

triumphant technocracy of the moment, appeals to science in order to legitimate itself. Moreover, the new 'student life' which is created in the faculties suddenly invaded by an incomparably more numerous and more diversified clientele than in the past, in terms both of social origins and of gender (it is around 1970 that girls become as numerous as boys in the arts faculties) forms a social experiment through which, as in bohemian circles in the nineteenth century, a new bourgeois 'life-style' developed, making room for values excluded from the old, pre-war, neo-Kantian university and still not admitted in the disciplines professed by the boarding schools leading to the 'schools for the elite' – that is, desire, pleasure and 'anti-repressive' dispositions. All these are themes which will be strongly orchestrated by all the philosophical avant-garde,<sup>14</sup> from Deleuze to Foucault, via Derrida and even Althusser (with his 'ideological state apparatuses'), not to mention the minor heresiarchs, more closely 'tuned in' to the new vulgate.<sup>15</sup>

I have made no concessions in writing this book, but I trust that it bears no malice, for it comprises, as the reader will have guessed, a considerable proportion of self-analysis by proxy, as well as a distance, no doubt encouraged by sociology but first affirmed in the fact of abandoning philosophy for the social sciences – and that, obviously, at a time when, thanks to Lévi-Strauss's rehabilitation of ethnology, it became possible to do so without stooping too low . . . And the special place held in my work by a somewhat singular sociology of the university institution is no doubt explained by the peculiar force with which I felt the need to gain rational control over the disappointment felt by an 'oblate' faced with the annihilation of the truths and values to which he was destined and dedicated, rather than take refuge in feelings of self-destructive resentment.

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## A 'Book for Burning'?

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*Historians don't want to write a history of historians. They are quite happy to plunge endlessly into limitless historical detail. But they themselves don't want to be counted as part of the limitless historical detail. They don't want to be part of the historical order. It's as if doctors didn't want to fall ill and die.*

Charles Péguy, L'Argent, suite

In choosing to study the social world in which we are *involved*, we are obliged to confront, in *dramatized* form as it were, a certain number of fundamental epistemological problems, all related to the question of the difference between practical knowledge and scholarly knowledge, and particularly to the special difficulties involved first in *breaking* with inside experience and then in reconstituting the knowledge which has been obtained by means of this break. We are aware of the obstacles to scientific knowledge constituted as much by excessive proximity as by excessive remoteness, and we know how difficult it is to sustain that relation of a proximity broken and restored, which requires much hard work, not only on the object of our research, but also on ourselves as researchers, if we are to reconcile everything we can know only as insiders, and everything we cannot or do not wish to know as long as we do remain insiders. But we are perhaps less aware of the problems which arise when we attempt to transmit scientific knowledge of the object, especially through the medium of writing. These problems are particularly apparent in the case of *exemplification*. This rhetorical strategy, which is commonly employed to 'make things clear', but by persuading the reader to draw on his own experience, that is, to smuggle subjective information into his reading, almost inevitably

results in reducing to the realm of ordinary knowledge those scientific constructs which had been painstakingly wrested from it.' Likewise, we have only to introduce proper names – and how can this be altogether avoided, when one of the objectives in this milieu is to 'make a name for oneself'? – to reinforce the reader's inclination to reduce to a concrete individual, seen confusedly, the constructed individual who exists as such only within the theoretically constructed space of the relations of identity and difference obtaining between the explicitly defined set of his properties, and the specific sets of properties, defined according to the same principles, which characterize other individuals. But, however vigorous our effort to eschew all the overtones which operate constantly, albeit implicitly, within ordinary reasoning, those of gossip, insult and slander, or those of the satire and the broadsheet (which, although nowadays very prone to masquerade as analysis, exploit every possible anecdote, barb and witticism, for the sheer pleasure of drawing blood or showing off), however systematically we may abstain (as we do here) from referring to matters which are none the less common knowledge – the overt involvement of academics with journalism, not to mention the secret relationships (and family affairs) that historians will make it their duty to discover – we will none the less still be open to the suspicion of undertaking an act of *denunciation* for which the reader is in fact responsible. It is the reader, reading between the lines, more or less consciously filling in the gaps in the analysis, or quite simply 'putting himself in their shoes', as the saying goes, who transforms the sense and the value of the intentionally censored report of the scientific investigation. Since the sociologist cannot describe everything that he knows (including matters which those readers most eager to denounce his 'denunciations' often know better than he does, albeit on an entirely different level), he is in danger of appearing to surrender to the well-known strategies of polemic, insinuation, allusion, hint and veiled innuendo – all those procedures which are especially dear to academic rhetoric. And yet the anonymous history to which the sociologist is reduced is no more representative of historical truth than the anecdotal narrative of the actions and attitudes of individual agents, famous or unknown, to which both old- and new-style history so easily surrender: the effects produced by the structural necessity of the field can be accomplished only through personal relationships, based on the apparent contingency of socially expressed coincidences of mutual encounter and

acquaintance and on the sympathies and antipathies inspired by a shared *habitus*.<sup>2</sup> And how can we not regret that it is *socially impossible* to expound and explain what I believe to be the real logic of historical action and the true philosophy of history by making full use of the advantages inherent in the relation of belonging, which would enable us to combine information gathered by the objective techniques of scientific enquiry with the profound intuitions gained from personal familiarity?

Thus sociological knowledge is always liable to be led back to superficial perception by the self-centred reading which focuses on anecdotes and individual details and which, if not checked by a formal language, reduces to their ordinary meaning words shared by scholarly and ordinary language. This almost inevitably partial reading generates a false understanding, based on ignorance of everything which defines specifically scientific knowledge as such, that is, the very structure of the explanatory system: it dismantles what scientific construction had created, mingling what had been separated, and in particular confusing *constructed* individuals (whether a person or an institution), which exist only in the network of relations elaborated by scientific study, with empirical individuals directly accessible to ordinary intuition. It dissolves everything that distinguishes scientific objectification either from ordinary knowledge or from the pseudo-scholarly knowledge which – as is patent in most essays on intellectuals, essays which demystify less than they suffer mystification – is almost always based on what we might call 'the Thersites principle', Thersites being the envious footsoldier in Shakespeare's *Troilus and Cressida*, determined to rubbish the mighty, or, closer to historical reality, 'The Marat principle', remembering that Marat was also, or even first and foremost, an unsuccessful physician.<sup>3</sup> The partial insight facilitated by this *reductive* drive, fuelled by resentment, leads to a naïvely finalist view of history which, unable to plumb the hidden principles of social practices, falls back on anecdotal denunciations of those apparently responsible, and finally overestimates the importance of the supposed authors of the 'conspiracies' denounced, by seeing them as the cynical authors of every despicable action.<sup>4</sup>

Moreover, those who frequent the borderland between scholarly and ordinary knowledge – essayists, journalists, academic journalists and journalistic academics – have a vital stake in blurring the frontier and denying or eliminating what separates scientific analysis from

partial objectifications, imputing to single individuals, or to a lobby, effects which in fact implicate the whole structure of the field – as we have seen in the case of the director of a certain television book programme,<sup>5</sup> or with the members of the Ecole des Hautes Etudes<sup>6</sup> associated with the *Nouvel Observateur*.<sup>7</sup> They can if they so desire indulge even here in a reading guided by idle curiosity, interpreting examples and individual cases in a perspective of snobbish gossip or critical infighting, if they wish to reduce the systematic and relational mode of explanation which is characteristic of science to the most ordinary procedure of polemical reduction, to *ad hoc* explanation using *ad hominem* arguments.

The reader can find in appendix 3 an analysis of the process (virtually a judicial procedure) through which journalistic notoriety is conferred. The main effect of this analysis is to denounce the naïvety of all personal denunciations which, on the pretext of treating the game objectively, are still fully implicated in the game, in so far as they attempt to enlist the appearance of objectivity in the service of interests linked with a position in the game: the source of the technique of the literary hit parade is neither an individual agent (Bernard Pivot, in the instance quoted), however influential and skilful he may be, nor a specific institution (such as a television programme or a magazine), nor even the totality of journalistic media able to influence the field of cultural production, but rather the set of the objective relations which constitute this field, and particularly those in force between the producers' field of production and the field of general production. The logic discovered by scientific analysis easily overruns the individual (or collective) intentions (or conspiracies) and desires of even the most lucid and powerful agents cited in the search for 'those responsible'. None the less, it would be quite wrong to derive from these analyses an argument for the dissolution of responsibilities in the network of objective relations in which each agent is involved. Against those who would use the formulation of social laws, converted into destiny, as an alibi for fatalistic or cynical resignation, we must remember that scientific explanation, which gives us the means to understand and even to exonerate, is also what may allow us to make changes. A greater understanding of the mechanisms which govern the intellectual world *should not* (and I choose these equivocal words with care) have the effect of 'releasing the individual from the embarrassing burden of moral responsibility', as Jacques Bouveresse fears.<sup>8</sup> On the contrary, it should teach him to place his responsibilities where his liberties are really situated and resolutely to refuse the infinitesimal acts of cowardice and laxness which leave the power of social necessity intact, to fight in himself and in others the opportunist indifference or conformist

ennui which allow the social milieu to impose the slippery slope of resigned compliance and submissive complicity.

