I

The state of the question

One cannot talk about such an object without exposing oneself to a permanent mirror effect: every word that can be uttered about scientific practice can be turned back on the person who utters it. This echo, this reflexivity, is not reducible to the reflexion on itself of an 'I think' (*cogito*) thinking an object (*cogitatum*) that is nothing other than itself. It is the image sent back to a knowing subject by other knowing subjects equipped with analytical tools which may have been provided to them by this knowing subject. Far from fearing this mirror – or boomerang – effect, in taking science as the object of my analysis I am deliberately aiming to expose myself, and all those who write about the social world, to a generalized reflexivity. One of my aims is to provide cognitive tools that can be turned back on the subject of the cognition, not in order to discredit scientific knowledge, but rather to check and strengthen it. Sociology, which invites the other sciences to address the question of their social foundations, cannot exempt itself from this calling into question. Casting an ironic gaze on the social world, a gaze which unveils, unmasks, brings to light what is hidden, it cannot avoid casting this gaze on itself – with the intention not of destroying sociology but rather of serving it, using the sociology of sociology in order to make a better sociology.

I will not conceal from you that I am myself somewhat daunted at having embarked on the sociological analysis of science, a particularly difficult object for several reasons. First, the sociology of science is an area that has grown enormously, at least in quantitative terms, over the last few years. This creates an initial difficulty, one of documentation, which a specialist describes very well: 'Although the social study of science is still a relatively small field, I cannot pretend to cover the entire literature. As is the case for other scholarly fields, the production of writing far outstrips anyone's ability to read a substantial portion of it. Fortunately, there is sufficient duplication, at least at a programmatic level, to enable a reader to gain a fairly confident grasp of the literature and its divisions without having to read all of it' (Lynch 1993: 83). The difficulty is compounded for someone who has not totally and exclusively devoted himself to the sociology of science. [Parenthesis: one of the major strategic choices as regards scientific investments, or, more precisely, the allocation of the finite temporal resources available to each researcher, is the choice between the intensive and the extensive – even if, as I believe, it is possible to do research that is both extensive and intensive, in particular thanks to the intensified productive efficiency that is obtained by the use of models such as that of the field, which enables one to import generic findings into each particular study, to notice specific features and to escape the ghetto effect which threatens researchers confined within a narrow specialty, such as art historians, who, as I showed last year, are often unaware of the findings of the history of education or even literary history.]

But this is not all. We are trying to understand a very complex practice (made up of problems, formulae, instruments, etc.) which can only really be mastered through a long apprenticeship. I know that some 'lab ethnographers' may turn this handicap into a privilege, convert the shortcoming into an accomplishment, and transform the outsider's situation into a deliberate 'approach', while giving themselves the air of ethnographers. On the other hand, it is not necessarily the case that the science of science is better when it is done by the 'half-pay officers' of science, defrocked scientists who have left science to go in for the sociology of science and who may have scores to settle with the science that has excluded or insufficiently recognized them: they may have the specific competence, but they do not necessarily have the posture required for the scientific implementation of that competence. In fact the solution to the problem (that of combining a very advanced technical, scientific competence – that of the cutting-edge researcher who does not have time for self-analysis – with the equally very advanced analytical competence associated with the dispositions needed to apply it in the service of a sociological analysis of scientific practice) cannot, short of a miracle, be found in and by one person alone. It no doubt lies in the construction of scientific collectives – which would presuppose that the conditions be fulfilled in order for researchers and analysts to have an interest in working together and to take the time to do it. We are clearly here in the order of utopias, since, as often in the social sciences, the obstacles to the progress of science are fundamentally social.

A further obstacle is the fact that, like the epistemologists (but less so), the most subtle analysts depend on documents (they work on archives, texts) and on what scientists say about scientific practice, and these scientists themselves depend to a large extent on the philosophy of science of the day or of an earlier period (being, like every acting agent, partially dispossessed of mastery of their own practice, they may unwittingly reproduce the sometimes inadequate or outdated epistemological or philosophical discourses with which they need to arm themselves in order to communicate their experience and to which they thereby lend their own authority).

A final and very significant difficulty is that science, and especially the legitimacy of science and the legitimate use of science, are, at every moment, at stake in struggles within the social world and even within the world of science. It follows that what is called epistemology is always in danger of being no more than a form of *justificatory discourse serving to justify science* or a particular position in the scientific field, or a spuriously neutralized reproduction of the dominant discourse of science about itself.

