *Sein und Zeit* of public time, history and so on, which, together with Husserl's analyses in *Ideen II*,[4] helped me a great deal – as was later the case with Schütz – in my efforts to analyse the ordinary experience of the social. But I never really got into the existentialist mood. Merleau-Ponty was something different, at least in my view. He was interested in the human sciences and in biology, and he gave you an idea of what thinking about immediate present-day concerns can be like when it doesn't fall into the sectarian over-simplifications of political discussion – in for instance his writings on history, on the Communist Party, on the Moscow Trials. He seemed to represent one potential way out of the philosophical babble found in academic institutions . . .

Q. But at that time, wasn't philosophy dominated by a sociologist?[5]

A. No – that was just the effect of institutional authority. And our contempt for sociology was intensified by the fact that a sociologist could be president of the board of examiners of the competitive 'agrégation' exam in philosophy and force us to attend his lectures – which we thought were lousy – on Plato or Rousseau. This contempt for the social sciences lasted among philosophy students at the École normale – who represented the 'elite', and therefore the dominant model – at least until the sixties. At that time, the only sociology was mediocre and empirical, without any theoretical or indeed empirical inspiration behind it. And this conviction on the part of philosophers from the École normale was reinforced by the fact that the sociologists of the twenties and thirties, Jean Stoetzel or even Georges Friedmann, who had written a rather poor book on Leibniz and Spinoza, struck them as being the products of a negative vocation. This was even more pronounced for the first sociologists of the

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3 *Esprit*: political and literary review (broadly Christian and left-wing) founded in the 1930s; became a forum for Resistance writing in the Second World War.

4 E. Husserl, *Ideas: General Introduction to Pure Phenomenology*, tr. W. R. B. Gibson (London, 1931).

5 Georges Davy, the last survivor of the Durkheimian school.

post-war years who, with a few exceptions, had not followed the royal road – École normale and agrégation – and who, in the opinion of some people, had even had to fall back on sociology because of their failure in philosophy.

Q. But how did the change that happened in the sixties come about?

A. Structuralism was very important. For the first time, a social science imposed itself as a respectable, indeed dominant discipline. Lévi-Strauss, who baptized his science anthropology, instead of ethnology, thus bringing together the Anglo-Saxon meaning and the old German philosophical meaning – at about the same time Foucault was translating Kant's *Anthropologie*[6] – ennobled the human science that was thus established, by drawing on Saussure and linguistics, and turned it into a royal science, to which even philosophers were obliged to pay heed. That was when the full force of what I call the '-ology effect' – in allusion to all those nouns that use that suffix, archaeology, grammatology, semiology, etc. – was felt; it was a clear expression of the effort philosophers were making to break down the frontier between science and philosophy. I never had much liking for these half-hearted changes of label which enable one to draw freely on the profits of scientificity and the profits associated with the status of philosopher. I think that just at that time what was necessary was to question the status of philosopher and all its prestige so as to carry out a true conversion into science. And, speaking for myself, although I made an attempt in my work to put into operation the structural or relational way of thinking in sociology, I resisted with all my might the merely fashionable forms of structuralism. And I was even less inclined to show any indulgence for the mechanical transference of Saussure or Jakobson into anthropology or semiology·that was common practice in the sixties, since my philosophical work had brought me very early on to read Saussure closely: in 1958–9 I lectured on Durkheim and Saussure, trying to establish the limits of attempts to produce 'pure theories'.

Q. But you became an ethnologist to begin with?

A. I had undertaken research into the 'phenomenology of

emotional life', or more exactly into the temporal structures of emotional experience. To reconcile my need for rigour with philosophical research, I wanted to study biology and so on. I thought of myself as a philosopher and it took me a very long time to admit to myself that I had become an ethnologist. The new prestige that Lévi-Strauss had given that science probably helped me greatly. . . . I undertook both research that could be called ethnological – on kinship, ritual and the pre-capitalist economy – and research that could be described as sociological, especially statistical surveys that I carried out with my friends from the INSEE,[7] Darbel, Rivet and Seibel, from whom I learned a great deal. For instance, I wanted to establish the principle (one that had never been clearly determined in the theoretical tradition) behind the difference between proletariat and sub-proletariat; and, by analysing the economic and social conditions of the appearance of economic calculation, in the field of economics but also that of fertility and so on, I tried to show that the principle behind this difference can be traced to the domain of the economic conditions enabling the emergence of types of rational *forecasting*, of which revolutionary aspirations are one dimension.

