

PIERRE BOURDIEU

## **The specificity of the scientific field and the social conditions of the progress of reason \***

"The training of the scientific mind is not only a reform of ordinary knowledge, but also a *conversion* of interests."  
Gaston Bachelard, *Le rationalisme appliqué*.

The sociology of science rests on the postulate that the objective truth of the product — even in the case of that very particular product, scientific truth — lies in a particular type of social conditions of production, or, more precisely, in a determinate state of the structure and functioning of the scientific field. The "pure" universe of even the "purest" science is a social field like any other, with its distribution of power and its monopolies, its struggles and strategies, interests and profits, but it is a field in which all these *invariants* take on specific forms.

1. As a system of objective relations between positions already won (in previous struggles), the scientific field is the locus of a competitive struggle, in which the *specific* issue at stake is the monopoly of *scientific authority*, defined inseparably as technical capacity and social power, or, to put it another way, the monopoly of *scientific competence*, in the sense of a particular agent's socially recognised capacity to speak and act legitimately (*i.e.* in an authorised and authoritative way) in scientific matters.

Two rapid observations, to dispel possible misunderstandings: First, care must be taken not to reduce the objective relations which constitute the field to the aggregate of the *interactions*, in the interactionist sense, *i.e.* the strategies — which it in fact determines, as will be seen below<sup>1</sup>. Secondly, it will be necessary to define "socially recognised": it will be seen that the group which grants this recognition always tends to be progressively narrowed to the group of scientists, *i.e.* competitors, as accumulated scientific resources, and correlatively the autonomy of the field, increase.

When we say that the field is the locus of struggles, we are not simply breaking away from the irenic image of the "scientific community", as described by scientific hagiography — and often, subsequently, by the sociology of science — *i.e.* the notion of a sort of "kingdom of ends" knowing no other laws than that of the perfect competition of ideas, a contest infallibly decided by the intrinsic strength of the true idea. We are also insisting that the ope-

ration of the scientific field itself *produces and presupposes* a specific form of interest (scientific practices appearing as "disinterested" only in relation to different interests, produced and demanded by other fields).

References to scientific interest and scientific authority (or competence) is intended to eliminate from the outset certain distinctions which, in the implicit state, pervade discussions of science: thus, to attempt to distinguish those aspects of scientific competence (or authority) which are regarded as pure social representation, symbolic power, marked by an elaborate apparatus of emblems and signs, from what is regarded as pure technical competence, is to fall into the trap which is constitutive of all competence, a *social authority* which legitimates itself by presenting itself as pure technical reason (as can be seen, for example, in the technocratic uses made of the notion of competence)<sup>2</sup>. In reality, the august array of insignia adorning persons of "capacity" and "competence" — the red robes and ermine, gowns and mortar-boards of magistrates and scholars in the past, the academic distinctions and scientific qualifications of modern researchers, all this social fiction which is in no way fictitious — modifies social perception of strictly technical capacity. In consequence, judgements on a student's or a researcher's scientific capacities are *always contaminated* at all stages of academic life, by knowledge of the position he occupies in the instituted hierarchies (the hierarchy of the universities, for example, in the USA).

This is well put by a physicist in a thoroughly remarkable article which contrasts in its clarity and lucidity with the bulk of the sociological literature devoted to science: "While still in high school, the scientist-to-be becomes aware that competition and prestige will affect his future success. He must strive for good grades in order to be admitted to college and later to graduate school. He realizes the importance of attending a college of high reputation *not only because it will provide him with a better education but also because it will facilitate his later admission to a good graduate school*".<sup>3</sup> In setting the testimony of a physicist above the works of the sociologists of science, I am conscious of committing what will appear to many an act of sacrilege, a profanation so outrageous that — but for this express statement, and even perhaps despite it — it could only be attributed to ignorance (ignorance of the fact that Fred Reif "is a mere physicist", or ignorance of the "right" authors) and would in itself be sufficient to disqualify its perpetrator. All the more so because it is accompanied by a whole series of transgressions which are *no less deliberate* but are likely to be interpreted within the same logic of hostile prejudice (a situation which has the virtue of exposing one of the functions of *quotation*, one which is haughtily neglected by the quotologists, that of *ingratiation* by the multiplication of signs of recognition intended to elicit recognition). Only the lacunae of "lack of education" could explain the presence of authors who are barely recognised (Kuhn himself...), marginal (Glaser, Feuer, etc.) or unknown (the Europeans, for instance, whom the official science loftily ignores), or, worse still, the *absence* of the canonical authors of that same official science who, moreover, fail to receive recognition (measurable by the number of quotations or the length of passages analysing or even taking issue with them) proportional to their place in the hierarchical order<sup>4</sup>. The social strength of false science lies partly in the fact that it attracts to its *reasons* a challenge which should be directed at its *causes*, and those who read to the end of this text will perhaps understand why energy which can be better employed elsewhere has not been expended on *arguing* with false science (having to read it is quite enough). It is moreover only to be expected that authors who themselves presume to give an account of science

without referring to the educational system and the work done on it should find it perfectly incongruous to make reference to the educational system, *haute couture* and art. And what will be said of the barbarism of taking the disciples, who at least put their concepts to the test of the facts, more seriously — in a text which bears all the outward signs of “theoretical writing” — than the master who has produced them? By the tribute it has to pay to science, false science lends itself at least to scientific criticism, and it is sometimes possible to take from it facts that it has produced and to set them in a quite different system of relations.

Because all scientific practices are directed towards the acquisition of scientific authority (prestige, recognition, fame, etc.), intrinsically *two-fold* stakes, what is generally called “interest” in a particular scientific activity (a discipline, a branch of that discipline, a method) is always two-sided; and so are the strategies tending to bring about the satisfaction of that interest.

An analysis which tried to isolate a purely “political” dimension in struggles for domination of the scientific field would be as radically wrong as the (more frequent) opposite course of only attending to the “pure”, purely intellectual, determinations involved in scientific controversies. For example, the present-day struggle between different specialists for research grants and facilities can never be reduced to a simple struggle for strictly “political” power: in the social sciences, those who in the USA have reached the top of the great scientific bureaucracies (such as the Columbia Bureau of Applied Social Research) cannot force others to recognise their victory as the victory of science unless they are also capable of imposing a definition of science implying that genuine science requires the use of a great scientific bureaucracy provided with adequate funds, powerful technical aids, and abundant manpower; and they present the procedures of large-sample surveys, the operations of statistical analysis of data, and formalisation of the results, as universal and eternal methodology, thereby setting up as the measure of all scientific practice the standard most favourable to their personal or institutional capacities. Conversely, epistemological conflicts are always, inseparably, political conflicts: so that a survey on power in the scientific field could perfectly well consist of apparently epistemological questions alone.

It follows from a rigorous definition of the scientific field as the objective space defined by the play of opposing forces in a struggle for scientific stakes, that it is pointless to distinguish between strictly scientific determinations and strictly social determinations of practices that are essentially *overdetermined*. In a passage which deserves to be quoted in full, Fred Reif shows, almost despite himself, how artificial and indeed impossible it is to distinguish between intrinsic and extrinsic interest, between what is important for a particular researcher and what is important for other researchers: “A scientist strives to do research which he considers important. But *intrinsic satisfaction and interest are not his only reasons*. This becomes apparent when one observes what happens if the scientist discovers that someone else has just published a conclusion which he was about to reach as a result of his own research. Almost invariably he feels upset by this occurrence, although the *intrinsic interest* of his work has certainly not been affected. The scientist

wants his work *to be not only interesting to himself but also important to others*”<sup>5</sup> What is regarded as important and interesting is what is likely to be recognised by others as important and interesting, and thus to make the man who produces it appear more important and interesting in the eyes of others. (We shall have to return to this dialectic and the conditions under which it operates to the advantage of scientific cumulativeness and not as a simple circle of mutual legitimation.)

If we are not to fall back into the idealist philosophy which credits science with the power to develop in accordance with its immanent logic (as Kuhn still does when he suggests that “scientific revolutions” occur only as a result of exhaustion of the “paradigms”) we must posit that investments are organised by reference to — conscious or unconscious — anticipation of the average chances of profit (which are themselves specified in terms of the capital already held). Thus researchers’ tendency to concentrate on those problems regarded as the most important ones (*e.g.* because they have been constituted as such by producers endowed with a high degree of legitimacy) is explained by the fact that a contribution or discovery relating to those questions will tend to yield greater symbolic profit. The intense competition which is then triggered off is likely to bring about a fall in average rates of symbolic profit, and hence the departure of a fraction of the researchers towards other objects which are less prestigious but around which the competition is less intense, so that they offer profits at least as great<sup>6</sup>.

The distinction which Merton makes (with reference to the social sciences) between “social” conflicts (over “the allocation of intellectual resources between different sorts of sociological work” or “the role appropriate to the sociologist”) and “intellectual” conflicts, “oppositions between strictly formulated sociological ideas”<sup>7</sup>, itself constitutes a social and intellectual strategy which tends to impose a delimitation of the field of objects of legitimate argument. This distinction is in fact a form of one of the strategies through which American official sociology tends to secure academic respectability and to impose a demarcation of the scientific and the non-scientific which is designed to forbid any inquiry liable to question the bases of its respectability as a breach of scientific decorum<sup>8</sup>.