But I must set out explicitly why I shall start the sociology of the sociology of science that I want to outline with a social history of the sociology of science, and how I conceive such a history. Sketching this history will be a way of giving you an idea of the current state of the questions that arise in relation to science within the universe of research on science (mastery of this problematic being the real condition of entry into a scientific universe). Through it I hope to enable you to apprehend the space of positions and position-takings within which I position myself (and so give you an equivalent for that sense of the problems that characterizes the researcher engaged in the game, for whom, from the interrelationship between the various position-takings – '-isms', methods, etc. – inscribed in the field, the problematic emerges as a space of 'possibles' and as the principle of strategic choices and scientific investments). It seems to me that the space of the sociology of science is currently fairly well marked out by the three positions that I am going to examine.

In outlining such a history, one can opt either to stress the differences, the conflicts (the logic of academic institutions helps to perpetuate false alternatives), or to stress the common points, to integrate, with a practical intention of cumulation. [Reflexivity inclines one to an integrative position which consists in bracketing-off in particular what the opposing theories may owe to the fictitious pursuit of difference: perhaps the best that can be derived from a history of their conflicts – of which one has to take note – is a vision which dissolves a large part of the conflicts, in the manner of philosophers like Wittgenstein, who have devoted much of their careers to destroying false problems – false problems socially constituted as real ones, especially by the philosophical tradition, and consequently very difficult to destroy. While doing so, as a sociologist one knows that it is not sufficient to show or even to demonstrate that a problem is a false problem in order to have done with it.] I shall therefore take the risk of presenting a vision of the various competing theories which will no doubt not be very 'academic', in other words not entirely in conformity with the canons of the scholastic summary; and, out of a concern to comply with the 'principle of charity' or, rather, generosity, but also to emphasize, in each case, what seems to me 'interesting' (from my standpoint, that is to say, in my particular vision of science), I shall lay stress on the theoretical or empirical *contributions* they have made – with, of course, the ulterior motive of integrating them into my own construction. So I am very conscious of presenting them in the form of free interpretations, or oriented reinterpretations, which at least have the merit of presenting the *problematic* as it appears to me, the space of possibles in relation to which I shall determine my own position.

The field of the disciplines and agents that take science as their object – philosophy of science, epistemology, history of science, sociology of science – a field with ill-defined frontiers, is criss-crossed by controversies and conflicts which, surprisingly, illustrate in an exemplary way the best analyses of scientific controversies put forward by the sociologists of science (bearing witness to the weak reflexivity of this universe, which might have been expected to use its own gains to monitor itself). No doubt because it is presumed to address ultimate problems and to situate itself in the order of the 'meta', of the reflexive, in other words at the pinnacle or foundation, it is dominated by philosophy, whose aspirations to grandeur it borrows or mimics (particularly through the rhetoric of the 'discourse of importance'). The sociologists and, to a lesser extent, the historians who are engaged in it remain turned towards philosophy (David Bloor fights under the flag of Wittgenstein, even if secondarily he quotes Durkheim; others call themselves philosophers; and the intended audience is always, visibly, that of philosophers); old philosophical problems are reactivated within it, such as that of idealism and realism (one of the major debates around David Bloor and Barry Barnes is about whether they are realists or idealists), or that of dogmatism and scepticism.

Another feature of this field is that relatively few empirical data are handled or demanded there, and these are generally reduced to texts, which are often drowned in interminable 'theoretical' discussions. A further characteristic of this hybrid region where all sociologists are philosophers and all philosophers are sociologists, where the (French) philosophers who concern themselves with the social sciences mingle and merge with the indeterminate devotees of the new sciences, 'cultural studies' or 'minority studies', who recklessly plunder and borrow from (French) philosophy and the social sciences, is that it is very undemanding as regards rigour in argumentation (I am thinking of the polemics around Bloor as described by Gingras (2000) and in particular the fairly systematic recourse to dishonest strategies of 'disinformation' or defamation – such as use of the label 'Marxist', a deadly but strictly political weapon, to describe someone who, like Barnes, claims allegiance with Durkheim and Mauss, or so many others; or the tendency to shift position according to the context, the interlocutor or the situation).

In recent times, the subfield of the new sociology of science (the universe mapped out by the volume edited by Pickering, *Science as Practice and Culture*, 1992) has been constituted through a series of ostentatious breaks. There has been much critique of the 'old' sociology of science. To take just one example among hundreds, Michael Lynch (1993) entitles one of his chapters: 'The demise of the "old" sociology of science'. [It is worth reflecting on such use of the opposition old / new, which is doubtless one of the obstacles to the progress of science, especially social science: sociology suffers greatly from the fact that the pursuit of distinction at any price, which prevails in certain states of the literary field, encourages an artificial emphasis on differences and prevents or delays the initial accumulation in a common paradigm – everything endlessly restarts from zero – and the establishment of strong, stable models. This is seen in particular in the use made of Kuhn's concept of the paradigm: any sociologist who feels so inclined will declare himself the bearer of a 'new paradigm', a 'new' ultimate theory of the world.] Cut off from the other specialties by a series of breaks which tend to turn it in on its own debates, traversed by countless conflicts, controversies and rivalries, this subfield is driven by the logic of supersession, of outflanking, the pursuit of an ever greater 'depth' ('deeper, more fundamental questions remain unanswered' – Woolgar 1988b: 98). Woolgar, a relativist reflexivist, endlessly refers to the inescapable, un-bypassable 'Problem' which even reflexivity cannot overcome (Collins and Yearley 1992: 307–8).