Q. But this theoretical project was inseparable from a methodology . . .

A. Yes. I re-read, of course, all of Marx's works – and many others – on the question (this was probably the period when I read Marx most, and even Lenin's survey of Russia). I was also working on the Marxist notion of relative autonomy in relation to the research that I was starting to carry out into art (a short book, *Proudhon, Marx, Picasso*,[8] written in French between the wars by a German émigré called Max Raphael, had been of great use to me). All of this was before the triumphant return of structuralist Marxism. But above all I wanted to get away from speculation – at that time, the works of Frantz Fanon, especially *The Wretched of the Earth*,[9] were the latest fashion, and they struck me as being both false and dangerous.

6 I. Kant, *Anthropologie du point de vue pragmatique*, tr. M. Foucault (Paris, 1964).

7 The National Institute of Statistics and Economic Studies.
8 M. Raphael, *Proudhon, Marx, Picasso: Three Studies in the Sociology of Art*, tr. I. Marcuse (London, 1980).
9 F. Fanon, *The Wretched of the Earth*, tr. C. Farrington (Harmondsworth, 1967).

q. At the same time you were engaged in anthropological research.

A. Yes. And the two were closely linked. This was because I also wanted to understand, through my analyses of temporal conscious-ness, the conditions of the acquisition of the 'capitalist' economic habitus among people brought up in a pre-capitalist world. And there too, I wanted to do it by observation and measurement, and not by second-hand thinking based on second-hand material. I also wanted to resolve purely anthropological problems, especially those that the structuralist approach raised for my work. I have related, in the introduction to my *The Logic of Practice*,[10] how I was stupefied to discover, by the use of statistics – something that was very rarely done in ethnology – that the type of marriage considered to be typical in Arabo-Berber societies, namely marriage with the parallel girl cousin, accounted for about 3 to 4 per cent of cases, and 5 to 6 per cent in Marabout families, that are stricter and more orthodox. This forced me to think about the notion of kinship, rule, and rules of kinship, which led me to the antipodes of the structuralist tradition. And the same thing happened to me with ritual: although it was coherent and, up to a certain point, logical, the system of the oppositions constitutive of ritual logic turned out to be incapable of integrating all the data gathered. But it was a very long time before I really broke with some of the fundamental presuppositions of structuralism (which I made use of simultaneously in sociology when I imagined the social world as a space of objective relations that transcends the agents and is irreducible to interactions between individuals). I first had to discover, by returning to observe a more familiar terrain, on the one hand the society of Béarn, where I come from, and on the other hand the academic world, and the objectivist presuppositions – such as the privilege of the observer with respect to the native, who is bound to remain ignorant of his situation – that are part and parcel of the structuralist approach. And then it was, I think, necessary for me to leave ethnology as a social world, by becoming a sociologist, so that the raising of certain unthinkable questions could become possible. I'm not telling my life story here: I am trying to make a contribution to the sociology of science. Belonging to a professional group brings into play an effect of censorship which goes far beyond institutional or personal con-straints: there are questions that you don't ask, and that you can't

10 P. Bourdieu, *Le sens pratique* (Paris, 1980); trans. as *The Logic of Practice*, tr. R. Nice (Cambridge, 1989).

ask, because they have to do with the fundamental beliefs that are at the root of science, and of the way things function in the scientific domain. This is what Wittgenstein says when he points out that radical doubt is so deeply identified with the philosophical stance that a well-trained philosopher does not so much as dream of casting doubt on this doubt.

q. You often quote Wittgenstein – why is that?

A. Wittgenstein is probably the philosopher who has helped me most at moments of difficulty. He's a kind of saviour for times of great intellectual distress – as when you have to question such evident things as 'obeying a rule'. Or when you have to describe such simple (and, by the same token, practically ineffable) things as putting a practice into practice.

q. What was the principle behind your doubt about structuralism?

A. I wanted, so to speak, to reintroduce agents that Lévi-Strauss and the structuralists, among others Althusser, tended to abolish, making them into simple epiphenomena of structure. And I mean agents, not subjects. Action is not the mere carrying out of a rule, or obedience to a rule. Social agents, in archaic societies as well as in ours, are not automata regulated like clocks, in accordance with laws which they do not understand. In the most complex games, matri-monial exchange for instance, or ritual practices, they put into action the incorporated principles of a generative habitus: this system of dispositions can be imagined by analogy with Chomsky's generative grammar – with this difference: I am talking about dispositions *acquired through experience*, thus variable from place to place and time to time. This 'feel for the game', as we call it, is what enables an infinite number of 'moves' to be made, adapted to the infinite number of possible situations which no rule, however complex, can foresee. And so, I replaced the rules of kinship with matrimonial strategies. Where everyone used to talk of 'rules', 'model' or 'structure', somewhat indiscriminately, and putting themselves in the objectivist position, that of God the Father watching the social actors like puppets controlled by the strings of structure, everyone now-adays talks of matrimonial strategies (which means they put them-selves in the place of the agents, without however making them into rational calculators). This word, strategies, evidently has to be stripped of its naively teleological connotations: types of behaviour

can be directed towards certain ends without being consciously directed to these ends, or determined by them. The notion of *habitus* was invented, if I may say so, in order to account for this paradox. Likewise, the fact that ritual practices are the product of a 'practical sense', and not of a sort of unconscious calculation or of obedience to a rule, explains that the rites are coherent, but that their coherence is the partial and never total coherence that we associate with practical constructions.