An authentic science of science cannot be constituted unless it radically challenges the abstract opposition (which one also finds elsewhere, in art history for example) between immanent or internal analysis, regarded as the province of the epistemologist, which recreates the logic by which science creates its specific problems, and external analysis, which relates those problems to the social conditions of their appearance. It is the scientific field which, as the locus of a political struggle for scientific domination, assigns each researcher, as a function of his position within it, his indissociably political and scientific problems and his methods — scientific strategies which, being expressly or objectively defined by reference to the system of political and scientific positions constituting the scientific field, are at the same time political strategies. Every scientific “choice” — the choice of the area of research, the choice of methods, the choice of the place of publication, the

choice, described by Hagstrom<sup>9</sup>, between rapid publication of partly checked results and later publication of fully checked results — is in one respect — the least avowed, and naturally the least avowable — a political investment strategy, directed, objectively at least, towards maximisation of strictly scientific profit, *i.e.* of potential recognition by the agent's competitor-peers.

2. The struggle for scientific authority, a particular kind of *social capital* which gives power over the constitutive mechanisms of the field, and can be reconverted into other forms of capital, owes its specificity to the fact that the producers tend to have no possible clients other than their competitors (and the greater the autonomy of the field, the more this is so). This means that in a highly autonomous scientific field, a particular producer cannot expect recognition of the value of his products ("reputation", "prestige", "authority", "competence", etc.) from anyone except other producers, who, being his competitors too, are those least inclined to grant recognition without discussion and scrutiny. This is true *de facto*: only scientists involved in the area have the means of symbolically appropriating his work and assessing its merits. And it is also true *de jure*; the scientist who appeals to an authority outside the field cannot fail to incur discredit<sup>10</sup>. (In this respect, the scientific field functions in exactly the same way as a highly autonomous artistic field<sup>11</sup>: one of the principles of the specificity of the scientific field lies in the fact that the competitors must do more than simply *distinguish themselves* from their already recognised precursors; if they are not to be left behind and "outclassed", they must integrate their predecessors' and rivals' work into the distinct and distinctive construction which transcends it.)

In the struggle in which every agent must engage in order to force recognition of the value of his products and his own authority as a legitimate producer, what is at stake is in fact the power to impose the definition of science (*i.e.* the delimitation of the field of the problems, methods and theories that may be regarded as scientific) best suited to his specific interests, *i.e.* the definition most likely to enable him to occupy the dominant position in full legitimacy, by attributing the highest position in the hierarchy of scientific values to the scientific capacities which he personally or institutionally possesses (*e.g.* by being highly trained in mathematics, having studied at a particular educational institution, being a member of a particular scientific institution, etc.)<sup>12</sup>.

In more than one debate on the priority of a scientific discovery, the scientist who discovered the unknown phenomenon, often in the form of a simple anomaly not covered by existing theories, has clashed with the scientist who made a new scientific *fact* of it by setting it in a theoretical construction irreducible to the simple empirical datum. These political arguments about scientific property rights, which are at the same time scientific debates on the *meaning* of what has been discovered and epistemological arguments as to the *nature of scientific discovery*, are in reality the expression of the conflict between two principles

of hierarchisation of scientific practices: the debate between the principle giving primacy to observation and experimentation, and hence to the corresponding dispositions and capacities, and the principle which privileges theory and the correlative scientific interests, is one which has never ceased to be at the centre of epistemological reflexion.

The definition of what is at stake in the scientific struggle is thus one of the issues at stake in the scientific struggle, and the dominant are those who manage to impose the definition of science which says that the most accomplished realisation of science consists in having, being and doing what they have, are or do. This means, incidentally, that the *communis doctorum opinio*, as the Scholastics put it, is never more than an *official fiction* which is not in the least fictitious because the symbolic efficacy which it derives from its legitimacy enables it to perform a symbolic function similar to that performed for liberal ideology by the notion of "public opinion". Official science is not what the sociology of science generally takes it to be, that is to say, the system of norms and values which the "scientific community", an undifferentiated group, is seen as imposing on and inculcating in all its members, so that revolutionary anomie can only be imputed to the occasional misfiring of scientific socialisation<sup>13</sup>. This "Durkheimian" vision of the scientific field may well be no more than the transfiguration of the naively "functionalist" representation of the scientific universe which the upholders of the scientific order have an interest in imposing on others, starting with their competitors.

The list of examples of this sort of "functionalism" would be never-ending, even in a writer who, like Kuhn, does make room for conflict in his theory of scientific evolution: "A community of scientific specialists *will do all that it can* to ensure the continuing growth of the assembled data that it can treat with precision and detail"<sup>14</sup>. Because "function", in the sense in which it is used by the American functionalist school, is simply *the interest of the dominant*, i.e. the interest that the dominant have in the perpetuation of a system which suits their interests (or the function that the system fulfils for that particular class of agents), one only has to fail to mention interests (i.e. differential functions) to fall into "functionalism".

And it is precisely because the definition of what is at stake in the struggle is itself an issue at stake in the struggle, even in sciences — like mathematics — in which there is apparently a high degree of consensus on the stakes, that the antinomies of legitimacy constantly arise. (This explains why social science researchers have a passionate interest in the natural sciences: what is at stake in their claim to impose the legitimate definition of the most legitimate form of science, i.e. natural science, in the name of epistemology or the sociology of science, is the definition of the principles of evaluation of their own practice.) In the scientific field as in the field of class relations, no arbitrating authority exists to legitimate legitimacy-giving authorities; claims to legitimacy draw their legitimacy from the relative strength of the groups whose interests they express: inasmuch as the definition of the criteria of judgment and the principles of hierarchisation is itself at issue in a struggle,

there are no good judges, because there is no judge who is not also a party to the dispute.

One sees the naivety of the technique of asking a panel of "judges" to rank names, to which the sociological tradition frequently resorts in order to define the characteristic hierarchies of a particular field (the hierarchy of agents or institutions — in the USA, of the universities —, the hierarchy of problems, areas or methods, the hierarchy of the fields themselves, etc.). The same naive philosophy of objectivity inspires appeals to "international experts" — as if their position as foreign observers were sufficient to shield them from pre-conceptions and partisanship at a time when the economy of ideological exchanges contains so many multinational corporations, and as if their "scientific" analyses could be anything more than the scientifically masked justification of the particular state of science or scientific institutions with which they are in league (in the example cited, the American university and its sociology)<sup>15</sup>.

Scientific authority is thus a particular kind of capital, which can be accumulated, transmitted, and even reconverted into other kinds of capital under certain conditions.

Fred Reif supplies a description of the process of the formation of scientific capital and the forms its reconversion takes. The context is the particular case of the field of modern physics, in which possession of a certain amount of scientific capital tends to favour acquisition of supplementary capital, and in which a "successful" scientific career therefore presents itself as a *continuous* process of accumulation, with the initial capital, represented by the agent's scholastic qualification, playing a determining role: "While still in high school, the scientist-to-be becomes aware that competition and prestige will affect his future success. He must strive for good grades in order to be admitted to college and later to graduate school. He realizes the importance of attending a college of high reputation [...] Finally he must earn the good opinion of his teachers to secure the letters of recommendation which will help him enter college and gain scholarship grants and prizes [...] The job-seeking scientist is clearly in a more advantageous situation if he comes from a well-known institution and has been associated with a scientist of reputation. Invariably it is essential to him that there should be prominent scientists in the world who are willing to comment favourably upon the quality of his work. [...] Promotion to higher academic rank is subject to similar criteria. Again the university requests letters of recommendation from outside scientists and in some cases may appoint reviewing committees before deciding to promote someone to a tenure position."

This process continues with access to administrative posts, government commissions, etc. And the researcher also relies on his reputation among his colleagues in order to obtain research funds, to attract high-quality students, to get grants and scholarships, invitations and consultations, tours abroad, honours and distinctions (a Nobel Prize, membership of the National Academy of Science.)

The recognition, socially marked and guaranteed (by a whole series of specific signs of consecration<sup>16</sup>), which the competitor-peer group bestows on each of its members, depends on the *distinctive value* of his products and the collectively recognised *originality* (in the information-theory sense) of his contribution to the scientific resources already accumulated. The fact that the authority-capital accruing from a discovery is monopolised by the first person to have made it, or at least, the first person to have made it known and got it recognised, explains the frequency and importance of *questions of priority*. If several names come to be attached to the first discovery, the

prestige of each of them is correspondingly diminished. A scientist who makes the same discovery a few weeks or a few months later has been wasting his time, and his work is reduced to the status of worthless duplication of work already recognised (and this is why some researchers rush into print for fear of being overtaken)<sup>17</sup>. The notion of "visibility" which is frequently used by American writers (as is often the case, this is a notion in everyday use among academics) clearly expresses the *distinctive, differential value* of this particular kind of social capital: to accumulate it is "to make a *name* for oneself", one's own name (and for some, their first name), a known, recognised name, a mark which immediately distinguishes its bearer, lifting him as a visible form out of the undifferentiated, unregarded, obscure background in which the common ruck remains (hence, no doubt, the importance of metaphors of perception, the paradigm of which is the opposition "*brilliant*" / "*obscure*", in most academic taxonomies)<sup>18</sup>.