But is it legitimate to speak of a field with reference to this universe? It is certain that a number of the features I have described can be understood as field effects. For example, the fact that the irruption of the new sociology of science has had the effect, as is observed in every field, of modifying the rules of profit distribution throughout this whole universe: when it appeared that what is important and interesting is not to study scientists (the statistical relations between the properties of scientists and the success accorded to their productions), as the Mertonians do, but science, or more precisely science in progress and laboratory life, all those whose capital was linked to the old way of doing the sociology of science suffered symbolic bankruptcy and their work was relegated to the superseded past, the archaic.

It is clear that it is not easy to construct the history of the sociology of science, not only because of the vast volume of 'literature' but also because this is a field in which the history of the discipline is a stake (among others) in struggles. Each of the protagonists develops a vision of this history consistent with the interests linked to the position he occupies within the history; the different historical accounts are oriented according to the position of their producer and cannot claim the status of indisputable truth. One sees, in passing, one of the effects of reflexivity: what I have just said puts my listeners on their guard against what I am going to say, and puts me on my guard too, against the danger of privileging one orientation or against even the temptation to see myself as objective on the grounds for example that I am equally critical of all positions.

The history that I shall relate here is not inspired by the concern to aggrandize the person who delivers it by leading up by stages to the ultimate solution, capable of combining the gains in a purely additive way (in accordance with the kind of spontaneous Hegelianism that is much practised in the logic of lectures . . .). It simply aims to identify and enumerate the gains – problems as much as solutions – that have to be integrated. For each of the 'moments' of the sociology of science that I distinguish (and which partially overlap), I shall try to establish on the one hand the 'cognitive style' of the current in question, and on the other hand its relationship with the historical conditions, the mood of the time.

1 An enchanted vision

The structural-functionalist tradition in the sociology of science is important in its own right, through its contributions to our knowledge of the scientific field, but also because the – now socially dominant – 'new sociology of science' has been constructed in relation to it. Although it makes many concessions to the official vision of science, this sociology does, all the same, break with the official vision of the American epistemologists: it is attentive to the contingent aspect of scientific practice (which scientists themselves may articulate in certain conditions). The Mertonians put forward a coherent description of science, which, for them, is characterized by universalism, communism or communalism (property rights are limited to the esteem or prestige linked to the fact of giving one's name to phenomena, theories, proofs, units of measurement – Heisenberg's principle, Gödel's theorem, volts, curies, roentgens, Tourette's syndrome, etc.), disinterestedness and organized scepticism. [This description is close to Weber's description of the ideal type of bureaucracy: universalism, specialized competence, impersonality and collective ownership of the function, institutionalization of meritocratic norms to regulate competition (Merton 1957a).]

Mertonian sociology of science, which (unlike the new sociology of science) is inseparable from a general theory, substitutes for a Mannheimian sociology of knowledge a sociology of researchers and scientific institutions conceived in a structural-functionalist perspective which also applies to other domains of the social world. To give a more concrete idea of the 'style' of this research, I would like to comment briefly on an article typical of Mertonian production, a quite remarkable and still valid article which needs to be integrated into the capital of gains made by the sub-discipline (Cole and Cole 1967). In the title ('Scientific output and recognition: a study in the operation of the reward system in science'), the word *recognition*, a Mertonian concept, is an express declaration of membership of a school; in their first footnote, the authors thank Merton for his 'helpful suggestions' on their work, which was financed by an institution controlled by Merton – so many social signs that this is a school united by a socially instituted cognitive style, backed by an institution. The problem addressed is a canonical one within a tradition: the next note refers to previous studies on the social factors of scientific success. Having established a correlation between quantity of publication and indices of recognition, the authors ask whether the best measure of scientific excellence is quantity or quality of output. They therefore study the relationship between the quantitative and qualitative production of 120 physicists (giving a detailed account of each moment of the methodological procedure, the sample, etc.): there is indeed a correlation, but some physicists publish many articles of little 'significance' and others a small number of articles of great 'significance'. The article enumerates the 'forms of recognition': 'honorific awards and memberships in honorific societies', medals, Nobel prizes, etc.; positions at 'top ranked departments'; citations as indices of the use made of the research by others and 'the attention the research receives from the community' (science is accepted as it presents itself). The authors test the correlations statistically (noting in passing that Nobel prize winners are much cited).