Q. Didn't this breaking away from the structuralist paradigm risk throwing you back on the 'individualist' paradigm of rational calculation?

A. In retrospect – although in fact things never happen this way in the context of real research – the use of the notion of *habitus*, an old Aristotelian and Thomist concept that I completely rethought, can be understood as a way of escaping from the choice between a structuralism without subject and the philosophy of the subject. There too, certain phenomenologists, including Husserl himself who gives a role to the notion of habitus in the analysis of antepredicative experience, or Merleau-Ponty, and also Heidegger, opened the way for a non-intellectualist, non-mechanistic analysis of the relations between agent and world. Unfortunately, people apply to my analyses – and this is the principal source of misunderstanding – the very alternatives that the notion of habitus is meant to exclude, those of consciousness and the unconscious, of explanation by determining causes or by final causes. Thus Lévi-Strauss sees in the theory of matrimonial strategies a form of spontaneism and a return to the philosophy of the subject. Others, on the contrary, will see in it the extreme form of what they reject in the sociological way of thinking: determinism and the abolition of the subject. But it's probably Jon Elster who presents us with the most perverse example of incomprehension. Instead of claiming, as does everyone else, that I advocate one of the terms of the alternative so that he can emphasize the importance of the other, he charges me with a sort of oscillation between the one and the other and he can thus accuse me of contradiction or, more subtly, of piling up mutually exclusive explanations. His position is all the more astonishing in that, probably as a result of the polemical situation, he has been led to take into account what is at the very basis of my representation of action, the way in which dispositions are adjusted in accordance with one's position, and expectations in accordance with opportunities:

the *sour grapes* factor. Since the habitus, the virtue made of necessity, is a product of the incorporation of objective necessity, it produces strategies which, even if they are not produced by consciously aiming at explicitly formulated goals on the basis of an adequate knowledge of objective conditions, nor by the mechanical determination exercised by causes, turn out to be objectively adjusted to the situation. Action guided by a 'feel for the game' has all the appearances of the rational action that an impartial observer, endowed with all the necessary information and capable of mastering it rationally, would deduce. And yet it is not based on reason. You need only think of the impulsive decision made by the tennis player who runs up to the net, to understand that it has nothing in common with the learned construction that the coach, after analysis, draws up in order to explain it and deduce communicable lessons from it. The conditions of rational calculation are practically never given in practice: time is limited, information is restricted, etc. And yet agents *do* do, much more often than if they were behaving randomly, 'the only thing to do'. This is because, following the intuitions of a 'logic of practice' which is the product of a lasting exposure to conditions similar to those in which they are placed, they anticipate the necessity immanent in the way of the world. One would have to re-examine in the perspective of this logic the analysis of distinction, one of the paradoxical modes of behaviour which fascinate Elster because they are a challenge to the distinction between consciousness and the unconscious. Let me say for now – though it's actually much more complicated – that the dominant agents appear distinguished only because, being so to speak born into a position that is distinguished positively, their habitus, their socially constituted nature, is immediately adjusted to the immanent demands of the game, and they can thus assert their difference without needing to want to, that is, with the unselfconsciousness that is the mark of so-called 'natural' distinction: they merely need to be what they are in order to be what they have to be, that is, naturally distinguished from those who are obliged to strive for distinction. Far from being identifiable with distinguished behaviour, as Veblen thinks (and Elster equates me wrongly with him), to strive for distinction is the opposite of distinction: firstly because it involves recognition of a lack and the avowal of a self-seeking aspiration, and secondly because, as can easily be seen in the *petit bourgeois*, consciousness and reflexivity are both cause and symptom of the failure of immediate adaptation to the situation which defines the virtuoso. The habitus entertains with the social world which has produced it a

real ontological complicity, the source of cognition without consciousness, intentionality without intention, and a practical mastery of the world's regularities which allows one to anticipate the future without even needing to posit it as such. We here find the foundations of the difference established by Husserl, in *Ideen I*, between protension as the practical aiming at a yet-to-come inscribed in the present, thus apprehended as already there and endowed with the doxic modality of the present, and the project as the position of a futurity constituted as such, that is, as capable of happening or of not happening; and it is because he did not understand this difference, and especially the theory of the agent (as opposed to the 'subject') that founds it, that Sartre, in his theory of action, and above all in his theory of the emotions, came up against difficulties absolutely identical to those that Elster, whose anthropology is very close to his, tries to solve by a sort of new philosophical casuistry: how can I freely free myself from freedom, freely give the world the power to determine me, as in fear, etc? But I dealt with all that in great detail in *The Logic of Practice*.