The logic of distinction operates to the full in the case of multiple authorship, which, as such, reduces the distinctive value accruing to each signatory. It is thus possible to see all the observations made by Harriet A. Zuckerman<sup>19</sup> on "patterns of name ordering" among authors of scientific papers as the product of strategies aimed at *minimising the loss of distinctive value* entailed by the necessities of the new division of scientific labour. Thus, in order to understand why Nobel Prize winners do not take first place more often than others, as one might expect given that authors are normally named in order of the relative value of their contribution, there is no need to invoke an aristocratic ethic of "noblesse oblige"; if one simply posits that a name's visibility in a series depends first on its *relative visibility*, defined by its rank in the series, and, secondly, on its *intrinsic visibility*, which it owes to the fact that, when already known, it is more easily recognised and remarked (one of the mechanisms which ensure that, here as elsewhere, the rich in capital are the ones who get richer), one can then see why the tendency to abandon first place to others increases as the capital possessed increases, and with it the symbolic profit automatically accruing to its possessor regardless of his place in the order<sup>20</sup>. The market in scientific goods has its laws, and they have nothing to do with ethics. And, if we are to avoid creating a place in the science of science, under various "scientific" names, for what agents sometimes call the "values" or the "traditions" of "the scientific community", we need to be able to recognise as such the strategies which, in universes in which people have an interest in being disinterested, tend to disguise strategies. These second-order strategies, through which agents *regularise their situation* by transfiguring submission to laws (which is the precondition of the satisfaction of their interests) into elective obedience to norms, enable them to compound the satisfactions of enlightened self-interest with the profits more or less universally bestowed on actions which apparently have no other determination than pure, disinterested respect for the rule.



3. The structure of the scientific field at any given moment is defined by the state of the power distribution between the protagonists in the struggle (agents or institutions), *i.e.* by the structure of the distribution of the specific capital, the result of previous struggles which is objectified in institutions and dispositions and commands the strategies and objective chances of the different agents or institutions in the present struggles. (Here as elsewhere, one only has to observe the dialectical relationship which is set up between the structures and the strategies — through the intermediary of dispositions — in order to dispose of the antinomy of the synchronic and the diachronic, structure and history, in which structuralist objectivism and spontaneist subjectivism remain trapped.) The structure of the distribution of scientific capital is the source of the transformations of the scientific field through the intermediary of the strategies for conservation or subversion of the structure which the structure itself produces: on the one hand, the position which each individual agent occupies in the structure of the scientific field at any given moment is the resultant, “crystallised” in institutions and dispositions, of the sum of the previous strategies of that agent and his competitors, strategies which themselves depend on the structure of the field through the intermediary of the structural positions from which they originate: and on the other hand, transformations of the structure of the field are the product of strategies for conservation or subversion whose orientation and efficacy are derived from the properties of the positions occupied within the field by those who produce them.

This means that in a given state of the field, researchers’ investments depend both in their amount (measurable, for example, in terms of the time devoted to research) and in their nature (and especially in the degree of risk involved) on the amount of actual and potential recognition-capital which they possess, and on their actual and potential positions in the field (by a circular process which may be observed in every area of practice). In accordance with a logic which has often been observed, researchers’ aspirations — *i.e.* what are generally called “scientific ambitions” — rise as their capital of recognition rises: possession of the capital which the educational system bestows at the very outset of a scientific career, in the form of a prestigious qualification, implies and imposes — through complex mediations — the pursuit of lofty aims which are socially demanded and guaranteed by the qualification. Thus, to attempt to measure the statistical relation between a researcher’s prestige and the prestige of his initial qualification (his *grande école* or faculty in France, the university where he obtained his Ph. D in the USA), *once allowance has been made for the effects of his productivity*<sup>21</sup>, is implicitly to accept the hypothesis that productivity and present prestige are mutually independent and also independent of the initial qualification: in reality, insofar as the qualification, as scholastic capital reconvertible into university and scientific capital, contains a probable trajectory, it governs the agent’s whole relationship with his scientific career (the choice of more or less “ambitious” projects, greater or lesser

productivity, etc.) through the intermediary of the "reasonable aspirations" which it authorises. The consequence is that the prestige of institutions produces its effects not only in a direct way, by "contaminating" judgements passed on the scientific capacities manifested in the quantity and quality of the work done, and in an indirect way, through the intermediary of contact with the most prestigious teachers thanks to prestigious schooling (usually associated with high social class origin), but also through the intermediary of the "causality of the probable", *i.e.* by the force of the aspirations which the objective chances authorise or favour (analogous observations could be made as to the effects of social origin when initial qualifications are equal). For example, the opposition between the risk-free investments of intensive, specialised research, and the hazardous investments of extensive research which may lead to wide-ranging (revolutionary or eclectic) theoretical syntheses — those which, in the case of physics which Fred Reif analyses, involve finding out about scientific developments occurring beyond the strict limits of one's speciality, instead of keeping to the beaten tracks of a tried and tested research direction, and may either lead nowhere or prove a source of fruitful analogies — tends to reproduce the opposition between high-flying and low-flying trajectories in the field of schooling and in the scientific field <sup>22</sup>. In the same way, in order to understand the transformation of scientific practices (one that has frequently been described) which accompanies advance in a scientific career, we must relate the different scientific strategies — *e.g.* massive, extensive investment in research alone, or moderate, intensive investment in research combined with investment in scientific administration — not, of course, to age classes, since each field defines its own laws of social ageing <sup>23</sup>, but to the amount of scientific capital possessed, which by defining at any given moment the objective chances of profit, defines "reasonable" strategies of investment and disinvestment. One sees how artificial it is to describe the generic properties of the different stages in "the scientific career" <sup>24</sup>, even the "average career" in a particular field <sup>25</sup> — because each career is fundamentally defined by its position in the structure of the system of possible careers <sup>26</sup>. There are as many ways of entering, staying in and leaving research, as there are classes of trajectories, and any description dealing with such a universe which limits itself to the generic characteristics of a "typical" career loses sight of the essential point, the *differences*. The decline with age in the quantity and quality of scientific output observed in the case of "average careers", which can apparently be explained if it is admitted that an increase in an agent's capital of consecration tends to reduce the urgency of the high productivity that was needed in order to obtain it, is not fully intelligible until we relate average careers to the highest careers, which alone yield right to the end the symbolic profits that are needed to constantly reactivate the propensity to new investment, thereby constantly delaying disinvestment.

4. The form assumed by the inseparably political and scientific struggle for scientific legitimacy depends on the structure of the field, *i.e.* the structure of the distribution of the specific capital of scientific recognition among those involved in the struggle. This structure can theoretically vary (as in every field) between two theoretical limits, which are in fact never reached — at one extreme, the situation of a monopoly of the specific capital of scientific authority, and at the other, the situation of perfect competition, which would imply equal distribution of this capital among all the competitors. The scientific field is always the locus of a *more or less unequal* struggle between agents unequally endowed with the specific capital, hence unequally equipped to appropriate the product of scientific labour accumulated by previous generations, and the specific profits (and also, in some cases, the external profits such as economic or strictly political benefits) which the aggregate of the competitors produce through their *objective collaboration* by putting to use the aggregate of the available means of scientific production. In every field there is a permanent struggle between forces that are *more or less unequally matched* depending on the structure of the distribution of capital in the field (the degree of homogeneity) — the dominant, who occupy the highest positions in the structure of the distribution of scientific capital, and the dominated, *i.e.* the newcomers to the field, who possess a scientific capital the amount of which (in absolute terms) increases in proportion with the accumulated scientific resources in the field.

Everything seems to indicate that as the accumulated scientific resources increase, and as, owing to the correlative rise in the cost of entry, the degree of homogeneity rises among the competitors (who, as a result of other factors, tend to become more numerous), so scientific competition tends to become very different in its form and intensity from the competition found in earlier states of the same field or in other fields in which there are smaller accumulated resources and less heterogeneity (*cf.* below, part 5). These structural and morphological properties of the various fields are what the sociologists of science generally fail to take into account, thereby running the risk of universalizing the particular case. It is because of these properties that the opposition between strategies for conservation and strategies for subversion (which will be analysed below) tends to weaken with the growing homogeneity of the field and the correlative decline in the likelihood of *great periodic revolutions* in favour of *countless small permanent revolutions*.

In the struggle between the dominant and the newcomers, the two sides resort to antagonistic strategies, profoundly opposed in their logic and their principle: the interests (in both senses of the word) which motivate them and the means they employ in order to satisfy them, depend in fact very closely on their position in the field, *i.e.* on their scientific capital and the power it gives them over the field of scientific production and circulation, and over the profits it produces. The dominant are committed to *conservation strategies* aimed at ensuring the perpetuation of the established scientific order

to which their interests are linked. This order cannot be reduced, as is often thought, to *official science*, the aggregate of the scientific resources inherited from the past which exist in *the state of objectification*, in the form of instruments, texts, institutions, etc., and in *the state of incorporation*, in the form of scientific habitus, systems of generative schemes of perception, appreciation and action, produced by a specific form of educative action, which make possible the choice of objects, the solution of problems, and the evaluation of solutions. It also embraces the aggregate of the institutions responsible for ensuring the production and circulation of scientific goods together with the reproduction of the producers (or reproducers) and consumers of these goods. In the forefront stands the educational system, the only institution capable of securing the permanence and consecration of official science by inculcating it systematically (the scientific habitus) upon all legitimate recipients of educative action, and in particular, upon all new entrants to the actual field of production. In addition to the institutions specifically charged with consecration (academies, prizes, etc.) the established scientific order also includes the instruments of circulation, in particular the scientific journals which, by selecting their articles in terms of the dominant criteria, consecrate productions faithful to the principles of official science, thereby continuously holding out the example of what deserves the name of science, and exercise a *de facto* censorship of heretical productions, either by rejecting them outright or by simply discouraging the intention of even trying to publish them by means of the definition of the publishable which they set forward<sup>27</sup>.

It is the field that assigns each agent his strategies, and the strategy of overturning the established scientific order is no exception to this. Depending on the position they occupy in the structure of the field (and also, no doubt, on secondary variables such as their social trajectory, which governs their assessment of their chances), the "new entrants" may find themselves orientated either towards the risk-free investments of *succession strategies*, which are guaranteed to bring them, at the end of a predictable career, the profits awaiting those who realise the official ideal of scientific excellence through limited innovations within authorised limits; or towards *subversion strategies*, infinitely more costly and more hazardous investments which will not bring them the profits accruing to the holders of the monopoly of scientific legitimacy unless they can achieve a complete redefinition of the principles legitimating domination: newcomers who refuse the beaten tracks cannot "beat the dominant at their own game" unless they make additional, strictly scientific investments from which they cannot expect high profits, at least in the short run, since the whole logic of the system is against them.