It is well known that no groups love an 'informer', especially perhaps when the transgressor or traitor can claim to share in their own highest values. The same people who would not hesitate to acclaim the work of objectification as 'courageous' or 'lucid' if it is applied to alien, hostile groups will be likely to question the credentials of the special lucidity claimed by anyone who seeks to analyse his own group. The sorcerer's apprentice who takes the risk of looking into native sorcery and its fetishes, instead of departing to seek in tropical climes the comforting charms of exotic magic, must expect to see turned against him the violence he has unleashed. Karl Kraus was well placed to formulate the law according to which objectification is all the more likely to be approved and acclaimed as 'courageous' in 'family circles', the more distant in social space are the objects to which it applies; and indeed, in the editorial of the first issue of his journal, *Die Fackel*, he says that anyone who rejects the pleasure and easy profits of long-distance criticism, in order to investigate his immediate neighbourhood, which everything bids him hold sacred, must expect the torments of 'subjective persecution'. Thus we have been tempted to adopt the title, *A Book for Burning*, which Li Zhi, a renegade mandarin, gave to one of those self-consuming works of his which revealed the rules of the mandarins' game. We do so, not in order to challenge those who, despite their readiness to denounce all inquisitions, will condemn to the stake any work perceived as a sacrilegious outrage against their own beliefs,<sup>9</sup> but simply to state the contradiction which is inherent in divulging tribal secrets and which is only so painful because even the partial publication of our most intimate details is also a kind of public confession.<sup>10</sup>

Sociology is not much given to creating illusions that enable the sociologist to imagine himself for one moment in the role of a liberating hero. None the less, by mobilizing all available scientific expertise in an attempt to objectify our social milieu, far from exerting reductive violence or totalitarian imperialism – as is sometimes claimed, especially when the sociologist's study is applied to those who wish to objectify without being objectified – he offers a potential liberty; and he can at least hope that his treatise on the academic passions will be for others what it has been for him, a means of *socio-analysis*.

## THE WORK OF CONSTRUCTION AND ITS EFFECTS

When faced with the challenge of studying a world to which we are linked by all sorts of specific investments, inextricably intellectual and 'temporal', our first automatic thought is to escape; our concern to escape any suspicion of prejudice leads us to attempt to negate ourselves as 'biased' or 'informed' subjects automatically suspected of using the weapons of science in the pursuit of personal interests, to abolish the self even as knowing subject, by resorting to the most impersonal and automatic procedures, those, at least in this perspective (which is that of 'normal science'), which are the least questionable. (Here we can see the attitude of resignation which so often underpins the choice of hyperempiricism; and also the genuinely political ambition – in the specific sense – which is dissimulated by scientific neutralism, the ambition to resolve confused debates, through scientific work and in the name of science, to offer oneself as referee or judge, to negate oneself as subject involved in the field, but only to resurface 'far from the madding crowd', with the irreproachable appearance of an objective, transcendent subject.)

There is no escaping the work of constructing the object, and the responsibility that this entails. There is no object that does not imply a viewpoint, even if it is an object produced with the intention of abolishing one's viewpoint (that is, one's bias), the intention of overcoming the partial perspective that is associated with holding a position within the space being studied. But our very operations of research, by obliging us to articulate and *formalize* the implicit criteria of ordinary experience, have the effect of rendering *possible* the logical verification of their own premises. Indeed, it goes without saying that the series of successive choices, spread moreover over several years, which, in the case of the enquiry into the power structure of the arts and social sciences faculties in 1967, led us for instance to draw up the list of individuals studied by determining the world of pertinent properties which would characterize them, that is the population of the most 'powerful' or the most 'important' academics, was not accomplished with perfect epistemological transparency or entire theoretical lucidity.<sup>11</sup> Only someone who has never undertaken empirical research could believe or claim the contrary, and we cannot be sure that the kind of opacity which the successive operations have for us (where there is a proportion of what is called 'intuition', that is, a more or less verifiable form of pre-scientific knowledge of the

object directly concerned, and also of the scholarly knowledge of analogous objects) is not the true source of the indispensable creativity of empirical research: when we act without entirely knowing what we are doing, we make it possible to discover in what we have done something of which we were previously unaware.

This scholarly construction is achieved through the slow and difficult accumulation of different indices, whose relevance is suggested from first hand knowledge of the different positions of power (for instance, the Universities Consultative Committee,<sup>12</sup> or the board of examiners for the *agrégation*),<sup>13</sup> and of people considered 'powerful', or even properties commonly designated or denounced as indices of power. The 'physiognomy', globally and approximately apprehended, of the 'powerful' and of power thus gradually gives way to an analytical series of the distinctive characteristics of the holders of power and of the different forms of power, whose significance, but also importance, become clarified, during the process of research, through the statistical relations which link them to each other. Far from being, as certain 'initiatory' representatives of the 'epistemological break' would have us believe, a sort of simultaneously inaugural and terminal act, the renunciation of first-hand intuition is the end product of a long dialectical process in which intuition, formulated in an empirical operation, analyses and verifies or falsifies itself, engendering new hypotheses, gradually more firmly based, which will be transcended in their turn, thanks to the problems, failures and expectations which they bring to light.<sup>14</sup> The logic of research is an intermeshing of major or minor problems which force us to ask ourselves at every moment what we are doing and permit us gradually to understand more fully what we are seeking, by providing the beginnings of an answer, which will suggest new, more fundamental and more explicit questions.

But it would be extremely dangerous to be satisfied with this 'learned ignorance'. And I would go so far as to say that the principal virtue of the scientific work of objectification (on condition, of course, that we know how to analyse its results) consists in its allowing us to objectify objectification. Indeed, for the researcher anxious to know what he is doing, the code changes from an instrument of analysis to an object of analysis: the objectified product of the work of codification becomes, under his self-reflexive gaze, the immediately readable trace of the operation of construction of the object, the grid which has been mapped out to construct the datum, the more or less coherent system of categories of perception which have produced the object of scientific analysis, in this particular

case, the world of 'important academics' and their properties. The set of properties identified unites on the one hand the world of criteria (or properties), which, apart from the *proper name*, the most precious of all properties in the case of 'a famous name', are in fact utilizable and utilized in ordinary practice to *identify* or even to classify academics (which is corroborated by the fact that we are dealing for the most part with published information, and especially with the formulas used for self-introduction), and, on the other hand, a series of characteristics which practical experience of the university field leads us to consider as pertinent and thus to establish as constituent properties.

In addition, our self-reflexive scrutiny of the very operation of coding reveals everything which separates the constructed code, which usually only duplicates socially identified codifications like school certificates or the socio-professional categories of INSEE statistical surveys<sup>15</sup> from the practical and implicit schemata of ordinary perception; and, in so doing, it reveals all the implications which an awareness of that difference has for the adequate understanding of scientific study and its object. Indeed, if it is true that any code, as much in the sense of information theory as in the legal sense, supposes a consensus on the finite set of properties chosen as pertinent (juridical formulas, says Weber, 'take into account exclusively the unequivocal general characteristics of the case in point'), and on a set of formal relations between these properties, it would be tendentious to ignore the distinction between cases where the scientific coding duplicates a coding already existing in social reality, and cases where it produces a new criterion from scratch, thus assuming that the question of the pertinence of this criterion has been resolved, whereas it could be an object of conflict – as, more generally, it would be tendentious to gloss over the question of the social conditions and effects of the codification. One of the most important properties of any property, which is abolished when we mingle criteria constructed by the researcher with socially recognized criteria, is in fact its degree of codification, just as one of the most significant properties of a field is the degree to which its social relations are objectified in public codes.