Q. Why did you pick up this notion of habitus?

A. The notion of habitus has been used innumerable times in the past, by authors as different as Hegel, Husserl, Weber, Durkheim and Mauss, all of whom used it in a more or less methodical way. However, it seems to me that, in all these cases, those who used the notion did so with the same theoretical intention in mind, or at least pointed to the same line of research – whether, as in Hegel (who also uses, with the same function, notions like *hexis*, *ethos*, etc.), there is an attempt to break with Kantian dualism and to reintroduce the permanent dispositions that are constitutive of realized morality (*Sittlichkeit*), as opposed to the moralism of duty; or whether, as in Husserl, the notion of habitus and different concepts akin to it, such as *Habitualität*, show an attempt to escape from the philosophy of consciousness; or whether, as in Mauss, there is an attempt to account for the systematic functioning of the socialized body. By developing the notion of habitus, with reference to Panofsky who, in *Gothic Architecture and Scholasticism*,[11] himself developed a pre-existing concept to account for the effect of scholastic thought, I wanted to rescue Panofsky from the Neo-Kantian tradition in which he was still imprisoned (this is even clearer in *Meaning in the Visual*

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11 E. Panofsky, *Gothic Architecture and Scholasticism* (New York, 1957).

Arts[12]), by turning to good account the altogether accidental, and in any case unique, use he had made of this notion (Lucien Goldmann had seen this clearly: he had criticized me sharply for reclaiming for materialism a thinker who, in his opinion, had always refused to go in that direction for reasons of 'political prudence' – that was the way he saw things . . .). Above all, I wanted to react against the mechanistic tendencies of Saussure (who, as I showed in *The Logic of Practice*, conceives practice as simple *execution*) and those of structuralism. In that respect I was very close to Chomsky, in whom I found the same concern to give to practice an active, inventive intention (he has appeared to certain defenders of personalism as a bulwark of liberty against structuralist determinism): I wanted to insist on the *generative capacities* of dispositions, it being understood that these are acquired, socially constituted dispositions. It is easy to see how absurd is the cataloguing which leads people to subsume under structuralism, which destroys the subject, a body of work which has been guided by the desire to reintroduce the agent's practice, his or her capacity for invention and improvisation.

But I wanted to emphasize that this 'creative', active, inventive capacity was not that of a transcendental subject in the idealist tradition, but that of an acting agent. At the risk of seeing myself aligned with the most vulgar forms of thought, I wanted to insist on the 'primacy of practical reason' that Fichte spoke of, and to clarify the specific categories of this reason (I tried to carry out this task in *The Logic of Practice*). I made much use, less for thinking than as a way of giving me the courage to express my thoughts, of the celebrated 'Theses on Feuerbach': 'The chief defect of all hitherto existing materialism (that of Feuerbach included) is that the thing is conceived only in the form of the object of contemplation, but not as human activity, practice.' It was necessary to take back from idealism the 'active side' of practical knowledge which the materialist tradition, notably with the theory of 'reflection', had abandoned to it. Constructing the notion of habitus as a system of acquired dispositions functioning on the practical level as categories of perception and assessment or as classificatory principles as well as being the organizing principles of action meant constituting the social agent in his true role as the practical operator of the construction of objects.

Q. All of your work, and especially the criticisms you make of the

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12 E. Panofsky, *Meaning in the Visual Arts* (Harmondsworth, 1970).

ideology of the gift or, in the theoretical field, of the deeply antigenetic tendency of structuralism, draws its inspiration from the concern to reintroduce the genesis of dispositions, the history of the individual.

A. In this sense, if I liked the games with labels that people have enjoyed playing in the intellectual field ever since certain philosophers introduced into it the modes and models of the artistic field, I would say that I am trying to develop a *genetic structuralism*: the analysis of objective structures – those of different *fields* – is inseparable from the analysis of the genesis, within biological individuals, of the mental structures which are to some extent the product of the incorporation of social structures; inseparable, too, from the analysis of the genesis of these social structures themselves: the social space, and the groups that occupy it, are the product of historical struggles (in which agents participate in accordance with their position in the social space and with the mental structures through which they apprehend this space).