On one side, there is invention according to a previously invented art of inventing, which, by solving all the problems likely to be raised within the limits of the established problematic, through the application of proven methods (or by working to save established principles from heretical challenges — one

thinks for example of Tycho Brahe) tends to occlude the fact that it only solves the problems it can raise and only raises the problems it can solve; on the other side, there is heretical invention, which, by challenging the very principles of the old scientific order, creates a radical dichotomy, with no chance of compromise, between two mutually exclusive systems. The founders of a heretical scientific order break the exchange agreement that is accepted, at least tacitly, by candidates for the succession: recognising no other principle of legitimation than the one they intend to impose, they refuse to enter the cycle of the *exchange of recognition* which ensures an orderly transmission of scientific authority between the holders and the pretenders (*i.e.* very often between members of different generations, which leads many observers to reduce conflicts over legitimacy to generation conflicts). Rejecting all the sanctions and guarantees offered by the old order, as well as the (progressive) accession to a share in the collectively guaranteed capital which is effected in accordance with the orderly procedures of a contract of delegation, they achieve their initial accumulation by means of a violent wrench, a sharp break with the existing order, diverting for their own benefit the credit which accrued to the former dominant group, without conceding in exchange the tribute of recognition which those willing to take their place in the continuity of a lineage bestow on their elders<sup>28</sup>.

And there is every reason to think that the propensity to conversion strategies or subversion strategies is that much less independent of dispositions towards the established order when the scientific order is itself less independent of the social order in which it is set. This is why there are grounds for supposing that the relation which Lewis Feuer establishes between the young Einstein's academically and politically subversive leanings and his scientifically revolutionary enterprise is true *a fortiori* in sciences such as biology or sociology which are far from having achieved the degree of autonomy attained by physics in Einstein's time<sup>29</sup>. And the opposition which Feuer establishes between Einstein's youthful revolutionary dispositions, as a member of a group of Jewish students in revolt against the university order and the social order, and the reformist dispositions evinced by Poincaré, a perfect representative of the "republic of professors", a man of order and orderly reform, both in the political and in the scientific order, cannot fail to remind us of the homologous opposition between Marx and Durkheim.

5. What are the social conditions which must be fulfilled in order for a social play of forces to be set up in which the true idea is endowed with strength because those who have a share in it have an interest in truth, instead of having, as in other games, the truth which suits their interests? It goes without saying that it is not a question of making this exceptional social universe an exception to the fundamental laws of all fields — in particular the law of interest, which is capable of introducing ruthless violence into the most "disinterested" scien-

tific struggles ("disinterestedness", as we have seen, never being anything other than a system of specific — artistic or religious, as well as scientific — interests which implies relative indifference to the ordinary objects of interest — money, honours, etc.). The scientific field always includes a measure of social arbitrariness, inasmuch as it serves the interests of those who are in a position, inside or outside the field, to gather in the profits; but this does not prevent the inherent logic of the field, and in particular, the struggle between the dominant and the new entrants, with the resultant cross-control, from bringing about, under certain conditions, a *systematic diversion of ends* whereby the pursuit of private scientific interests (again in both senses of the word) continuously operates to the advantage of the progress of science<sup>30</sup>.

Partial theories of science and its transformations are predisposed to perform ideological functions in the struggles within the scientific field (or within fields laying claim to scientificity, such as the field of the social sciences) because they universalize the properties attached to *particular states* of the scientific field: this is true of the positivist theory which confers on science the power to solve all the questions it raises, provided they are raised scientifically, and to impose a consensus on its solutions by applying objective criteria, thus inserting progress into the routine of "normal science" and implying that science passes from one system to another — from Newton to Einstein, for example — through simple accumulation of knowledge, refinement of measurements and rectification of principles; it is equally true of Kuhn's theory, which, though valid for the beginnings of science (for which the Copernican revolution provides the paradigm — in the true sense of the word), over-simplifies by taking the diametrically opposite position to the positivist model<sup>31</sup>. In reality, the field of astronomy in which the Copernican revolution occurred contrasts with the field of contemporary physics in the same way that, according to Polanyi, the market "embedded in social relationships" of archaic societies contrasts with the "self-regulating market" of capitalist societies. It is no accident that the Copernican revolution implies an express demand for autonomy for a scientific field still "embedded" in the religious field and the field of philosophy, and through them, in the political field; and this demand implies the assertion of scientists' right to decide on scientific questions ("mathematics for the mathematicians") in the name of the specific legitimacy which they derive from their competence.

Until scientific method and the control and/or assistance which it proposes or imposes have been objectified in mechanisms and dispositions, breaks in the continuity of science necessarily take on the aspect of revolutions against the establishment. But once these founding revolutions have excluded all recourse to any weapons or powers, even purely symbolic ones, other than those which are legal tender within the field, it is the operation of the field itself which defines more and more completely not only the ordinary order of "normal science" but also the extra-ordinary breaks, the "orderly revolutions" as Bachelard calls them, which are written into the logic of the his-

tory of science, *i.e.* the logic of scientific polemics <sup>32</sup>. When scientific method is built into the mechanisms of the field, revolution against instituted science is carried out with the aid of an institution which provides the institutional conditions of the break; the field becomes the scene of a permanent revolution, but a revolution that is increasingly devoid of political effects. That is why this universe of permanent revolution can also, without contradiction, be that of "legitimate dogmatism" <sup>33</sup>: the scientific equipment required to effect a scientific revolution can only be acquired in and by the citadel of the scientific establishment. As accumulated scientific resources increase, so the incorporated scientific capital needed in order to appropriate them and thereby gain access to scientific problems and tools, and thus to the scientific struggle, becomes greater and greater (the cost of entry) <sup>34</sup>. The consequence is that scientific revolution is the business not of the poorest but of the richest (in scientific capital) among the new entrants <sup>35</sup>. The antinomy of upheaval and continuity is weakened in a field which makes no distinction between revolutionary phases and "normal science" and which finds the true principle of its continuity in continuous upheaval; and correlatively the opposition between succession strategies and subversion strategies increasingly tends to lose its meaning, since the accumulation of the capital needed to accomplish revolutions, and of the capital accruing from revolutions, increasingly tends to occur in accordance with the regulated procedures of a career <sup>36</sup>.

The transmutation of the anarchic antagonism of particular interests into a scientific dialectic becomes more and more complete as the interest that each producer of symbolic goods has in producing products that, as Fred Reif puts it, are "not only interesting to himself but also important to others", hence likely to win recognition of their importance and of the importance of their author, comes up against competitors more capable of applying the same means in the service of the same intentions — which, with simultaneous discoveries, leads more and more frequently to one or both producers' interests being sacrificed <sup>37</sup>; or, to put it another way, the transmutation becomes more complete as each individual agent's private interest in fighting and dominating his competitors in order to win their recognition comes to be equipped with a whole set of tools which endow his polemical intention with maximum efficacy by giving it the universal scope of methodical control. And indeed, as accumulated resources increase, together with the amount of capital needed in order to appropriate them, so the market in which the scientific product is put on offer increasingly becomes restricted to competitors who are increasingly well equipped to criticize it rationally and to discredit its author. The antagonism which is the basis of the structure and transformation of any field tends to become more and more radical and more and more fruitful because the *forced agreement* in which reason is generated leaves less and less room for the unthought assumptions of doxa. The collective order of science is built up in and through the competitive anarchy of self-interested actions, each agent finding himself dominated — as is the

whole group — by the seemingly incoherent criss-crossing of individual strategies. This means that the opposition between “functional” and “dysfunctional” aspects of the operation of a highly autonomous scientific field has little meaning: the most “dysfunctional” tendencies (e.g. secretiveness and refusal to cooperate) are inherent in the very same functions which generate the most “functional” dispositions. As scientific method takes its place among the social mechanisms regulating the operation of the field, and thereby acquires the higher objectivity of an immanent social law, so it can realise itself objectively in tools capable of controlling and sometimes dominating their users, and in the lastingly constituted dispositions inculcated by schooling. And these dispositions are continuously reinforced by the social mechanisms which, themselves finding support in the rational materialism of objectified, incorporated science, produce both control and censorship, and also innovation and rupture <sup>38</sup>.

6. Science never has any other basis than the collective belief in its bases which is produced and presupposed by the very operation of the scientific field. The objective orchestration of the practical schemes inculcated by explicit instruction and familiarisation, which constitutes the basis of the practical consensus on what is at stake in the field, *i.e.* on the problems, methods and solutions immediately regarded as scientific, is itself based on the whole set of institutional mechanisms which ensure the social and academic selection of the researchers (through, for example, the established hierarchy of the disciplines), the training of the selected agents, control over access to the instruments of research and publication, etc. <sup>39</sup>. The field of argument which orthodoxy and heterodoxy define by their struggles is demarcated against the background of the field of *doxa*, the aggregate of the presuppositions which the antagonists regard as self-evident and outside the area of argument, because they constitute the tacit condition of argument <sup>40</sup>: the censorship exercised by orthodoxy — and denounced by heterodoxy — conceals a more radical censorship which is also harder to detect because it is constitutive of the very functioning of the field, and because it bears on the totality of what is admitted by the mere fact of belonging to the field, and on the totality of what is set beyond discussion by the mere fact that the agents accept the issues at stake in argument, *i.e.* the consensus on the objects of dissensus, the common interests underlying conflicts of interest, all the undiscussed and unthought areas tacitly kept outside the *limits* of the struggle <sup>41</sup>.