In fact it is clear that the different properties chosen to construct the identity of different academics are very unequally used in ordinary experience to perceive and appreciate the pre-constructed individuality of these same

agents, and above all very unequally objectified, therefore very unequally present in the written sources. The frontier between the institutionalized properties, which are therefore identifiable in official documents, and properties which are not objectified, or mostly not, is relatively fluid, and is bound to change according to situations and periods (any particular scientific criterion, a socio-professional category, for instance, can become a political criterion in certain political conjunctures): thus one moves through decreasing degrees of objectification, from the formal titles used when introducing oneself (for example, on official letterheads, identity cards, visiting cards, etc.) such as university posts ('professor at the Sorbonne'), or positions of power or authority ('dean' or 'member of the Institute'), which *official* terms of reference, known and recognized by all, are often associated with terms of address ('professor', 'dean', etc.), to properties which are little used in official classifications of ordinary existence, although they are institutionalized, like the direction of a laboratory, membership of the University Supreme Council or of examiners' boards for *concours*,<sup>16</sup> and finally to all those indices, often impenetrable for the foreigner, which define what is called 'prestige', that is, one's position in strictly intellectual or scientific hierarchies. In this case the researcher is constantly faced with an alternative: either to introduce classifications which are more or less artificial or even arbitrary (or, at the very least, always liable to be denounced as such), or to bracket out hierarchies which, even if they do not exist in an official, public, objectified state, are constantly at issue and operative in the very constitution of objectivity. In fact, as we shall see, the same is true of all criteria, even the most 'self-evident', such as purely 'demographic' indices, which allow their authorized users to view their science as a 'natural' science.<sup>17</sup> But what we find when we choose indices of 'intellectual' or 'scientific prestige' – is that the questions of criteria which the researcher is led to formulate about his object, that is, the problems of legitimate membership and of hierarchization and, more precisely, the problems of power and of the principles defining and hierarchizing power, are problems which are already inherently formulated in *the object itself*.

Thus the work of construction of the object determines a finite set of *pertinent properties*, established hypothetically as *effective variables*, whose variations are associated with the variations of the phenomenon observed, and it thereby defines the population of *constructed individuals*, themselves characterized by the possession of these properties to varying degrees. These logical operations produce a set of effects which must be articulated to avoid recording them unwittingly in the form of an affirmation (which constitutes the cardinal error of objectivist positivism). In the first place, the

objectification of the non-objectified (for example, scientific prestige), as we have just seen, tends to create an effect of officialization of a quasi-judicial kind: thus the establishment of classes of international celebrity founded on the number of references or the elaboration of an index of journalistic participation are operations absolutely analogous to those achieved from within the field by the producers of hit parades.<sup>18</sup> This effect cannot be overlooked in the test case of properties which are officially or tacitly excluded from all taxonomies, whether official and institutionalized or unofficial and informal, such as religious denomination or sexual preference (heterosexuality or homosexuality), although they can intervene in practical judgements and be associated with variations visible in observed reality (it is no doubt this kind of information which people have in mind when they denounce the 'police investigation' tendencies of sociological enquiry).

In order to illustrate the effects of scholarly codification, especially the homogenization of the status accorded to different properties which are very unequally objectified in real life, we need only consider the mode and degree of existence *qua groups* of the populations identified by the different criteria, which vary from age-groups or sexual groupings (despite the appearance of a feminist consciousness and movement), to sets such as *normaliens*<sup>19</sup> or *agrégés*,<sup>20</sup> which are characteristic of two different modes of collective existence: the title of *normalien* underwrites a network of practical solidarity maintained by a minimum of institutional support (graduate association, annual newsletter, annual dinner of matriculation years); the title of *agrégé*, which implies no real solidarity linked to any common experience, underwrites an organization, the Société des Agrégés, oriented towards defending the value of the diploma and all the values dependent on it, and mandated with the power to speak and act for the group as a whole, to express and defend its interests (in negotiations with government institutions, for instance).

The effects of institutionalization and homogenization which operate through simple codification, and of the elementary form of recognition which it instinctively accords to unequally acknowledged criteria, operate as if they were laws. In so far as they operate unbeknown to the researcher, they lead him to conclude 'in the name of science' what is not conclusive in reality: indeed, the degrees of recognition granted in practice to the different properties vary considerably according to the agents (and also according to situations

and periods), and certain properties which some might advance and lay public claim to, such as the fact of writing for the *Nouvel Observateur* (not an imaginary case), will be perceived by others, situated in different positions in their world, as stigmata, entailing exclusion from that world. Cases of perfect inversion like this, where one person's pedigree can become another's mark of infamy, one's coat of arms another's insult, and vice versa, are there to remind us that the university field is, like any other field, the locus of a struggle to determine the conditions and the criteria of legitimate membership and legitimate hierarchy, that is, to determine which properties are pertinent, effective and liable to function as capital so as to generate the specific profits guaranteed by the field. The different sets of individuals (more or less constituted into groups) who are defined by these different criteria have a vested interest in them. In proffering these criteria, in trying to have them acknowledged, in staking their own claim to constitute them as legitimate properties, as specific capital, they are working to modify the laws of formation of the prices characteristic of the university market, and thereby to increase their potential for profit.

Thus there exists quite objectively a plurality of rival principles of hierarchization, and the values which they determine are incommensurate, or even incompatible, because they are associated with mutually conflicting interests. One cannot simply, as believers in indices doubtless would, conflate participation in the Universities Consultative Committee or the board of examiners of the *agrégation* with the fact of being published by Gallimard or of writing for the *Nouvel Observateur*, and a pseudo-scholarly construction conflating these indices would only reproduce the polemical amalgam designated by the semi-scholarly notion of 'mandarin'. A number of the criteria used by scientific construction as instruments of knowledge and analysis, even those most neutral and 'natural' in appearance, like age, also function in the reality of practices as principles of division and hierarchization (we only have to think of the classificatory and often polemical use of oppositions – old/young, palaeo/neo, former/recent, etc.) and thereby also become an object of conflict. That is to say that we can only avoid claiming that the truth of the university field is one or other of the more or less rationalized representations which are engendered in the struggle for classification, and especially the semi-scholarly representations which scholarly circles give of themselves, if we include in our study the process of classification.

effected by the researcher, and the relationship between that and the classificatory attributions indulged in by the agents (and by the researcher himself, once he is not directly involved in research).

Indeed, it is because it does not clearly operate the break between these two discourses that in this domain, as elsewhere, sociology so often tends to offer semi-scholarly taxonomies, which it calls 'typologies', mingling indigenous labels, often closer to the stigma or the insult than the concept, with 'scholarly' notions, constructed on the basis of a more or less informed analysis. Organized around several typical characters, these 'typologies' are neither really concrete – although they are no doubt derived, like the 'characters' described by moralists, from familiar figures of first-hand experience or from more or less polemical categories – nor really constructed, although they resort to terms current in the jargon of the American *social scientists*, such as *local* or *parochial* or *cosmopolitan*. Being the product of a realist intention, that of describing *typical* individuals or groups, they combine, in disordered fashion, different principles of opposition, mingling criteria as heteroclit as age, relations to political power or to science, etc. For instance, we have *the locals* (including *the dedicated*, 'strongly committed to the institution', *the true bureaucrat*, *the homeguard* and *the elders*) and the *cosmopolitans* (including *the outsiders* and *the empire builders*), whom Alvin W. Gouldner distinguishes according to their attitudes towards the institution (*faculty orientations*), their investment in professional competence and their internal or external orientation;<sup>21</sup> or again, according to Burton Clark, who sees in them the representatives of different 'cultures', *the teacher*, devoted to his students, *the scholar-researcher*, 'chemist or biologist totally committed to his laboratory', *the demonstrator*, a sort of instructor bent on transmitting technical competence, and finally *the consultant*, 'who spends as much time in the air as on the campus';<sup>22</sup> or finally – although we might continue at length in this vein – the six types distinguished by John W. Gustad, *the scholar*, who considers himself 'not as an employee but as a free citizen of the academic community', *the curriculum adviser*, *the individual entrepreneur*, *the consultant*, 'always off campus', *the administrator* and *the cosmopolitan*, 'oriented towards the outside world'.<sup>23</sup>

It is hardly necessary to point out all the cases where the concept-as-insult and the semi-scholarly stereotype – like that of the *jet sociologist* – become transformed into semi-scientific 'types' –

*consultant*, *outsider* – and all the subtle indices where the position of the analyst in the space being analysed is betrayed. In fact these typologies gain credence inasmuch as, being the product of classificatory schemes current in the milieu under consideration, they operate as series of *real divisions*, analogous to those operated by ordinary intuition, of a domain of objective relations thus reduced to a *population* of university lecturers, and prevent us from conceptualizing the university field, either as such or in the relations which, at different times in its history and in different national contexts, link it with the field of political power on the one hand, and the intellectual and scientific field on the other. If these typologies, unfortunately very common and perfectly representative of what often passes for sociology, warrant our interest, it is because, through retranslating things into a language of scholarly appearance, they can lead people – and not only their authors – to believe that they are providing access to a superior level of knowledge and reality, whereas ultimately they are telling us less than we would learn from a direct description by a good inside informer. The classifications engendered by a disguised application of the principles of vision normally used for the needs of practice 'are like those that someone would give who tried to classify clouds by their shapes', as Wittgenstein says.<sup>24</sup> But appearances are often superficially convincing, and these objectless descriptions, which have in their favour the logic of ordinary experience and the façade of scientificity, are better placed to satisfy common expectations than are scientific constructions, which both directly confront the individuality of the particular case seized in all its complexity, and are much more distant from the immediate representation of the real given by ordinary language or its semi-scholarly retranslation.