Q. All this seems very far from the rigid determinism and the dogmatic sociologism which is sometimes ascribed to you.

A. I can't recognize myself in that image and I can't help finding an explanation for it in a certain resistance to analysis. In any case, I find it quite ridiculous that sociologists or historians, who aren't always the best equipped to enter these philosophical discussions, are now reviving that debate indulged in by ageing scholars of the Belle Époque who wanted to save spiritual values from the threat of science. The fact that they can't find anything to set against a scientific construction except a metaphysical thesis strikes me as a clear sign of weakness. The discussion must be situated on the terrain of science, if we want to avoid falling into debates for schoolchildren and cultural weekly magazines, in which night all philosophical cats are black. Sociology's misfortune is that it discovers the arbitrary and the contingent where we like to see necessity, or nature (the gift, for instance, which, as has been known since Plato's myth of Er, is not easy to reconcile with a theory of liberty); and that it discovers necessity, social constraints, where we would like to see choice and free will. The habitus is that unchosen principle of so many choices that drives our humanists to such despair. It would be easy to establish – though I am doubtless rather overstating the challenge – that the choice of this philosophy of free

choice isn't randomly distributed . . . The essential thing about historical realities is that one can always establish that things could have been otherwise, indeed, *are* otherwise in other places and other conditions. This means that, by historicizing, sociology denaturalizes, defatalizes. But it is then accused of encouraging a cynical disenchantment. The question of knowing whether what the sociologist presents as an objective report and not a thesis – for instance, the fact that the consumption of food or the uses of the body vary depending on the position one occupies in the social space – is true or false, and of showing how one can explain these variations, is thus avoided, on a terrain in which this question would stand some chance of being solved. But in other respects, driving to despair those whom we have to call absolutists, whether enlightened or not, who criticize his disenchanting relativism, the sociologist discovers the necessity, the constraint of social conditions and conditionings, right in the very heart of the 'subject', in the form of what I have called the habitus. In short, he reduces the absolutist humanist to the depths of despair by showing him necessity in contingency, by revealing the system of social conditions which have made a particular way of being or doing possible, a way that is thus necessitated without, for all that, being necessary. Wretchedness of man without God or any hope of grace – a wretchedness that the sociologist merely reveals and brings to light, and for which he is made responsible, like all prophets of evil tidings. But you can kill the messenger: what he says is still true, and has still been heard.

This being the case, how can it escape notice that by expressing the social determinants of different forms of practice, especially intellectual practice, the sociologist gives us the chance of acquiring a certain freedom from these determinants? It is through the illusion of freedom from social determinants (an illusion which I have said a hundred times is the specific determination of intellectuals) that social determinations win the freedom to exercise their full power. Those who walk into the debate with their eyes closed and a little nineteenth-century philosophical baggage would do well to think about this if they don't want to lay themselves open to the easiest forms of objectification in the future. And so, paradoxically, sociology frees us by freeing us from the illusion of freedom, or, more exactly, from the misplaced belief in illusory freedoms. Freedom is not something given: it is something you conquer – collectively. And I regret that in the name of a petty narcissistic libido, encouraged by an immature denial of the realities, people can deprive themselves of an instrument that allows one truly to constitute oneself – a little

more than before, at any rate – as a free subject, by making an effort of reappropriation. Let's take a very simple example: through one of my friends, I had obtained the dossiers that a philosophy teacher in the preparatory classes had compiled on his pupils; there was a photo, the parents' occupation, and appraisals of written work. Here is a simple document: a teacher (of freedom) wrote of one of his pupils that she had a servile relationship to philosophy; it so happens that this pupil was the daughter of a housewife (and she was the only one of her kind in this sample). The example – a real one – is evidently somewhat facile, but the elementary act which consists of writing on a piece of schoolwork 'dull', 'servile', 'brilliant', 'thoughtful', etc., is the implementation of socially constituted taxonomies which are in general the interiorization of oppositions existing in the university in the form of divisions into disciplines and departments, and also in the social field overall. The analysis of mental structures is an instrument of liberation: thanks to the instruments of sociology, we can realize one of the eternal ambitions of philosophy – discovering cognitive structures (in this particular case, the categories of understanding of the teacher) and at the same time uncovering some of the best-concealed limits of thought. I could give hundreds of examples of social dichotomies relayed by the education system which, becoming categories of perception, hinder or imprison thought. The sociology of knowledge, in the case of the professionals of knowledge, is the instrument of knowledge *par excellence*, the instrument of knowledge of the instruments of knowledge. I can't see how we can do without it. Let no one pretend that I think it's the *only* instrument. It's one instrument among others, which I think I have made more powerful than it was before, and which can be made even more powerful. Every time the social history of philosophy, the social history of literature, the social history of painting, etc., is written, this instrument will be developed further: I can't see what objections anyone, except perhaps a mere obscurantist, could make to it. I think that enlightenment is on the side of those who turn their spotlight on our blinkers . . .