Depending on a particular field's degree of autonomy in relation to external determinations, social arbitrariness figures to a greater or lesser extent in the system of presuppositions which constitutes the particular belief characteristic of the field in question. This means that, in the abstract space of theory, any scientific field — that of social science or mathematics nowadays, like that of alchemy or mathematical astronomy in the time of Copernicus — may be situated somewhere between the two limits represented at one



extreme by the religious field (or the field of literary production), in which official truth is nothing other than the legitimate imposition (*i.e.* arbitrary imposition misrecognised as such) of cultural arbitrariness expressing the specific interest of the dominant — inside and outside the field —, and at the other extreme by a scientific field from which every element of social arbitrariness (or unthought assumption) would be banished and the social mechanisms of which would bring about the necessary imposition of the universal norms of reason.

This raises the question of the degree of social arbitrariness of the *belief* which is produced by the functioning of the field and is the condition of its functioning, or, — and this amounts to the same thing — the question of the field's degree of autonomy (in relation, first, to the social demands of the dominant class and the internal and external social conditions of that autonomy). The principle of all the differences between, on one side, scientific fields capable of producing and satisfying a strictly scientific interest and thus maintaining an unending dialectical process, and, on the other side, *learned fields* in which collective labour has no other effect or function than to perpetuate a field identical to itself, by producing, both within the field and outside it, belief in the autonomous value of the objectives and objects which it produces, lies in the relationship of *dependence in the guise of independence* which false science maintains with external demands: the doxosophers, the professors of false science, learned in appearance and learned in appearances, cannot legitimate either the dispossession that they effect by the arbitrary constitution of an esoteric learning inaccessible to the laity, or the delegation that they demand by arrogating to themselves the monopoly of certain practices or of reflexion on those practices, unless they can impose the belief that their false science is perfectly independent of the social demands which it could not satisfy so perfectly if it ceased to proclaim so loudly that it refuses to serve them.

From Heidegger speaking of the masses and the élites in the highly euphemised language of the "authentic" and the "inauthentic", to the American political scientists who reproduce the official vision of the social world in the semi-abstractions of a descriptive-normative discourse, one always encounters the same strategy of *false separation* which defines learned jargon as opposed to scientific language. Where scientific language, as Bachelard points out, uses inverted commas to indicate that the words of ordinary language or of previous scientific language which it retains are completely redefined, and draw their meaning entirely from the new theoretical system<sup>43</sup>, learned language makes use of inverted commas and neologisms so as to symbolically manifest a fictitious distance and separation from common sense: lacking any real autonomy, it cannot in fact produce its full ideological effect unless it remains sufficiently transparent to continue to evoke the ordinary experience and expression which it *denies*<sup>43</sup>.

Strategies of false separation express the objective truth of fields which

have only a false autonomy: whereas the dominant class grants the natural sciences an autonomy corresponding to the interest it finds in the economic applications of scientific techniques, so that they are now (even for the religious consciousness) fully autonomised in relation to the laws of the social world, the dominant class has no reason to expect anything from the social sciences — beyond, at best, a particularly valuable contribution to the legitimisation of the established order and a strengthening of the arsenal of symbolic instruments of domination. The belated and precarious development of the social sciences is evidence that the progress towards real autonomy which is the condition of the establishment of the constitutive mechanisms of a self-regulating, autarkic scientific field necessarily comes up against obstacles not encountered elsewhere; and it cannot be otherwise, because the power which is at stake in the internal struggle for scientific authority within the field of the social sciences, *i.e.* the power to produce, impose and inculcate the legitimate representation of the social world, is one of the things at stake in the struggle between the classes in the political field<sup>44</sup>. It follows that positions in the internal struggle can never attain the degree of independence in relation to positions in the external struggle which is to be found in the natural sciences. The idea of a neutral science is a fiction, an interested fiction which enables its authors to present a version of the dominant representation of the social world, neutralised and euphemised into a particularly misrecognisable and symbolically, therefore, particularly effective form, and to call it scientific<sup>45</sup>. By bringing to light the social mechanisms which ensure the maintenance of the established order and owe their strictly symbolic efficacy to misrecognition of their logic or their effects, the basis of a subtly exacted recognition, social science necessarily takes sides in the political struggle. This means that when it succeeds in getting started (which implies the fulfilment of certain conditions correlative with a particular state of the power relations between the classes), the struggle between genuine science and the false science of the doxosophers (who may claim allegiance with the most revolutionary theoretical traditions) necessarily makes a contribution to the struggle between the classes who, at least in this case, do not have an equal interest in scientific truth<sup>46</sup>.

The fundamental question of the sociology of science assumes a particularly paradoxical form in the case of the social sciences: what are the social conditions of development of a science freed from social constraints and demands, given that, in this case, progress in the direction of scientific rationality does not mean progress in the direction of political neutrality? The question can be denied. It is denied, for example, by all those who impute all the particularities of the social sciences to their situation as the most recent arrivals, in the name of a naively evolutionist philosophy which sets official science at the summit of evolution. In reality, the theory of *backwardness* is, paradoxically, only true in the case of official sociology, and more precisely, the official sociology of sociology. One has only to think of Alexander

Gerschenkron's famous analyses of "economic backwardness" in order to understand the most characteristic features of the particular forms of learned discourse produced by the *false sciences* (would-be science and science-to-be). Gerschenkron points out that when the process of industrialisation starts *late*, it presents systematic differences from the form it assumed in more developed countries, not only in the rate of development but also in the "productive and organisational structures", because it applies new "institutional instruments" and develops in a different ideological climate<sup>47</sup>. The existence of more advanced sciences — major suppliers not only of methods and techniques, which are generally made use of outside the technical and social conditions of their application, but also of examples — is what enables official sociology to furnish itself with all the appearances of scientificity: the outward show of autonomy can here take on an unprecedented form, surpassing the carefully maintained esotericism of the old academic traditions. Official sociology aims not to realise itself as a science but to realise an official image of science (which the sociology of science plays an important part in providing). The official sociology of science, a sort of tribunal which the *community* of official sociologists (the word "community" is perfectly apt here) sets up for itself, has the function not only of providing that community with a justificatory ideology but also, and above all, of imposing on it respect for the norms and models taken from the natural sciences — at the cost of a positivistic reinterpretation.

The first of these functions is most apparent in the social history of social science as practised by the American sociological establishment<sup>48</sup>. Convincing evidence of its function as a justificatory ideology is obtained as soon as one starts to count the number of works directly or indirectly devoted to *competition*, the key term (though used in a highly restrictive sense), in all American sociology of science and a notion whose obscurity as a native concept raised to the dignity of science concentrates all the unthought assumptions (the *doxa*) of that sociology. The thesis that productivity and competition are directly linked<sup>49</sup> is based on a functionalist theory of competition which is a sociological variant of belief in the virtues of the "free market economy". An approach which reduces all competition to competition between universities, or makes competition between universities the precondition of competition among researchers, ignores the question of the obstacles to scientific competition that are imputable to the inseparably *economic and scientific competition* which reigns in the "academic market place". The competition recognised by this establishment science is competition within the limits of orthodoxy, within the forms and norms of intellectual free enterprise: the extent to which this competition within the limits of social acceptability is an obstacle to true scientific competition, which challenges orthodoxy and, whenever it can, *doxa*, rises with the degree of social arbitrariness in the universe in question<sup>50</sup>. It is not hard to see how exaltation of the unanimity of the paradigm can coincide with competition — or how it is possible, depending on the author, to accuse European

sociology both of too much and of too little competition. As an American observer of the British university remarks: "Without intense interpersonal competition with prizes to be won, most scientists simply get on with their research and do not spend a significant part of their time thinking about where they will move next" <sup>51</sup>.

No less evident is the second function, that of supplying the instruments and above all the symbolic attributes of scientific respectability, disguises and cosmetics such as technological gadgetry and rhetorical kitsch. In addition to its tools and techniques — computers and standard data-processing programs for example — official sociology takes over a model of scientific practice as it appears to the positivist imagination, and a model of the organisation of what it calls "the scientific community" as pictured by its rudimentary science of organisations <sup>52</sup>. But official sociology holds no monopoly of interested readings of the history of science: the particular difficulty which sociology has in conceiving *science scientifically* is related to the fact that sociology is situated at the very bottom of the social hierarchy of the sciences. Whether it rises to conceive other more scientific sciences better than they conceive themselves, or descends to record the triumphant image produced and propagated by scientific hagiography, sociology always encounters the same difficulty in conceiving itself, *i.e.* conceiving its own position in the social hierarchy of the sciences. The reactions provoked by Thomas Kuhn's book, *The structure of scientific revolutions*, show this very clearly, and would provide high-quality experimental material for an empirical analysis of the ideologies of science and their relationship with their authors' positions in the scientific field. It is true that this book, which never really makes clear whether it is describing or prescribing the logic of scientific change (an example of implicit prescription: the existence of a paradigm is a sign of scientific maturity), invited its readers to seek answers to the question of good and bad science <sup>53</sup>. Among those whom the native language calls "radicals", Kuhn's book was seen as an invitation to "revolution" against the "paradigm" <sup>54</sup>, or a justification of liberal plurality of *world views* <sup>55</sup> — two positions on the book probably corresponding to different positions within the field <sup>56</sup>. Among the upholders of the established scientific order, it was read as an invitation to drag sociology out of its "pre-paradigmatic" phase by imposing the unified configuration of beliefs, values and techniques symbolised by the Capitoline triad of Parsons and Lazarsfeld reconciled in Merton. The exaltation of quantification, formalisation and ethical neutrality, disdain for philosophy, and rejection of system-building aspirations in favour of meticulous empirical verification and the loose ("operational") conceptualisation of "middle-range theorising" — all flow from a wretchedly transparent transmutation of what is into what ought to be, and find their justification in the need to contribute to the strengthening of "community values", without which sociology could not "get off the ground".