Thus social science cannot break with common criteria and classifications and disentangle itself from the struggles of which they are both end and means, unless it takes them explicitly as its object instead of letting them slyly infiltrate scientific discourse. The milieu which it must study is the object and, to a certain extent at least, the product of rival, sometimes hostile representations, which all claim the status of truth and thereby the right to exist. Any position adopted towards the social world orders and organizes itself from a certain position in the world, that is to say from the viewpoint of the preservation and augmentation of the power associated with this position. Thus it is that, in a milieu which depends as much for its

very reality as the university field does on the representation which its agents have of it, these agents can exploit the plurality of principles of hierarchization and the low degree of objectification of symbolic capital, in an attempt to impose their vision and modify their position inside that space, as far as their symbolic power allows, by modifying the representation which others (and they themselves) can have of this position. There is nothing more revealing, from this point of view, than the forewords, exordia, preambles or prefaces, which often disguise behind the appearance of a methodologically indispensable methodological premise their more or less skilful attempts to translate into scientific virtues the necessities and above all the limits inherent in a position and a trajectory, at the same time as depriving inaccessible virtues of their enchantment. Thus we may see the scholar whom we readily call 'narrowly specialized', and who cannot be unaware of this (it must have been pointed out to him a thousand times, in a thousand ways, in the cruelly euphemistic language of academic judgement, and first of all, perhaps, through those magisterial verdicts which grant him only 'solid scholarship'), working to discredit the flights of 'brilliant' essayists and 'ambitious' theorists; as for the latter, they will rely on the rhetoric of irony to praise the erudition which delivers such 'precious material' for their reflection, and only if they felt really threatened in the hegemonic position which they allocate themselves would they overtly express their arrogant contempt for the petty and sterile caution of 'positivist' hacks.<sup>25</sup>

In short, as we can see in polemical exchanges, which are the high spots of a constant process of symbolic competition, practical knowledge of the social world, and especially of adversaries, obeys a *reductionist* tendency; it resorts to classificatory epithets which designate or identify groups, and groups of properties, in an eclectic perspective, and do not admit awareness of the principles on which they are based. And we would have to be quite ignorant of all this logic to suppose that a technique like that used by 'judges', which consists in interrogating a group of agents, treated as experts, on the problems under discussion – for instance, the relevant criteria for defining university power or a hierarchy of prestige – might evade the question of which agencies can be authorized to legitimate the agencies of legitimation. Indeed, we have only to put this technique to the test to see that it reproduces the very logic of the game which it is supposed to referee: the different 'judges' – and the same judge

at different moments – deploy different or even incompatible criteria, thus reproducing (but only imperfectly, because in an *artificial situation*) the logic of classificatory judgements produced by the agents in ordinary existence. But, above all, the slightest attention to the relations between the categories selected and the properties of those who formulate them shows that we anticipate the nature of the judgements obtained if we anticipate the criteria of selection of the 'judges', that is to say the spatial position, still unknown at the moment of research, which motivates their judgements.

Does this mean that the sociologist has no choice but to use the technical yet also symbolic force of science to set himself up as a judge of the judges, and impose a judgement which can never be entirely free from the presuppositions and prejudices associated with his position in the field which he claims to objectify, or to renounce his claim to objectivist absolutism and be satisfied with a perspectivist recording of the viewpoints at issue (including his own)? In fact, his freedom in the face of the social determinisms which affect him is proportionate to the power of his theoretical and technical methods of objectification, and above all, perhaps, to his ability to use them on himself, so to speak, to objectify his own position through the objectification of the space within which are defined both his position and his primary vision of his position, and positions opposed to it; it is proportionate to his capacity simultaneously to objectify the very intention of objectifying, to take a sovereign, absolute view of the world, and especially of the world which he belongs to, and to work at excluding from scientific objectification everything that it might owe to the ambition to dominate by means of the weapons of science; finally, it is proportionate to his capacity to orientate the effort of objectification towards the dispositions and interests which the researcher himself owes to his trajectory and to his position and also towards his scientific practice, towards the presuppositions which this entails in its concepts and problematics, and in all the ethical or political aims associated with the social interests inherent in a position within the scientific field.<sup>26</sup>

When research comes to study the very realm within which it operates, the results which it obtains can be immediately reinvested in scientific work as instruments of reflexive knowledge of the conditions and the social limits of this work, which is one of the principal weapons of epistemological vigilance. Indeed, perhaps we can only make our knowledge of the scientific field progress by

using whatever knowledge we may have available in order to discover and overcome the obstacles to science which are entailed by the fact of holding a determined position in the field. And not, as is so often the case, to reduce the *reasons* of our adversaries to *causes*, to social interests. We have every reason to think that the researcher has less to gain, as regards the scientific quality of his work, from looking into the interests of others, than from looking into his own interests, from understanding what he is motivated to see and not to see. And thus we can suggest, without any suspicion of moralizing, that scientific benefit could only be obtained in this case by renouncing social benefit, and particularly by resisting any tendency to use science or scientific effects to attempt to achieve a social triumph in the scientific field. Or, in other words, that we may well have some chance of contributing to the science of power if we renounce the attempt to turn science into an instrument of power, above all in the world of science.

Nietzschean genealogy, the Marxist critique of ideologies, the sociology of knowledge – all the perfectly legitimate procedures which tend to relate cultural productions to social interests – have usually been led astray as a result of the dual strategy deriving from the temptation to use the science of conflict in the conflict itself. This sort of illicit use of social science (or of the authority which it can bestow) finds an exemplary illustration, exemplary in its naïvety, in an article where Raymond Boudon passes off as a scientific analysis of the intellectual field in France a denunciation of 'extra-scientific' success, which (barely) disguises its chauvinist plea, which amounts to making a virtue out of obscurity.<sup>27</sup> A description which contains no critical reflection on the position from which it is articulated can have no other principle than the interests associated with the unanalysed relation that the researcher has with his object. Thus it is hardly surprising if the fundamental thesis of the article is nothing but a social strategy aiming to discredit the national hierarchy of celebrities by reproaching it with being purely French, that is linked to 'singularities' and particularities, automatically identified as anachronistic – harping on the theme of the literary turn of mind – and by taking this hierarchy, tacitly designated as different from the international hierarchy (the only scientific one), and therefore as extra-scientific, to compare it unfavourably with a hierarchy presumed scientific, because international, that is, American.<sup>28</sup> Strikingly enough, this scientific declaration receives not the slightest trace of empirical verification. For this would force us to admit, for instance, as we shall see, that an important fraction of the producers who dominate what, in

an article published some time ago,<sup>29</sup> I called the restricted field or restricted market (and which Raymond Boudon, forever concerned with the external trappings of scientific appearances, calls 'Market I', without acknowledging his source) are also the best known on the general production market, or to discover that the highest scores as regards foreign translations or mentions in the *Citation Index*, which has nothing typically French about it, are generally obtained by the best-known researchers in the most extra-scientific sectors of the national market – except for the most traditional disciplines, like ancient history or archaeology, which are not as 'literary' as all that.

In constructing the finished, finite set of the properties which function as effective forces in the struggle for specifically university power, and which are possessed to diverse degrees by the set of effective agents, the sociologist produces an objective space, defined in a methodical and unambiguous (and therefore reproducible) way, and irreducible to the sum of all partial representations of agents. Thus the 'objectivist' construction which is the condition of the break with intuitive vision and with all hybrid discourse mingling the semi-concrete and the semi-constructed, the label and the concept, is also what enables us to reintegrate into our knowledge of the object the pre-scientific representations which are an integral part of the object. We cannot in fact dissociate the intention to establish the structure of the university field, a space with several dimensions, constructed on the basis of the whole set of the powers which can prove effective at any particular moment in competitive struggles, from the intention to describe the logic of the struggles which derive their principle from this structure and aim to preserve or transform it by redefining the hierarchy of powers (and therefore of criteria). Even when it does not take the organized form of rivalry between consciously militant or tacitly loyal groups, the struggle, whose criteria and the properties they imply are both end and means, is an ineluctable fact which the researcher is bound to integrate into his model of reality instead of artificially attempting to exclude it by setting himself up as arbiter or as 'impartial observer', as a judge of last appeal who would alone be competent to produce the *right order* able to reconcile everyone by finding a place for everything. He needs to transcend the alternatives of the objectivist vision of objective classification – of which the search for a single scale and for cumulative indices is a caricatural illustration – and the subjectivist or, even better, *perspectivist* vision, which would settle for recording

the diversity of hierarchies, treated as so many incommensurable viewpoints. In fact, like the social field taken as a whole, the university field is the locus of a classification struggle which, by working to preserve or transform the state of the power relations between the different criteria and between the different powers which they designate, helps create the classification, such as it may be objectively grasped at a given moment in time; but the representation which the agents have of the classification, and the force and the orientation of the strategies which they may deploy to maintain or subvert it, depend on their position in the objective classifications.<sup>30</sup> Scientific work, therefore, aims to establish an adequate knowledge both of objective relations between the different positions and the necessary relations which are established, through the mediation of the systems of dispositions of their occupants, between these positions and the respective dispositions which they adopt, that is to say between the point occupied in the space and their viewpoint of that same space, which is part of reality and of the development of that space. In other words, the 'classification' produced by scientific work through the delineation of *regions* in the space of positions is the objective ground of the classificatory strategies through which the agents aim to preserve or modify the space. Among these strategies we must include the constitution of groups mobilized to ensure the defence of the interests of their members.