Paradoxically, this critical and reflexive disposition is not at all self-evident, especially for philosophers, who are often led, by the social definition of their function, and by the logic of competition with the social sciences, to refuse as something scandalous the historicization of their concepts or their theoretical inheritance. I will take (since it allows one to reason *a fortiori*) the example of Marxist philosophers who are led by their concern for 'a grand theoretical design', for instance, to perpetuate 'fighting concepts' such as

spontaneism, centralism, voluntarism (one could think of others), and to treat them as philosophical – in other words transhistorical – concepts. For instance, in France they have just published a *Dictionnaire critique du marxisme*[13] in which three-quarters (at least) of the entries are of this type (the few words which do not belong to this category were made up by Marx himself). These concepts are very frequently insults, words of abuse produced in the course of different struggles and for the needs of those struggles. Many 'Marxist' philosophers perpetuate them, wrest them out of their historical context and discuss them independently of their original use.

Why is this example interesting? Because you can see that the constraints, interests or dispositions associated with belonging to the philosophical field weigh more heavily on Marxist philosophers than does Marxist philosophy. If there is one thing that Marxist philosophy should make necessary, it is close attention to the history (and the historicity) of the concepts that we use to think about history. But the feeling that philosophy is somehow aristocratic leads one to forget to submit to historical criticism concepts that are visibly marked by the historical circumstances of their production and use (the Althusserians excelled in this way). Marxism, in the reality of its social use, ends up by being a mode of thought completely immune to historical criticism, which is a paradox, given the potentialities and indeed the demands inherent in Marx's thought. Marx laid down the bases of a sociolinguistic pragmatics, in particular in *The German Ideology*[14] (I referred to it in my sociological analysis of the style and rhetoric of Althusser). These directions have remained a dead letter, because the Marxist tradition has never had much time for reflexive criticism. In the Marxists' defence, I will say that, although one can derive from Marx's work the principles of a critical sociology of sociology and of the theoretical instruments that sociology, especially that of the Marxist variety, uses in order to think of the social world, Marx himself never made much use of historical criticism against Marxism itself . . .

Q. I remember that in Frankfurt we tried to discuss certain aspects of *Distinction*:[15] would you say that symbolic structures are a

13 G. Labica and G. Bensussan, *Dictionnaire critique du marxisme* (Paris, 1985).
14 K. Marx and F. Engels, *The German Ideology* (Moscow, 1964).
15 P. Bourdieu, *La distinction. Critique sociale du jugement* (Paris, 1979); trans. as *Distinction. A Social Critique of the Judgement of Taste*, tr. R. Nice (Cambridge, Mass., 1984).

representation of the fundamental articulations of social reality, or would you say that these structures are to a certain extent autonomous or produced by a universal mind?

A. I have always been uncomfortable with the hierarchical representation of stratified levels (infrastructure/superstructure) which is inseparable from the question of the relations between symbolic structures and economic structures which dominated the debate between structuralists and Marxists in the 1960s. I am starting to wonder more and more whether today's social structures aren't yesterday's symbolic structures and whether for instance class as it is observed is not to some extent the product of the theoretical effect of Marx's work. Of course, I won't go so far as to say that it's the symbolic structures that produce the social structures: the theoretical effect is exerted all the more powerfully in that there pre-exist, *in potentia*, 'in outline', in reality, as one of the possible principles of division (which isn't necessarily the one that's most evident to common perception), those divisions which theory, as an explicit principle of vision and division, brings into visible existence. What is sure is that, within certain limits, symbolic structures have an altogether extraordinary power of *constitution* (in the sense of philosophy and political theory) which has been greatly underestimated. But these structures, even if they no doubt owe much to the specific capacities of the human mind, like the very power to symbolize, to anticipate the future, etc., seem to me defined in their specificity by the historical conditions of their genesis.

Q. So the desire to break away from structuralism has always been very strong in you, at the same time as the intention of transferring to the domain of sociology the experience of structuralism – an intention that you set out in your 1968 article, 'Structuralism and Theory of Sociological Knowledge', which appeared in *Social Research*.[16]

A. The retrospective analysis of the genesis of my concepts that you invite me to make is necessarily an artificial exercise, which risks making me fall into what Bergson called the 'retrospective fallacy'. The different theoretical choices were no doubt more negative than positive, to begin with, and it's probable that they also arose from

16 P. Bourdieu, 'Structuralism and Theory of Sociological Knowledge', *Social Research*, 35, no. 4 (Winter 1968), pp. 681–706.

the quest for solutions to problems that one could call personal, like the concern to apprehend in a rigorous way politically burning problems which doubtless guided the choices I made, from my work on Algeria up to *Homo academicus*,[17] by way of *The Inheritors*, or else those kinds of deep and only episodically conscious drives that lead one to feel an affinity for or an aversion to this or that way of living the intellectual life, and thus to support or combat this or that philosophical or scientific position. I think, too, that I have always been strongly motivated in my choices by a resistance to the phenomena of fashion and to the dispositions, which I perceived as frivolous or even dishonest, of those who connived with them: for instance, many of my research strategies draw their inspiration from a concern to refuse the totalizing ambition that is usually identified with philosophy. In the same way, I've always had a pretty ambivalent relationship with the Frankfurt School: the affinities between us are clear, and yet I felt a certain irritation when faced with the aristocratic demeanour of that totalizing critique which retained all the features of grand theory, doubtless so as not to get its hands dirty in the kitchens of empirical research. The same goes for the Althusserians, and for those interventions, both simplistic and peremptory, that philosophical arrogance enables people to make.