As a false science serving to produce and maintain false consciousness,

official sociology (the finest flower of which is currently political science) has to flaunt its objectivity and "ethical neutrality" (its neutrality in the struggle between the classes, whose existence it moreover denies) and to present all the appearances of a sharp *separation* from the dominant class and its ideological demands, by multiplying the outward signs of scientificity: thus on the "empirical" side we find the ostentatious *display of technology*, and on the "theoretical" side "*neo*" *rhetoric* (thriving in the artistic field too), which apes scientific cumulativeness by applying the typically academic procedure of "re-reading" to a work or set of works, a paradigmatically scholastic operation of simple reproduction which, within the limits of the field and of the belief that the field produces, succeeds in producing all the appearances of "revolution". A systematic analysis is needed of the *rhetoric of scientificity* with which the dominant "community" produces belief in the scientific value of its products and the scientific authority of its members: for example, the whole set of strategies designed to present the *appearances of cumulativeness*, such as reference to canonical sources, generally reduced, as the phrase goes, "to their simplest expression" (consider the posthumous fate of Durkheim's *Suicide*), i.e. banal formalities simulating the rigour of scientific discourse, and to articles, the more recent the better (cf. the opposition between the "hard" and the "soft" sciences); or the *foreclosing strategies*, which are intended to mark a decisive separation between the scientific, problematic and profane, public debates (still present, but only as "ghosts in the machine"), generally by means of simple linguistic retranslations; or the *denial strategies* favoured by political scientists, who are skilful at realising the dominant ideal of "objectivity" in an apolitical discourse on politics, in which repressed politics can only appear in the misrecognisable, hence irreproachable guise of its political-scientific denial<sup>57</sup>. But these strategies perform another essential function: like any circle of legitimacy, this circular circulation of objects, ideas, methods and above all signs of recognition within a community (one should say, a club, open only to native and adopted members of the Ivy League)<sup>58</sup>, produces a universe of belief which has its equivalent both in the religious field and also in the fields of literature or *haute couture*<sup>59</sup>.

But here too, one must be careful not to credit official false science with the significance it is accorded in the "radical" critique. Despite their conflict over the *value* which they attribute to the paradigm, seeing it either as a principle of unification needed for the development of science or as an arbitrary instrument of repression — or as both alternately, in Kuhn's case — the conservatives and their "radical" opponents are objective accomplices who agree on the essential point: from the one-sided points of view which they necessarily adopt on the scientific field, by opting, unconsciously at least, for one or the other of the opposing camps, they are unable to see that control or censorship are not effected by any specific institution but by the *objective relationship between opposing accomplices* who, through their very antagonism, demarcate the field of legitimate argument, excluding as absurd, eclectic,

or simply unthinkable, any attempt to take up an unforeseen position (for example, in this particular case, to use the technical tools created by official science in the service of a different scientific axiomatics) <sup>60</sup>.

“Radical” ideology, a thinly euphemised expression of the interests of those dominated in the scientific field, tends to treat every revolution against the established scientific order as a scientific revolution, behaving as if an “innovation” only had to be rejected by official science in order to be regarded as scientifically revolutionary, and thereby neglecting the question of the particular social conditions under which a revolution against the established scientific order is inseparably a scientific revolution and not a mere heresy intended to reverse the established distribution of power in the field without transforming the principles underlying its functioning <sup>61</sup>. As for the dominant, having made all their investments (economically and psychoanalytically speaking) in the established scientific order, and being in a position to appropriate its profits, they are disposed to accept that it is the realisation of what ought to be and are logically led to the spontaneous philosophy of science which finds its expression in the positivist tradition, a form of liberal optimism which holds that science progresses through the intrinsic strength of the true idea, and that the most “powerful” are also the most “competent” <sup>62</sup>: one only has to think of earlier states of the field of the natural sciences to see the ideological function of sociodicy that is performed by this philosophy of science, which, by presenting the ideal as realised, eliminates the question of the social conditions of the realisation of the ideal.

When it posits that the sociology of science itself functions in accordance with the laws governing the operation of any scientific field, which are established by the scientific sociology of science, the sociology of science in no way condemns itself to relativism. A scientific sociology of science (and the scientific sociology which it helps to make possible) can only be constituted on condition that it is clearly seen that different representations of science correspond to different positions in the scientific field, and that these representations are *ideological strategies* and *epistemological positions* whereby agents occupying a particular position in the field aim to justify their own position and the strategies they use to maintain or improve it, while at the same time discrediting the holders of the opposing position and their strategies. Every sociologist is a good sociologist of his rivals; the sociology of knowledge or of science is no more than the most irreproachable form of the strategies used to disqualify rivals, until it ceases to take as its object the rivals and their strategies and turns its attention to the *complete system of strategies*, i.e. the field of positions within which they are generated <sup>63</sup>. The sociology of science is so difficult only because the sociologist has a stake in the game he undertakes to describe (first, the scientificity of sociology and secondly the scientificity of the form of sociology which he practises) and because he cannot objectify what is at stake, and the corresponding strategies,

unless he takes as his object not simply the strategies of his scientific rivals but the game as such, which governs his own strategies too and is always liable to exert an insidious influence on his sociology.

*Pierre Bourdieu, Professor at the École des Hautes Études en Sciences Sociales, Paris, is head of its Centre de Sociologie Européenne. Among his numerous publications, we particularly wish to mention the following titles related to the theme of the present article: "Une interprétation de la sociologie religieuse de Max Weber", Archives européennes de sociologie, 12 (1), 1971; "Le marché des biens symboliques", L'année sociologique, 22, 1971; "Les doxosophes", Minuit, (1), 1973; "Les fractions de la classe dominante et les modes d'appropriation de l'œuvre d'art", Information sur les sciences sociales, 13 (3), 1974; "Le couturier et sa griffe: contribution à une théorie de la magie", Actes de la recherche en sciences sociales, (1) 1975.*

## Notes

\* This article is an English translation by Richard Nice of "La spécificité du champ scientifique et les conditions sociales du progrès de la raison".

1. Cf. P. Bourdieu, "Une interprétation de la sociologie religieuse de Max Weber", *Archives européennes de sociologie* 12 (1), 1971, pp. 3-21.

2. An excellent example of this is the conflict described by Sapolsky, between the advocates of fluoridation, i.e. the holders of official authority, the "health officials" who regard themselves as the sole competent judges in matters of public health, and the opponents of the project, including many scientists who, in official eyes, had stepped outside "the limits of their own area of competence". The social truth of competence can here be clearly perceived as the right to authorised, authoritative discourse which is at stake in the struggle between groups (cf. H.M. Sapolsky, "Science, voters and the fluoridation controversy", *Science* 162, (3852) 25 October 1968, pp. 427-433). The problem of competence emerges in its acutest and clearest form in the relationship with "laymen" (cf. S.B. Barnes, "On the reception of scientific beliefs", in B. Barnes (ed.), *Sociology of science*, London, Penguin, 1972, pp. 269-291; L. Boltanski et P. Maudidier, "Carrière scientifique, morale scientifique et vulgarisation", *Information sur les sciences sociales* 9 (3), 1970, pp. 99-118).

3. Cf. F. Reif, "The competitive world of the pure scientist", *Science* 134 (3494), 15 December, 1961, pp. 1957-1962.

4. Here is just one example, for those who might doubt the existence and recognition of this hierarchy: "Kuhn's influence on the sociology of science has proved to be so profound that he has all but attained the rank of Merton", P. Weingart, "On a sociological theory of scientific change", in R. Whitley (ed.), *Social processes of scientific development*, London and Boston, Routledge and Kegan Paul, 1974, pp. 45-68.

5. Reif, *op. cit.*

6. It is in this light that one can understand why capital should be transferred from a particular field to a lower field, where less intense competition offers the holder of a determinate scientific capital greater chances of profit.

7. Cf. R.K. Merton, *The sociology of science*, Chicago and London, University of Chicago Press, 1973, p. 55.

8. Countless expressions of this neutralist credo are to be found; here is one particularly representative specimen: "One essential notion of sociologists as professionals — either academically based or practice oriented — is that they are able to separate in a socially responsible fashion, their personal ideology from their professional roles in dealing with their clients, their publics, and their peers. Clearly, this dimension represents the deepest

and most profound issue in the application of the concept of professionalisation to sociology and especially in the period of university activism since 1965 (Ben David, 1972). Many sociologists, since the initial organisation of sociology as a discipline, have had strong personal ideologies which have pressed them to seek to make their knowledge relevant or effective for social change; yet as academics they have had to face or have been attracted to the norms of the teacher-researcher" (M. Janowitz, *American journal of sociology* 78 (1), July 1972, pp. 105-135).

9. W.D. Hagstrom, *The scientific community*, New York, Basic Books, 1965, p. 100.

10. Fred Reif points out that scientists who are so eager to get their work published quickly that they resort to the daily press (like the physicists who announced important discoveries in the *New York Times*) incur the disapproval of their competitor-peers, in the name of the distinction between *publication* and *publicity*; the same distinction lies behind the hostility towards certain forms of popularisation, which are regarded as self-publicisation. We need only quote the comments of the editor of the American physicists' official journal: "As a matter of courtesy to fellow physicists, it is customary for authors to see to it that releases to the public do not occur before the article appears in the scientific journal. Scientific discoveries are not the proper subject for newspaper scoops, and all media of mass communication should have equal opportunity for simultaneous access to the information. In the future we may reject papers whose main content has been published previously in the daily press" (Reif, *loc. cit.*).