The need to integrate the two visions, objectivist and perspectivist, requiring us to work at attempting to objectify objectification, to create a theory of the effect of theory, is necessary for another, no doubt fundamental, reason, as much from the theoretical as from the ethical or political viewpoint: the scholarly construction of the 'objective' space of agents and of operative properties tends to replace a global and confused perception of the population of the 'powerful' with an analytic and reflexive perception, thus destroying the vagueness and the mist of imprecision and uncertainty which constitute our everyday world. To understand 'objectively' the world in which we live without understanding the logic of this understanding, and what differentiates it from practical understanding, is to prevent oneself from understanding what makes the world livable in and viable, that is to say the very vagueness of practical understanding. As in the case of the exchange of gifts, the objectivist approach, which is unaware of its own truth, denies the condition which makes practice possible, that is, a misconstrual of the model

which would explain that practice. And only the satisfactions that a reductivist vision offers those of reductivist mood could lead us to forget to introduce into the model of reality the distance between experience and the objectivist model, which corresponds to the truth of experience as we know it.

There are no doubt few worlds which provide so much scope, or even so much institutional support, for the game of self-deceit and for the gap between the representation experienced and the true position occupied in a social field or space; the tolerance granted to this gap doubtless reveals the inner truth of a milieu which authorizes and encourages all forms of *splitting the ego*, in other words all ways of making the confusedly perceived objective truth coexist with its negation, thus permitting those most lacking in symbolic capital to survive in this struggle of each against all, where everyone depends on everyone else, at once his competitor and client, his opponent and judge, for the determination of his own truth and value, that is, of his symbolic life and death.<sup>31</sup> For it is obvious that these individual systems of defence would hardly have any social effectiveness unless they met with the complicity of those who are led by their occupation of an identical or homologous position to recognize in these vital errors and illusions of survival the expression of an effort to persevere in a social identity which they recognize as their own.

Many more or less institutionalized representations and practices can indeed only be understood in terms of *collective defence mechanisms* through which the agents find a way to avoid the excessively harsh questioning that the rigorous application of their explicit criteria, such as those of science or erudition, would provoke. Thus it is that the multiplicity of scales of evaluation, scientific or administrative, academic or intellectual, offers a multiplicity of paths to salvation and of forms of excellence, allowing everyone, with the complicity of everyone else, to disguise truths known to all.<sup>32</sup> The scientific report should take account of the effect of vagueness which the indeterminacy of the criteria and the principles of hierarchization engender in objectivity itself: the indeterminacy, for instance, of criteria such as place of publication or the number of foreign colloquia or visiting lectures is due to the fact that there is, for each science, a complex and contentious hierarchy of reviews and publishers, of foreign countries and colloquia, and also that those who refuse to participate may appear difficult to distinguish from those who are

not invited. In short, it would be a serious blow to objectivity not to write into the theory the objective imprecision of the hierarchies which the model, constructed on the basis of an (indispensable) survey of the indicators of scientific status, aims precisely to overcome. And we must ask ourselves if the very plurality of hierarchies, and the coexistence of practically incommensurate forces, scientific prestige and university power, internal recognition and external renown, are not the effect of a sort of anti-trust law both written into the structures and at the same time tacitly recognized as a protection against the consequences of a strict application of the norms officially professed.

We can see another instance of this in the paradoxical fact that this milieu which claims allegiance to science proposes practically no institutionalized signs of scientific prestige as such. No doubt we can invoke the Institute,<sup>33</sup> and the gold medal of the CNRS,<sup>34</sup> but the former distinction seems to consecrate politico-moral attitudes as much as scientific accomplishments, while the latter is absolutely exceptional. And it is in the same perspective, that is to say as a concession imposed by the need to subscribe to and insure against the specific risks of the profession of researcher, that we should interpret the tendency of so many scientific committees to function like bipartite committees, or the strategies familiar to occupants of subordinate positions inside the academic or scientific fields which consist in using the ability to universalize offered by political or trade-union rhetoric in order to treat homologies of position as identity of condition (in accordance, for instance, with the pattern of the 'three Ps', *père, patron, professeur* [i.e. father, manager, professor], which was such a success in 1968) and thus to establish more or less strained identifications, in the name of the solidarity, which is never insignificant, of all the subordinates in all the fields, between positions and stances as far apart as those of the Renault car worker on the assembly line and the temporary researcher<sup>35</sup> at the CNRS, between the struggle against accelerated production rhythms and the rejection of purely scientific criteria. We would also need to note methodically all the cases where politicization functions as a compensatory strategy allowing an escape from the specific laws of the academic or scientific market, for instance all forms of political criticism of scientific studies which allow scientifically outmoded producers to give themselves – and to give their peers – the illusion that they transcend what transcends them: the state of historical Marxism – such as it may be observed in the reality of the social uses which are made of it – would not be comprehensible if we failed to see that often, with all its references to the 'people' and the 'popular', it has that function of last resort which

allows the least scientifically capable to set themselves up as scientific judges.

#### EMPIRICAL INDIVIDUALS AND EPISTEMIC INDIVIDUALS

If we have had to try to clarify, through a self-reflexive detachment from the operations of research and from the object which they have produced, the principles of production which have been *deployed*, it is because this logical work, as long as it is successful, can contribute to reinforcing the logical and sociological control of writing and its effects and giving greater efficacy to warnings against readings which tend to ruin the work of construction. For it is in fact only if we know 'what the sociologist does', to use Saussure's terms, that we can adequately read the product of his operations.

The risks of misunderstanding in the transmission of scientific discourse on the social world depend, in a very general way, on the fact that the reader tends to make the utterances of the language of construction function as they would function in ordinary usage. This is quite clear in the case where the reader, ignorant of Weber's distinction, perceives as sociological value-judgements 'references to values' inherent in the object of study:<sup>36</sup> when, for example, he speaks of a 'second-class faculty', of a 'subordinate discipline' or of the 'lower echelons' of the university space, the sociologist is only recording a *fact of evaluation* which he attempts to explain by relating it to the ensemble of the social conditions of its existence, and he can even see in it the explanatory principle of the form of the *value-judgements* destined to 'refute' it (for example, the protests which it can provoke, if incorrectly read). But it is only a minor form of misunderstanding, since it is blatant, and blatantly evident. And the most dangerous effect of this reading, as we can see in the case of named individuals, consists in substituting the logic of ordinary knowledge for the logic of scientific knowledge.