It was the concern to react against the pretensions of grand criticism that led me to 'dissolve' the big questions by applying them to objects that from a social point of view were minor or indeed insignificant and, in any case, closely defined, and thus capable of being empirically apprehended, such as photographic practices. But I was reacting no less against the microphrenic empiricism of Lazarsfeld and his European epigones, whose false technological perfection concealed an absence of any real theoretical problematic – an absence that generated empirical errors, sometimes of a completely elementary sort. (In parenthesis, I'd say that it would be granting far too much to the so-called 'hard' current of American sociology if one were to accord it the empirical rigour it claims for itself, as opposed to the more 'theoretical' traditions, often identified with Europe. One needs to take into account the whole effect of domination exercised by American science, and also the more or less apologetic or unconscious adherence to a positivist philosophy of science, to explain how the inadequacies and technical mistakes caused by the positivist conception of science, on all levels of

17 P. Bourdieu, *Homo academicus* (Paris, 1984); trans. as *Homo academicus*, tr. P. Collier (Cambridge, 1988).

research, from sampling to the statistical analysis of data, can pass unnoticed. One soon loses count of the number of cases in which segments of experience aping experimental rigour conceal the total absence of a real sociologically constructed object.)

Q. And, in the case of structuralism, how did your practical attitude to this particular trend develop?

A. On this point too, to be completely honest, I think I was guided by a sort of theoretical sense, but also and perhaps above all by a rejection – quite a deep-seated one – of the ethical position implied by structuralist anthropology, the haughty and distant relationship established between the researcher and the object of his research, namely ordinary people, thanks to the theory of practice, explicit in the case of the Althusserians, who made the agent into a mere 'bearer' (*Träger*) of the structure (the notion of the unconscious fulfilled the same role in Lévi-Strauss). In this way, breaking away from Lévi-Strauss's analysis of native 'rationalizations', which are quite incapable of enlightening the anthropologist about the real causes or the real reasons behind modes of practice, I insisted on asking informants the question why. This obliged me to discover, with reference to marriages for instance, that the reasons for contracting the same category of marriage – in this case, marriage with the parallel cousin on the father's side – could vary considerably depending on the agents and also on the circumstances. I was on the track of the notion of strategy . . . And at the same time, I was beginning to suspect that the privilege granted to scientific and objectivist analysis (genealogical research, for example), in dealing with the natives' vision of things, was perhaps an ideology inherent in the profession. In short, I wanted to abandon the cavalier point of view of the anthropologist who draws up plans, maps, diagrams and genealogies. That is all very well, and inevitable, as *one moment*, that of objectivism, in the anthropologist's procedures. But you shouldn't forget the other possible relation to the social world, that of agents really engaged in the market, for example – the level that I am interested in mapping out. One must thus draw up a theory of this non-theoretical, partial, somewhat down-to-earth relationship with the social world that is the relation of ordinary experience. And one must also establish a theory of the theoretical relationship, a theory of all the implications, starting with the breaking off of practical belonging and immediate investment, and its transform-

ation into the distant, detached relationship that defines the scientist's position.

This vision of things, that I am presenting in its 'theoretical' form, probably started out from an intuition of the irreducibility of social existence to the models that can be made of it or, to put it naively, of 'life's profusion', of the gap between real practices and experiences and the abstractions of the mental world. But far from making this the foundation and justification of irrationalism or of a condemnation of scientific ambition, I tried to convert this 'fundamental intuition' into a theoretical principle, which must be seen as a factor in everything that science can say about the social world. For instance, you have the whole set of ideas, which I'm dealing with at present, about *scholè*, leisure and school, as the principle of what Austin called the *scholastic view*, and of the errors that it systematically creates.

Science can't do anything by paying lip-service to the rich inexhaustibility of life: that is merely a feeling, a mood without interest, except for the person expressing it, who in this way puts on the airs of an emancipated lover of life (in opposition to the frigid and austere scientist). This acute feeling for what Weber called the *Vielseitigkeit*, the manysidedness, of social reality, its resistance to the venture of knowledge, was doubtless the basis of the thinking that I have been constantly engaged in on the limits of scientific knowledge. And the work that I am preparing on the theory of fields – and which could be called 'the plurality of worlds' – will end with a consideration of the plurality of logics corresponding to different worlds, that is, to different fields as places in which different kinds of common sense, different commonplace ideas and different systems of topics, all irreducible to each other, are constructed.