11. On this point, see P. Bourdieu, "Le marché des biens symboliques", *L'année sociologique* 22, 1971, pp. 49-126 (the numerous self-references in this text should be seen as a form of shorthand).

12. At any given moment, there is a social hierarchy of the scientific fields — the disciplines — which strongly orientates practices and especially the "choices" of "vocation" — and within each field, there is a social hierarchy of objects and methods of treatment. (On this point, see P. Bourdieu, "Méthode scientifique et hiérarchie sociale des objets", *Actes de la recherche en sciences sociales* 1, 1975, pp. 4-6.)

13. Like "Durkheimian" social philosophy, which describes conflict in terms of marginality, deviance or anomie, this philosophy of science tends to reduce the relations of competition between the dominant and the dominated to relations between a "centre" and a "periphery", reviving the emanatist metaphor dear to Halbwachs of distance from the "hearth" of the central values (cf. for example, J. Ben David, *The scientist's role in society*, Englewood Cliffs, NJ, Prentice-Hall, 1971, and E. Shils, "Center and periphery", in *The logic of personal knowledge: Essays presented to Michael Polanyi on his seventieth birthday*, London, Routledge and Kegan Paul, 1961, pp. 117-130).

14. T. Kuhn, *The structure of scientific revolutions*, Chicago, Ill., University of Chicago Press, 1962, p. 168.

15. Behind the experts' problematics on the relative worth of different university systems there inevitably lurks the question of the optimum conditions for the development of science, and hence the question of the best political system, the American sociologists tending to make American-style "liberal democracy" the precondition of "scientific democracy". Cf. for example R.K. Merton, "Science and technology in a democratic order", *Journal of legal and political sociology* 1, 1942, republished in revised edition, R.K. Merton, *Social theory and social structure*, Glencoe, Ill., Free Press, 1967, pp. 550-561, under the title "Science and democratic social structure"; B. Barber, *Science and the social order*, Glencoe, Ill., Free Press, 1952, pp. 73 and 83.

16. Glaser lists "eponymy, prizes, awards, fellowships, scholarships, honorary memberships and committee work in scientific organizations, editorships, honorary degrees, professorships, chairs, lectureships, consultantships, mention by historians of science, publication, acknowledgements in others' work, evaluations by colleagues" (B.G. Glaser, *Organizational scientists: Their professional careers*, Indianapolis, Bobbs-Merrill, 1964, p. 2). According to Glaser, "average recognition" is shown in the "supervisor's favorable evaluation of the quality of the scientist's current research, and proper credit, through publication and through acknow-



ledgement in publications of others for his contribution to the cumulative knowledge in his field". The high-prestige honours, "awards, prizes, grants, lectureships, professorships, etc.," are the signs of recognition reserved for "great men" (B.G. Glaser "Comparative failure in science", *Science* 143 (3610), March 6, 1961, pp. 1012-1014).

17. This explains researchers' very different strategies in the diffusion of preprints and reprints. It would be easy to show how all the differences observed according to the discipline and age of the researchers or the institution to which they belong can be understood in terms of the very different functions performed by these two forms of scientific communication. Preprints enable the scientist to avoid the usual delays involved in scientific publication, by the rapid diffusion among a small number of readers who are also his most competent competitors, of products which are not protected against fraudulent appropriation but are likely to be improved by being put into circulation. Reprints permit the wider circulation of "patented" products, socially imputed to a particular name, among all the writer's colleagues or all those interested (*cf.* W. Hagstrom, "Factors related to the use of different modes of publishing research in four scientific fields", in C.E. Nelson and D.K. Pollock (eds), *Communication among scientists and engineers*, Lexington, Mass., Heath Lexington Books, 1970).

18. Hence the difficulty that is met with in research on intellectuals, be they scientists or artists, both in the inquiry itself and in publishing the results : if people who spend their lives trying to make a name for themselves are offered *anonymity*, this destroys their principal motivation to take part in an inquiry (*cf.* the model of the literary survey or the interview); if anonymity is not offered, one cannot ask "indiscreet" — *i.e.* objectifying, reductive — questions. The publication of the results raises similar problems, if only because anonymity has the effect of rendering the discourse unintelligible or transparent depending on how well-informed the readers are (all the more so because certain positions may contain only one element, a name).

19. H.A. Zuckerman, "Patterns of name ordering among authors of scientific papers : a study of social symbolism and its ambiguity", *American journal of sociology* 74 (3), November 1968, pp. 276-291.

20. The model set out here explains perfectly — without appealing to any moral determinant — the fact that prize-winning scientists more readily abandon first place after having won their prize, and that their contribution to the prize-winning research is more visibly marked than their share in other collective research.

21. *Cf.* for example L.L. Hargens and W.O. Hagstrom, "Sponsored and contest mobility of American academic scientists", *Sociology of education* 40 (1), Winter 1967, pp. 24-38.

22. *Cf.* P. Bourdieu, L. Boltanski and P. Maledier, "La défense du corps", *Information sur les sciences sociales* 10 (4), 1969, pp. 45-86.

23. Statistical analysis shows, for example, that in past generations as a whole, the age of maximum scientific productivity was between 26 and 30 for chemists, between 30 and 34 for physicists and mathematicians, and between 35 and 39 for bacteriologists, geologists and physiologists (H.C. Lehman, *Age and achievement*, Princeton, NJ, Princeton University Press, 1953).

24. *Cf.* F. Reif and A. Strauss, "The impact of rapid discovery upon the scientist's career", *Social problems* 12 (3), 1965, pp. 297-311. Systematic comparison of this article — for which the physicist collaborated with the sociologist — with the article that the physicist wrote a few years earlier would cast a great deal of light on the functioning of American sociological thought. I shall do no more than point out that the price of "conceptualisation" (*i.e.* the translation of naive native concepts into official jargon) is the total disappearance of any reference to the field as a whole, and in particular, to the *system of trajectories* (or careers) from which each career derives its most important properties.

25. *Cf.* B.G. Glaser, "Variations in the importance of recognition in scientist's careers", *Social problems* 10 (3), Winter 1963, pp. 268-276.

26. Rather than repeat here the full demonstration, I shall simply refer the reader to

P. Bourdieu, "Les catégories de l'entendement professoral", *Actes de la recherche en sciences sociales* (3), 1975, pp. 68-93.

27. On the "filtering" action of social science journal editorial committees, see D. Crane, "The gate-keepers of science: some factors affecting the selection of articles for scientific journals", *American sociologist* 73 (2), 1967, pp. 195-201. There is every reason to think that, in scientific as in literary production, authors consciously or unconsciously choose places of publication on the basis of what they take to be their "norms". Self-disqualification, which is naturally less perceptible, is probably at least as important a factor as overt elimination (quite apart from the effect of imposing a norm for publishable material).

28. The novel form which the orderly transmission of scientific capital takes on in fields like that of modern physics, in which conservation and subversion are virtually indistinguishable, is discussed below.

29. "Einstein's high interval of original thought was sustained by a strange little circle of young intellectuals, filled with emotions of social and scientific generational rebellion, a counter-community of scientists outside the official scientific establishment, a group of cosmopolitan bohemians, moved in a revolutionary time to see the world in a new way" (L.S. Feuer, "The social roots of Einstein's theory of relativity", *Annals of science* 27 (3), September 1971, pp. 277-298 and 27 (4), December 1971, pp. 313-344). Transcending the naive opposition between individual habitus and the social conditions in which they are realised, Feuer suggests the hypothesis, corroborated by recent work on the science education system in France (cf. M. de Saint Martin, *Les fonctions sociales de l'enseignement scientifique*, Paris-La Haye, Mouton 1971, Coll. : Cahiers du Centre de sociologie européenne, n° 8, and P. Bourdieu and M. de Saint Martin, *Le système des grandes écoles et la reproduction de la classe dominante*, forthcoming), that the rapid and easy access to administrative responsibilities which was available in France for pupils of the science *grandes écoles* tended to discourage revolutionary dispositions, whereas they flourish among groups of marginal intellectuals halfway between the educational system and the revolutionary bohemian community: "One might indeed venture the hypothesis that precisely because France was a 'republic' of professors, precisely because the ablest talent of the École Polytechnique was promptly absorbed into the military and engineering cadres, that it was less likely that a very fundamental break with the received principles would take place there. A scientific revolution evidently finds its most fertile soil in a counter-community. Where administrative responsibilities soon beckoned the young scientist, his energies were less available for sublimation in radical research curiosity. As far as revolutionary creativity was concerned, the very openness of the French administration to scientific talent was perhaps more important for explaining its scientific conservatism than other factors that have usually been emphasized."

30. It is a mechanism of this sort which tends to regulate relations with the external universe, the world of "laymen", i.e. "scientific popularisation", the scientist's self-publicisation (cf. Boltanski and Maldidier, *op. cit.*).

31. There is no doubt that the philosophy of the history of science offered by Kuhn, with its alternation of monopolistic concentration (the paradigm) and revolution, owes a great deal to the particular case of the "Copernician revolution" which he analyses and considers "typical of any major upheaval in science" (T. Kuhn, *The Copernician revolution*, New York, Vintage Books, 1957): when science still has relatively little autonomy in relation to social power and especially the church, scientific revolution (in mathematical astronomy) involves political revolution and implies a revolution in all scientific disciplines, which may have political effects.