Scientific discourse demands a scientific reading, capable of reproducing the operations of which it is itself the product. However, the words of scientific discourse, and especially those designating persons (named individuals) or institutions (such as the Collège de France),<sup>37</sup> are exactly the same as those of ordinary discourse, of fiction or history, whereas the referents of these two species of discourse are separated by all the distance which is introduced by

the scientific break and by construction. Thus, in everyday life, the proper name merely *identifies*, and, acting in the same way as what logicians call an indicator, it is in itself virtually insignificant ('Smith' does not signify 'a blacksmith') and gives virtually no information about the person designated (unless it is an aristocratic or famous name or if it is ethnically specific). As a label capable of being arbitrarily applied to any object, it says that the object designated is different, without specifying *in what respect* it differs; as an instrument of *recognition*, and not of *cognition*, it singles out an *empirical individual*, generally apprehended as singular, that is to say, different, but without analysing the difference. The constructed individual, on the contrary, is defined by a finite set of explicitly defined properties which differ through a series of identifiable differences from the set of properties, constructed according to the same explicit criteria, which characterize other individuals; more precisely, it identifies its referent not in ordinary space, but in a space constructed of differences produced by the very definition of the finite set of effective variables.<sup>38</sup> Thus, strictly speaking, the constructed Lévi-Strauss processed and produced by scientific analysis does *not* have the same referent as the proper name which we use in daily life to designate the author of *Tristes tropiques*; in an ordinary utterance, 'Lévi-Strauss' is a signifier to which may be applied the infinite universe of predicates corresponding to the various differences which may distinguish the French ethnologist not only from all other professors but from all other human beings, and which we will bring into being, in each case, as a function of the principle of implicit permanence which will be imposed on us by the needs or the exigencies of practice. Sociological construction is distinguished from other possible constructions – those of psychoanalysis, for example – by the finite list of the effective properties, of the active variables, which it includes and, concomitantly, by the infinite list of the properties which it excludes, at least provisionally, as irrelevant. Variables such as eye or hair colour, blood group or height are bracketed out, so to speak, and it is as if the constructed Lévi-Strauss had none. But, as is shown by the diagram of the analysis of correspondences, where he is distinguished by the position which he occupies in a constructed space, the epistemic space is characterized by the system of differences, of uneven intensity and unevenly linked to each other, established between the finite set of his relevant properties in the theoretical domain considered, and the whole set

of finite sets of properties attached to the set of other constructed individuals. In short, he is defined by the position which he occupies in the space which his properties have helped to construct (and which also partly helps to define it). As opposed to the doxic Lévi-Strauss, who is inexhaustible, the epistemic individual contains nothing evading conceptualization; but this self-transparency is the corollary of reduction, and any progress in this theory as *viewpoint*, as principle of selective vision, will have to stem from the invention of categories and operations able to reconcile the theory with properties provisionally excluded (for instance, those which the psychoanalyst would construct).<sup>39</sup>

The spatial diagram uses one of the properties of ordinary space, the reciprocal externality of the objects distinguished, to reproduce the logic of a strictly theoretical space of differentiation, that is, the logical efficacy of a set of principles of differentiation (the factors of the analysis of correspondences) allowing us to distinguish between individuals who have been constructed through the statistical treatment of properties determined by the application to different empirical individuals of a common definition, that is, of a common viewpoint, concretized in a set of identical criteria.<sup>40</sup> And the best illustration of what marks the difference between the epistemic and the empiric individual is the way in which at a certain point of the analysis we can observe the fusion of more than one pair of empirical individuals (Raymond Polin and Frédéric Deloffre,<sup>41</sup> for example). They become *indistinguishable* (they had the same co-ordinates on the first two axes) from the viewpoint of the analyst at that moment, which was written into the list of variables chosen at that stage of research.<sup>42</sup>

This example, which I have chosen deliberately, poses the problem of the effect of reading and of the danger of regression into ordinary knowledge as simple recognition. A naïve reading of the diagram tends to dissolve what constitutes precisely the scientific virtues of its construction: in this theoretical space of differences, a space constructed from a finite – and relatively restricted – set of explicitly defined variables, such a reading can 'recognize' the set of differences noted empirically in everyday experience, since these do in fact provide its principle, even including differences noted empirically which had not been introduced into the viewpoint originally adopted for the construction, such as differences in political attitudes, particularly in May 1968, or, as could no doubt be verified, in styles

and works. Thus any reader endowed with the practical sense of placing which is acquired through prolonged exposure to the regularities and rules of that milieu will recognize himself easily (too easily, if we forget the conditions of construction) in the epistemic space constructed with a rigour and a self-transparency which are completely excluded from ordinary experience. This feeling of certainty is understandable if we realize that, like a well-drafted map or plan, the diagram is a model of 'reality' as we know it or, more accurately, as it is revealed to us, in ordinary existence, in the (veiled) guise of distance observed, respected, denied by transgression or condescension, etc.: in its forms of hierarchies and precedences, of affinities or incompatibilities – of style, character, etc. – of sympathies or antipathies, of complicities or hostilities; and, in this way, it can function as the objectified, *codified* form of the practical patterns of perception and action which orientate the practices of the agents best adjusted to the immanent necessity of their domain. In truth, the multi-dimensional space shown by the diagram claims to be an isomorphic representation of the university field: a true image of this structured space, it establishes between each agent and each property of the two spaces a one-to-one correlation, such that the set of relations between the agents and the properties of the two spaces show the same structure. The structure revealed by this research is the true principle of the nature, which is essentially relational, of each element and its operations, and notably of the strategies of the agents, and thereby of the development of the elements and the structure of the relations which define them.

After these analyses, we understand better that scientific discourse on the social world is problematic, and reaches crisis-point in the case of a discourse bearing directly on the very game which its author finds himself playing, and wagering on. If it is difficult, if not impossible, to prevent utterances from containing proper names, or individual examples from assuming a polemical value, it is because the reader almost inevitably substitutes for the epistemic subject and object of the discourse the practical subject and object, converting a neutral utterance on a constructed agent into a performative denunciation, or, as the saying goes, into an *ad hominem* polemic.<sup>43</sup> The writer occupies a position in the space described: he knows it and he knows that his reader knows it. He knows that the latter will tend to relate the constructed vision he offers to the position he occupies in the field, and to reduce it to a viewpoint like any

other; he knows that he will see in the slightest nuances of the writing – a *but*, a *perhaps* or, simply, the tense of a verb – indices of bias; he knows that he is likely to notice, amid all the efforts expended to produce a neutral language stripped of all personal resonance, only the effect of greyness, judging that it is a high price to pay for what is, after all, never more than a form of autobiography. And it is probable that the effort of the enquiring subject to negate himself as empirical subject, to disappear behind the anonymous record of his operations and his results, is doomed in advance to failure: thus the use of circumlocution which would substitute for the proper name the (partial) enumeration of pertinent properties, apart from guaranteeing only an appearance of anonymity, resembles one of the classic procedures of university polemics, which is to designate opponents only by allusions, insinuations or undertones understood solely by those initiated in the code, that is to say, more often than not solely by the opponents attacked. Scientific neutralization can thus help to charge discourse with that extra violence which is added to the discreet polemics of academic hatred by the methodical erasure of all external signs of violence. In short, just as the proper name constituted by general terms, 'Prairie Trail', 'Black Bear', 'Bear's Back-fat', 'Wagtail Fish',<sup>44</sup> does not in practice, whatever Lévi-Strauss says, function as a classificatory act conferring on the bearer of the name the properties designated by the general terms which it combines, so the circumlocution (Professor of Ethnology at the Collège de France) intended to show that the *agent* thus designated is not the *individual* Claude Lévi-Strauss has very little chance, unless a deliberate warning is issued, of being read as other than a euphemistic substitute for Claude Lévi-Strauss. And the concepts constructed to designate the regions of the theoretical space of pertinent positions, in this particular case, the classes of individuals defined by the tenure of the same region of constructed space (thanks to the analysis of correspondences), have every likelihood of suffering the same fate, either by being overshadowed, in the reading, by the institutions which they partially overlap (Collège de France, Ecole des Hautes Etudes, Sorbonne, etc.), or by functioning as simple *labels*, similar to the impressionistic notions which are current in everyday life, especially in polemics, and which are taken up more or less unthinkingly by the authors of 'typologies'.

Because, among other reasons, the rigorous use of techniques of analysis of the most refined data, such as the analysis of correspondences,

would suppose a perfect mastery of the mathematical principles which underlie them, and of the sociological effects which they produce by their more or less conscious application to social data, there is no doubt that, despite all the warnings issued by their 'inventors', a good deal of users (and readers) find it difficult to assign their veritable epistemological function to the notions devised in order to name the factors, or the divisions which these determine: indeed, these units are not strictly defined logical classes, separated by clearly marked frontiers whose numbers possess *all* the pertinent characteristics, that is to say, a number of attributes all necessary, and all to the same degree, to determine belonging (in such a way that the possession of certain properties cannot be compensated for by the possession of certain others). The set of agents collected in the same region of space is united by what Wittgenstein calls a 'family resemblance', in a sort of common physiognomy, often close to that apprehended in a confused and *implicit* way by 'first-hand' intuition. And the properties which contribute to the characterization of these sets are united by a complex network of statistical relations which are also relations of *intelligible affinity* – rather than of logical similarity – which the analyst must *render explicit* as completely as possible and condense in a designation at once stenographic, mnemotechnic and suggestive.