It is clear that all this was rooted in a particular social experience: a relationship, which was not experienced as either natural or self-evident, with the theoretical position. This difficulty in adopting a cavalier point of view, from a position of superiority, on Kabyle peasants, their marriages or their rituals, doubtless stemmed from the fact that I had known very similar peasants, who had a similar way of talking about honour and shame, etc., and that I could sense the artificiality both of the vision that I sometimes had by observing things from a strictly objectivist point of view – that of genealogy, for example – and indeed of the vision that informants proposed to me when, in their concern to play the game, to be equal to the situation created by the theoretical questioning, they turned themselves as it

were into the spontaneous theoreticians of their practice. In a word, my critical relation to intellectualism in all its forms (and especially in its structuralist form) is without any doubt linked to the particular place I originally occupied in the social world and to the particular relation to the intellectual world that this form favoured, and that sociological work has only reinforced, by neutralizing the sanctions and repressions linked to learning at school – which, for their part, by giving me the means to overcome the repressions of scientific language, doubtless made it possible for me to say a number of things that scientific language excluded.

Q. By working within a structuralist logic, albeit in an unorthodox way, you drew people's attention to the concept of honour and domination, of strategies for acquiring honour; you also emphasized the category of *praxis*.

A. I really must point out that I have never used the concept of praxis which, at least in French, tends to create the impression of something pompously theoretical – which is pretty paradoxical – and makes one think of trendy Marxism, the young Marx, the Frankfurt School, Yugoslav Marxism . . . I've always talked, quite simply, of practice. That being said, the big theoretical intentions, those condensed in the concepts of habitus, strategy and so on, were present in my work, in a half-explicit and relatively undeveloped way, right from the start (the concept of *field* is much more recent: it emerged from the encounter between research into the sociology of art that I was starting to undertake, in my seminar at the École normale, around 1960, and the beginning of the chapter devoted to religious sociology in *Wirtschaft und Gesellschaft*). For instance, in my earliest analyses of honour (I've since reformulated them several times . . .), you find all the problems that I am still tackling today: the idea that struggles for recognition are a fundamental dimension of social life and that what is at stake in them is the accumulation of a particular form of capital, honour in the sense of reputation and prestige, and that there is, therefore, a specific logic behind the accumulation of symbolic capital, as capital founded on cognition [*connaissance*] and recognition [*reconnaissance*]; the idea of strategy, as a way of directing practice that is neither conscious and calculated, nor mechanically determined, but is the product of the sense of honour as a feel for that particular game, the game of honour; and the idea that there is a logic of practice, whose specificity lies above all in its temporal structure. I would refer here to the critique I wrote

of the analysis of the exchange of gifts in Lévi-Strauss: the model which shows the interdependence of gift and counter-gift destroys the practical logic of exchange, which can only function if the objective model (every gift requires a counter-gift) is not experienced as such. And this misconstrual of the model is possible because the temporal structure of exchange (the counter-gift is not only different, but *deferred*) masks or contradicts the objective structure of exchange. I think these analyses included, potentially, the essentials of what I have since developed. That is why I was able to pass imperceptibly and quite naturally from the analysis of Berber culture to the analysis of school culture (in any case, I got these two activities to coexist in practical terms between 1965 and 1975, since I was working simultaneously on what would lead on the one hand to *Distinction* and on the other to *The Logic of Practice*, two complementary books which summarize that whole period): most of the concepts around which I organized the work on the sociology of education and culture that I carried out or directed at the Centre for European Sociology came into being on the basis of a generalization of the results of the ethnological and sociological work that I had done in Algeria (that is particularly easy to see in the preface that I wrote for the collective book on photography, *Un art moyen*[18]). I am thinking in particular of the relationship between subjective hopes and objective opportunities that I had observed in the economic, demographic and political behaviour of Algerian workers, and that I could also observe in French students or their families. But the transfer is even more evident in the interest I took in the cognitive structures, taxonomies and classificatory activities of social agents.

Q. And is the development of your empirical interest in the way education is directed (*The Inheritors*) linked to your position in the intellectual field?

A. It's clear that my vision of culture and the education system owes a great deal to the position I occupy in the university, and especially to the path that led me there (which doesn't mean that it is relativized by this fact) and to the relationship with the school institution – I've described it several times – that was favoured by this path. But it also goes without saying that, as I have just shown, the analysis of schools – and this is something which is misunder-

18 P. Bourdieu, L. Boltanski, R. Castel and J. C. Chamboredon, *Un art moyen, essai sur les usages sociaux de la photographie* (Paris, 1970).