32. As well as Bachelard and Reif (already quoted), D. Bloor has seen that transformations in the social organisation of science have determined a transformation of the nature of scientific revolutions (cf. D. Bloor, "Essay review: Two paradigms for scientific knowledge?", *Science studies* 1, 1971, pp. 101-115).

33. G. Bachelard, *Le matérialisme rationnel*, Paris, Presses Universitaires de France, 1953, p. 41.

34. The principal control is constituted by this entrance fee, *i.e.* by the conditions of access to the scientific field and to the educational system which give access to it. A question which deserves attention is that of the properties which the natural sciences (not to mention the social sciences, in which the limitations of the methods give free rein to habitus) owe to their social recruitment, *i.e.*, roughly speaking, to the conditions of access to higher education (*cf.* Saint Martin, *op. cit.*).

35. We know that the *inaugural revolutions* which give rise to a new field by making a break which sets up a new domain of objectivity, are themselves almost always the work of the holders of a large specific capital who, as a result of secondary variables (such as belonging to a social class or ethnic group improbable in that universe), find themselves in an unstable position which favours revolutionary inclinations: such is the case, for example, with new entrants who bring into the field capital they have accumulated in a socially superior scientific field (*cf.* J. BenDavid, "Roles and innovation in medicine, *American journal of sociology* 65, 1960, pp. 557-568; J. BenDavid and R. Collins, "Social factors in the origins of a new science: the case of psychology", *American sociological review* 31, 1966, pp. 451-465).

36. We have already seen Fred Reif's description of the most usual form of capital accumulation in such a state of the field.

37. It is generally agreed that disputes over priority become more and more frequent as the scientific struggle becomes more and more intense (despite the effect of a continuous differentiation of the field, which limits the competitors' universe) *i.e.*, more precisely, as accumulated scientific resources grow and the capital needed in order to innovate becomes more widely and more uniformly spread among the competitors, as a result of the rise in the *cost of entry* to the field.

38. All the processes accompanying the autonomisation of the scientific field are in dialectical relationship: thus the constant raising of the cost of entry implied in accumulation of the specific resources contributes in return to the autonomisation of the scientific field by setting up a social separation from the profane world of laymen, a separation made all the more radical by the fact that it is not sought for its own sake.

39. The habitus produced by class upbringing in the earliest years of life and the secondary habitus inculcated by schooling play a part (of differing importance in the case of the social sciences and that of the natural sciences) in determining a pre-reflexive adherence to the tacit pre-suppositions of the field (on the role of socialisation, see Hagstrom, *op. cit.*, p. 9, and T.S. Kuhn, "The function of dogma in scientific research", in A.C. Crombie (ed.), *Scientific change*, London, Heineman, 1963, pp. 347-369).

40. One sees what ethnomethodology might become (but would it still be ethnomethodology?) if it realised that what it takes as its object, Schutz's "taken for granted", is pre-reflexive adherence to the established order.

41. In the case of the field of ideological production (to which the field of the social sciences still belongs) the basis of the consensus in dissensus which defines doxa lies, as we shall see, in the censored relationship of the field of production as a whole with the field of power (*i.e.* the field's hidden function in the class struggle).

42. Bachelard, *op. cit.*, pp. 216-217.

43. The rhetoric of false separation is the object of two analyses now being prepared, one on the philosophy of Heidegger and the other on political science.

44. This is why social systems of classification (taxonomies), which are one of the issues at stake in the ideological struggle between classes (*cf.* P. Bourdieu and L. Boltanski, "Le titre et le poste: rapports entre le système de production et le système de reproduction", *Actes de la recherche en sciences sociales* 2, 1975, pp. 95-107) also constitute — through the positions adopted as to the existence or non-existence of social classes — one of the major principles of division of the sociological field (*cf.* P. Bourdieu, "Classes et classements", *Minuit* 5, 1973, pp. 22-24, and A.P.A. Coxon and C.L. Jones, *Occupational categorization and images of society*, Project on occupational cognition, Working Paper n° 4, Edinburgh University Press, 1974).

45. It follows that the sociology of science (and in particular the sociology of the relationship between social science and the dominant class) is not a speciality among others but one of the conditions of a scientific sociology.

46. Cf. P. Bourdieu, "La théorie", *VH* 1012, Summer 1970, pp. 13-21.

47. A. Gerschenkron, *Economic backwardness in historical perspective*, Cambridge, Harvard University Press, 1962, p. 7.

48. The philosophy of history pervading this social history of social science achieves paradigmatic expression in Terry Clark's book, which Paul Vogt sociologically characterises in two adjectives: "Terry N. Clark's long-awaited, much circulated in manuscript, *Prophets and patrons*" (cf. T. Clark, *Prophets and patrons, The French university and the emergence of the social sciences*, Cambridge, Mass., Harvard University Press, 1973, and J.C. Chamboredon, "Sociologie de la sociologie et intérêts sociaux des sociologues", *Actes de la recherche en sciences sociales*, 2, 1975, pp. 2-17).

49. J. Ben David deserves credit for presenting this thesis in its most direct form: for him the high degree of competition which characterises the American university explains its higher scientific productivity and greater flexibility (J. BenDavid, "Scientific productivity and academic organization in nineteenth century medicine", *American sociological review* 25, 1960, pp. 828-843; *Fundamental research and the universities*, Paris, OCDE, 1968; J. Ben David and A. Zloczower, "Universities and academic systems in modern societies", *European journal of sociology* 3, 1962, pp. 45-84).

50. And correlatively, this soft theory of competition, shared by all American writers, constitutes the subtlest obstacle to the construction of the scientific field as such, *i.e.* as the locus of a struggle.

51. J. Gaston "The reward system in British science", *American sociological review* 35 (4), August 1970.

52. On the distortion to which sociological methodology subjects the epistemological reality of scientific practice in the natural sciences, see P. Bourdieu, J.C. Chamboredon and J.C. Passeron, *Le métier de sociologue*, Paris, Mouton-Bordas, 1968, 430 p.

53. Even more than in this book — whose theses contain little that is radically new, at least for readers of Bachelard, who was the object of similar manoeuvres at much the same time, in a different tradition — Kuhn's normative intention comes out in two articles in which he describes the positive functions of "convergent" thought for scientific development, and maintains that dogmatic adherence to a tradition is conducive to research (T. Kuhn, "The function of dogma in scientific research" in Crombie (ed.), *op. cit.*, pp. 347-369; "The essential tension: tradition and innovation in scientific research", in L. Hudson (ed.), *The ecology of human intelligence*, London, Penguin, 1970, pp. 342-359).

54. Cf. for example, R.W. Friedrichs, *A sociology of sociology*, New York, Free Press, 1970.

55. E. Gellner, "Myth, ideology and revolution", in B. Crick and W.A. Robson (ed.), *Protest and discontent*, London, Penguin, 1970, pp. 204-220.

56. The purely social importance of a journal like *Theory and society*, which allows it to remain in existence without any substantive content beyond the sort of vague anti-positivist humanism with which "critical sociologists" (another native concept) identify, lies in the fact that it gives a *strictly negative unity* to all the currents outside the American sociological establishment, from ethnomethodology, the heir of phenomenology, to neo-Marxism, via psychohistory. (A fairly accurate synoptic table of this constellation is to be found in P. Bandyapadhyay, "One sociology or many : some issues in radical sociology", *Sociological review* 19, February 1971, pp. 5-30.)

57. Cf. P. Bourdieu, "Les doxosophes", *Minuit* 1, 1973, pp. 26-45 (especially the analysis of the Lipset effect).

58. The official sociology of science offers a justification for each of these features. For example, avoidance of the fundamental theoretical problems finds its justification in the notion that in the natural sciences, researchers are not concerned with the philosophy of science (cf. Hagstrom, *op. cit.*, pp. 277-279). It is not hard to see how much this sociology of science

owes to the need to legitimate a *de facto* situation and transform unavoidable limits into elective exclusions.

59. On the production of belief and fetishism in the field of *haute couture*, see P. Bourdieu and Y. Delsaut, "Le couturier et sa griffe : contribution à une théorie de la magie", *Actes de la recherche en sciences sociales* 1, Janvier 1975, pp. 7-36.

60. Such epistemological couples, which are simultaneously sociological couples, may be observed in any field (*cf.* for example the *Positivismusstreit* between Habermas and Popper in Germany — a diversionary mechanism which, having proved its effectiveness in Europe, is now beginning to take hold in the USA with the importing of the Frankfurt school).

61. An analysis is needed of all the strategic uses which those dominant in a particular field may make of the ideological transfiguration of their objective position: for example, the *ostentatious display of exclusion* which enables the excluded to continue to make use of the institution (which they recognise sufficiently to reproach it for not having recognised them) while making exclusion a guarantee of scientific status; or the challenge to the "competence" of the dominant which is at the centre of every heretical movement (*cf.* the contesting of the monopoly of the sacraments) which has that much less need of scientific arguments when there is less accumulated scientific authority, etc.

62. A proposition which is true sociologically, but is then reduced to a mere tautology, "competence" denoting nothing other than socially recognised authority, that of the Sorbonne theologian in the Middle Ages or the Nobel Prize-winning physicist nowadays.

63. On the need to construct the intellectual field as such, so as to make possible a sociology of the intellectuals which would be something more than an exchange of insults and anathemas between "right-wing intellectuals" and "left-wing intellectuals", see P. Bourdieu, "Les fractions de la classe dominante et les modes d'appropriation de l'œuvre d'art", *Information sur les sciences sociales* 13 (3), 1974, pp. 7-32.