There too, the choices of writing are rendered difficult by ordinary usage, and especially by the tradition which consists in using concepts in '-ism' as slogans or as sanitized insults, that is, usually, as proper names designating empirical individuals or groups. The designation of a class by a concept is thus reduced to an act of *nomination*, obeying the ordinary logic of this kind of operation. To give a name, one single name, to an individual or group of individuals, as we see in the *nickname*, which unlike the ordinary proper name is not in itself insignificant, and which functions in the manner of the proper name according to Lévi-Strauss, is to adopt one of the possible viewpoints towards them and claim to impose it as the single, legitimate viewpoint. What is at stake in the symbolic struggle is the monopoly of legitimate nomination, the dominant viewpoint which, in gaining recognition as the legitimate viewpoint, causes its truth as a specific, situated, dated viewpoint to be misconstrued.<sup>45</sup> Thus, to escape the danger of polemical recuperation, we might think of designating each of the sectors of the space by a plurality of concepts designed to remind us that each of the regions of the space can, by definition, only be conceptualized and expressed in its relation to the others, and also to remind us that, in practice – which the theory

must incorporate – each sector becomes the object of different or even antagonistic nominations, according to the viewpoint from which it is perceived: to give to an individual or a group the name which it gives itself ('the emperor', 'the nobility') is to *recognize* it, accept it as dominant, accept its viewpoint, agree to adopt towards it the viewpoint of perfect coincidence which it adopts towards itself; but we can also give it another name, the name given it by others and especially its enemies ('the usurper'), and which it rejects as insult, calumny, defamation. Or we can even give it its *official* name, conferred by an official authority, recognized as legitimate, that is, by the state, holder of the monopoly of legitimate symbolic violence (the socio-professional categories of the INSEE). In this particular case, the sociologist, both judge and plaintiff, has little chance of seeing his monopoly of nomination recognized. In any case, there is every likelihood that his designations will also function in ordinary logic and that the reader will interpret them as hostile, as alien, and therefore insulting, if they are applied to himself and his own group, but will adapt and exploit them for his own purposes, still using them as insults and polemical attacks, when they are objectifying others, the *out-group*.

To struggle to defeat these readings, to prevent anyone from reducing the instruments of generalized objectification to the weapons of partial objectification, our constant aim should be to choose systematic circumlocution (despite the threat to communication, which prefers simple and stable appellations), leading to a complete enumeration of the relevant properties, or to choose the most 'synoptic' concept, the one most capable of instantly evoking the system of relations which characterize it objectively, that is to say from the viewpoint of the outside observer,<sup>46</sup> and combine them with the epistemic polyonymy which would effectively express the different ways in which the same set can be defined in its *objective* relation to other sets; without forgetting to refer to empirical polyonymy – that is, the diversity of the names really used to designate the same individuals or the same groups, and thereby the diversity of aspects with which a person or a group appears to other persons and other groups – which should remind us that the struggle for the imposition of a legitimate viewpoint is part of objective reality.<sup>47</sup>

We would need, I believe, an unshakeably positivist confidence to see in these questions of scientific writing the complacent survival

of a 'literary' disposition. The concern to control his discourse, that is the reception of his discourse, imposes on the sociologist a scientific rhetoric which is not necessarily a rhetoric of scientificity: he needs to inculcate a scientific reading, rather than belief in the scientificity of what is being read – except in so far as the latter is one of the tacit conditions of a scientific reading. Scientific discourse is distinct from the discourse of *fiction* – from the novel, for instance, which passes itself off more or less openly as a feigned and fictitious discourse – in that, as John Searle remarks, it *means* what it says, it takes seriously what it says and accepts responsibility for it, that is, if the case arises, for its mistakes.<sup>48</sup> But the difference is not only situated, as Searle believes, at the level of illocutionary intentions. A survey of all the features of discourse designed to signify the doxic modality of utterances, that is to inspire belief in the truth of what is said, or on the other hand to point out that it is only a pretence, would no doubt show that the novel can resort to a rhetoric of veracity while scientific discourse can conform to a rhetoric of scientificity destined to produce a fiction of science, superficially matching the picture that the upholders of 'normal science' have, at a given moment, of a discourse which is socially accredited as being responsible for what it proposes.

If socially accredited scientificity is such an important objective, it is because, although truth has no intrinsic force, there is an intrinsic force of belief in truth, of belief which produces the appearance of truth. In the struggle between different representations, the representation socially recognized as scientific, that is to say as true, contains its own social force, and, in the case of the social world, science gives those who hold it, or who appear to hold it, a monopoly of the legitimate viewpoint, of self-fulfilling prophecy. It is because it contains the possibility of this specifically social force that science, in the case of the social world, is *necessarily contested*, and that the threat of attack which it contains is bound to provoke defensive strategies, especially from the holders of temporal power and those who are their homologues and their allies in the field of cultural production. The most common of these defensive strategies consists in reducing the epistemic viewpoint, at least partly liberated from social determinism, to a simple doxic viewpoint, by relating it to the position of the researcher in the field. But those who do so ignore the fact that this strategy of disqualification entails a recognition of the very intention which defines the sociology of scientific

knowledge, and the fact that it could only be justified if it challenged scientific discourse in the name of a more rigorous scientific knowledge of the limits associated with the conditions of its production.<sup>49</sup>

The importance of the social issues which are linked, in the case of the social sciences, to the social effects of scientificity explains the fact that the rhetoric of scientificity can play such a decisive part in these sciences. Any discourse with scientific pretensions towards the social world must take into account the state of representations of scientificity and the norms to be respected in practice in order to produce a *scientific effect* and thereby acquire symbolic efficacy and the social profits associated with conformity to scientific appearances. Thus any such discourse is bound to be situated in the space of possible discourses on the social world, and is bound to receive a part of its properties from the objective relations which unite it to them, especially to their style. And it is within the framework of this relation, in ways largely independent of the wishes and the knowledge of the authors, that its *social value*, its status as science, as fiction or as fiction of science, is defined. The art we call realist, in painting as in literature, is only ever that art which is able to produce an impression of reality, that is to say an impression of conforming to reality; similarly, the discourse we call scientific is likely to be the one which produces an impression of scientificity based on at least apparent conformity to the norms by which we recognize science. It is according to this logic that the style which we call literary or scientific plays a determining role: just as, in other times, professional philosophy in the process of establishing itself affirmed its claims to rigour and profundity, especially in the case of Kant, through stylistic precision, in opposition to the facility and frivolity of fashionable society, or as, contrariwise (as Wolf Lepenies has so well demonstrated), Buffon compromised his claims to scientificity through excessive attention to fine writing, so too any sociologists whose exaggerated concern with linguistic finesse might threaten their status as scientific researchers can resist this, more or less consciously, by rejecting literary elegance and draping themselves in the trappings of scientificity (graphs, statistical tables, even mathematical formalism, etc.).

In fact, positions adopted in the space of styles correspond closely to positions in the university field. Thus it is that, faced with the alternative of writing too well, which can procure literary benefit

but undermines the impression of scientificity, or of writing badly, which can produce an impression of rigour or profundity (as in philosophy) but to the detriment of social success, geographers, historians and sociologists adopt strategies which, transcending individual differences, are related to their respective positions. Placed in a central position in the field of the arts and social science faculties, therefore halfway between the two systems of demands, the historians, while accepting the compulsory attributes of scientificity, generally take considerable care over their writing. If the geographers and sociologists are related in showing more indifference to literary qualities, the former display the humility of disposition which suits their station by adopting the neutral style which is equivalent, in the order of expression, to the empiricist abdication to which they most commonly resign themselves. As for the sociologists, they often betray their claim to hegemony (inherent right from the start in the Comtean classification of the sciences) by borrowing alternatively or simultaneously from the most powerful rhetorics in the two fields in relation to which they are forced to situate themselves, the mathematical, often used as an external sign of scientificity, or the philosophical, often reduced to lexical display.<sup>50</sup>

Knowledge of the social space within which scientific practice is accomplished, and the world of options, stylistic or other, with reference to which its choices are defined, does not lead us to repudiate scientific ambition and reject all possibility of knowing and of stating facts, but rather to reinforce, through awareness and the vigilance it encourages, our capacity to know reality scientifically. Indeed, it leads to a much more radical questioning than all the cautionary advice and safety regulations assigned by 'methodology' to 'normal science', and which make it possible to obtain scientific respectability at a low cost: 'reliability' (*le sérieux*), in science as elsewhere, is a typically social virtue, and it is no coincidence that it is attributed to those who, in their lifestyle as in the style of their studies, provide guarantees of the predictability and measurability characteristic of 'dependable', settled people. This is the case for all the bureaucrats of normal science who, settled in science as in an official residence, are careful to take seriously only what deserves to be, that is, especially, themselves, to heed only what counts and what can be counted on. The social character of these demands is seen in the fact that they concern more or less exclusively the external display of scientific virtue: do not the greatest symbolic profits of

science accrue to those kinds of scientific pharisees who know how to deck themselves with the most visible signs of scientificity, for example by mimicking the procedures and the languages of the most advanced sciences? Ostentatious conformity to the formalist requirements of normal science (significance tests, calculation of error, bibliographical references, etc.), and the superficial respect of minimal prescriptions, necessary but not sufficient, those thoroughly social virtues with which all the holders of social authority in the domains of science automatically identify, have more than the simple effect of ensuring that the directors of the major scientific bureaucracies have a scientific respectability out of all proportion to their real contribution to science. Institutionalized science tends to establish as its model of scientific activity a routine practice where the most scientifically decisive operations can be accomplished without reflection or critical control, since the apparent flawlessness of the visible procedures – often entrusted to assistants – deflects any questioning likely to challenge the respectability of the scientist and his science. This is why, far from being a scientific form of the claim to absolute knowledge, a social science armed with a scientific knowledge of its social determinations constitutes the strongest weapon against 'normal science' and against positivist *self-confidence*, which represents the most formidable social obstacle to the progress of science.

Marx suggested that, every now and then, some individuals managed to liberate themselves so completely from the positions assigned to them in social space that they could comprehend that space as a whole, and transmit their vision to those who were still prisoners of the structure. In fact, the sociologist can affirm that the representation which he produces through his study transcends ordinary visions, without thereby laying claim to such absolute vision, able fully to grasp historical reality as such. Taken from an angle which is neither the partial and partisan viewpoint of agents engaged in the game, nor the absolute viewpoint of a divine spectator, the scientific vision represents the most systematic totalization which can be accomplished, in a given state of the instruments of knowledge, at the cost of as complete as possible an objectification both of the historical moment and of the work of totalization. In this way it marks a genuine step along the path leading to the *focus imaginarius* spoken of by Kant, this imaginary focus from which the perfected *system* could be discovered but which a properly scientific intention

can only posit as the ideal (or regulative idea) of a practice which can only hope to approach ever closer to it in so far as it has renounced all hope of reaching it immediately.

Thus we return to our point of departure, that is to say to the work on himself that the researcher must accomplish to try to objectify everything that links him to his subject, and which the reader must duplicate on his own behalf in order to master the social principles of the more or less unhealthy interest which he may take in his reading. Unless he wants to run the risk of universalizing an individual viewpoint and of delivering up a more or less rationalized form of the unconscious determined by his position in the social space, he must unwrap in succession all the boxes within which the researcher and the greater part of his readers are enclosed, and all the more surely the less they wish to know; that is, he must evoke the structure of the field of power and the relation which the university field taken as a whole maintains with it, analyse – as far as the empirical data permit – the structure of the university field and the position which the different faculties occupy within it, and, finally, analyse the structure of each faculty and the position that the different disciplines occupy within it. Thus (in chapter 3) we will be able to allow the return, albeit in profoundly modified form, of the question which lay behind our research into the foundations and forms of power in the arts and social science faculties on the eve of 1968, only when (in chapter 2) we have better defined the position of the initial object inside the interlocking social spaces and, thereby, the position of the researcher who himself participates in these different spaces with the insights and blind spots they imply. Having sketched the structure of the university field as a whole and the structure of the field of the arts and social science faculties which, because of their central position in the university field and because of the very division between the humanities and the social sciences, allow us to see with striking clarity the tensions, caused by the strengthening of science and scientists, which rack the whole university field and each of its faculties, we will then be able to ask relevant questions of history, and attempt to grasp the determinants and the object of certain transformations, of which the state of the structure observed represents one moment. The increase in the population of students and the concomitant increase in the population of lecturers have profoundly modified the power relations at the heart of the university field and at the heart of each faculty, especially

in the relations between the professional grades and between the disciplines, themselves unequally affected by the transformation of hierarchical relations, and this has happened in spite of all the actions which have been objectively orchestrated (without being intentionally co-ordinated) by the professors trying to ensure the defence of their corps (chapter 4). The morphological changes here (as also in the literary field) are the medium whereby history, which mechanisms of reproduction tend to resist, invests the fields, which are *open* spaces obliged to draw from outside the resources necessary for their functioning, and are thereby liable to become the locus of that collision of independent causal series which creates the event, that is, the quintessentially historical (chapter 5).

This attempt to sketch a structural history of recent developments in the educational system presents a problem of writing which touches on the use of tenses, and beyond that on the epistemological status of discourse. Should we, because of the relative specificity of the documents and enquiries used, and because of their clearly declared limitations in social space and time, refrain from endowing our discourse with the generality which is conveyed by the transhistoric present of the scientific utterance? That would be equivalent to repudiating the very project of any intellectual enterprise attempting to 'immerse itself' in historical singularity in order to sort out the transhistorical invariants (abandoning the privilege of timeless generalities to essayists or anthologists who are embarrassed by no historical reality other than their reading or their personal experience). Unlike 'the discursive tenses' (often a present) which according to Benveniste 'suppose a speaker and a listener and in the former the intention to influence the latter in some way', but just like the aorist, 'the supreme historical tense' which according to Benveniste again 'objectifies the event by detaching it from the present' and 'excludes all autobiographical linguistic form',<sup>51</sup> the omnitemporal present of scientific discourse marks the objectifying distance without referring to a situated and dated past. On these lines it is suitable for the scientific report, when this presents *structural invariants* which can be observed as such in very different historical contexts and still function in the same world as *constants*, everywhere operative. Incidentally, it is this presence in the present – that is, what matters now – which makes of sociology an eventful or, as the Anglo-Saxons say, a *controversial* science, and all the more so the more advanced it is. It is clear that if we are more ready to

grant the historian the objectivity and neutrality of the scientist it is because we are generally more indifferent to the games and the objectives in which he is involved; always remembering that chronological distance in the chronological present is not a good measure of historical distance, that is, the distance which converts into history, into the past historic, and not forgetting that belonging to the present as *news*, that is to say, as a universal agency of objects and ideas which can be chronologically past or present but which are effectively a *live issue*, thus practically *made present* in the moment considered, is what defines the divide between the 'ever-living and ever-burning' present and the past, 'dead and buried' like the social worlds for which it was once still a live issue, new and up to date, active and reactive.

Thus the present seems necessary for the description of all the mechanisms or processes which, despite superficial changes – as regards vocabulary, notably *president* instead of *dean*, *UER*<sup>52</sup> instead of *faculty*, etc. – are still part of our historic present because they still exercise their effects. To take an extreme case, it is certain that we can still use the present to discuss the principle of clarification dear to Thomas Aquinas, as long as, in the unchanging time of university life, dissertations and so many other forms of discourse are organized according to the triadic divisions and subdivisions of scholastic thought. Even the ahistorical model of the quintessential historical event, the crisis as synchronization of different social times, can be written in the omnitemporal present as the unique accomplishment of a series of omnitemporal effects whose conjunction produces a historical conjuncture.

The present is valid also for everything which is true at the moment of the enquiry and is still true at the moment of reading, or which can be understood by using the patterns and mechanisms established on the basis of the enquiry. Thus it is that the distance of nearly twenty years between the moment of study and the moment of publication allows everyone, using the changes which have occurred in the meantime and those which they portend, to verify whether the proposed model – and in particular the analysis of the transformations of the power relations between academic disciplines and professional grades – allows us to explain phenomena which, being subsequent to the enquiry and difficult to grasp in a methodical way, are only alluded to here. I am thinking of the appearance of new forms of power, particularly that of the unions, which tend to

force to its ultimate conclusions the process initiated by the transformation of the mode of recruitment of lecturers and assistant lecturers, by giving the products of the new mode of recruitment control over the appointment of the new subordinate teachers – which can lead in certain cases to the *de facto* elimination of the prime categories of the old mode of recruitment, that is the *normaliens* or the *agrégés*.<sup>53</sup> And how can we not see that the contradiction between the new mode of recruitment and the previous mode of career promotion, which, being protected by the past which it aims to maintain, tends to confine in subordinate positions the products of the new mode of recruitment, is the principle behind a number of claims, pressures and transformations, which, helped especially by political change, attempt to abolish the differences associated with the initial differences of the school and university trajectory (by abolishing either the differences between grades or the differences between the diplomas which give access to these grades).

Finally, we should underline the various precautions needed to avoid misreadings of these analyses, yet at the same time we should elaborate them to the point where we convert them into *ad hoc* responses, that is to say in more than one case into *ad hominem* arguments: indeed, everything leads us to believe that readings of our scientific reconstructions of variations and invariants will vary, as does experience of real history, according to the reader's relation to the past and the present of the academic institution. To understand in this case is difficult only because we understand far too well, in a manner of speaking, and because we *do not wish* to see or know what it is we understand. Thus it is that the easiest thing can also be the most extraordinarily difficult because, as Wittgenstein says, 'The problem of *understanding* language is connected with the problem of the Will'. Sociology, which of all sciences is the best placed to know the limits of the 'intrinsic force of the true idea', knows that the force of the resistances which will be opposed to it will be very exactly commensurate with the 'problems of the will' which it has managed to overcome.<sup>54</sup>