This research takes the indices of recognition, such as citation, at face value, and everything takes place as if the statistical inquiries aimed to verify that the distribution of 'rewards' is perfectly justified. This typically structural-functionalist vision is inscribed in the notion of the 'reward system' as defined by Merton: 'the institution of science has developed an elaborate system for allocating rewards to those who variously live up to its norms' (1957b: 642). 'When the institution of science works efficiently. . . recognition and esteem accrue to those who have best fulfilled their roles, to those who have made genuinely original contributions to the stock of knowledge' (1957b: 639). The scientific world offers a system of rewards which fulfils functions that are useful, even necessary (Merton goes on to refer to 'reinforcement by early rewards' to deserving scientists) to the functioning of the whole. [It can be seen in passing that, contrary to what some of my critics claim – I shall return to this – the replacement of 'recognition' by 'symbolic capital' is not a mere more or less gratuitous change of vocabulary, or inspired by the sheer pursuit of originality, but points to a different vision of the scientific world. Structural functionalism sees the scientific world as a 'community' which has 'developed' for itself just and legitimate regulatory institutions and where there are no struggles – or at least, no struggles over what is at stake in the struggles.]

Structural functionalism thus reveals its true nature as a collective finalism: the 'scientific community' is one of those collectives which accomplish their ends through subjectless mechanisms oriented towards ends favourable to the subjects, or at least to the best of them. 'It appears that the reward system in physics operates to give all three kinds of recognition primarily to *significant* research' (Cole and Cole 1967: 387). If the big producers publish the most important research, this is because 'the reward system operates in such a way as to encourage the creative scientists to be productive and to divert the energies of less creative scientists into other channels' (Cole and Cole 1967: 388). The reward system orients the most productive towards the most productive channels and the wisdom of the system which rewards those who deserve reward diverts the others into sidetracks such as administrative careers. [This is a secondary effect whose implications ought to be considered, especially as regards scientific productivity and equity in evaluation, and with a view to verifying whether they are really 'functional', and for whom . . . One would need to consider, for example, the consequences of giving positions of authority, whether in running laboratories or in scientific administration, to second-rank researchers who, because they lack the scientific vision and the 'charismatic' dispositions needed to mobilize people's energies, often help to reinforce the forces of inertia in the scientific world.] The more that scientists are recognized (first by the educational system, then by the scientific world), the more productive they are and continue to be. The most consecrated researchers are those who were consecrated early, the 'early starters' who, thanks to their scholastic consecration, enjoy a rapid early career – appointment as assistant professor in a prestigious department for example (and 'late bloomers' are rarities). [One sees here an application of a general law of the functioning of scientific fields. Systems of selection (such as elite schools) favour great scientific careers – in two ways: first by designating those whom they select as remarkable for others and also for themselves, thus summoning them to make themselves remarked through remarkable actions, especially in the eyes of those who have remarked them (this is the concern to live up to expectations: *noblesse oblige*); on the other hand, by conferring a particular competence.]

This approach – very objectivist, very realist (it is not questioned that the social world exists, that science exists, etc.), very classical (the most classic instruments of scientific method are brought into play) – does not make the slightest reference to the way in which scientific conflicts are settled. It accepts, in fact, the dominant – logicist – definition of science, to which it seeks to conform (even if it somewhat maltreats this paradigm). This having been said, it has the merit of bringing to light things which cannot be seen on the scale of the laboratory. This sociology of science, a central element in a whole apparatus aimed at constituting social science as a *profession*, is inspired by the intention of a 'self-vindication' of sociology on the basis of the cognitive consensus (moreover empirically verified by the school's work in the sociology of science). I am thinking in particular of the article by Cole and Zuckerman, 'The emergence of a scientific speciality: the self-exemplifying case of the sociology of science' (1975).

[It has appeared to me retrospectively that I was somewhat unfair to Merton in my early writings in the sociology of science – no doubt under the effect of the position I then occupied, that of a newcomer in an international field dominated by Merton and structural functionalism. On the one hand I have reread the texts in a different way, on the other I have learned things about the conditions in which they were produced of which I was unaware at the time. For example, the text entitled 'The normative structure of science', which became chapter 13 of *Sociology of Science*, was first published in 1942 in a short-lived journal founded and edited by Georges Gurvitch, then a refugee in the USA: the naïvely idealistic tone of the text, which exalts democracy, science, etc., can be understood better in this context as a way of setting the scientific ideal in opposition to barbarism. I also think I was wrong to lump together with Parsons and Lazarsfeld the Merton who reintroduced Durkheim, who studied the history of science and who rejected both concept-less empiricism and data-less theoreticism, even if his attempt to escape from this choice led him into syncretism rather than a real supersession.

A remark in passing: when one is young – this is elementary sociology of science – other things being equal, one has less capital, and also less competence, and so, almost by definition, one is inclined to put oneself forward in opposition to the established figures, and therefore to look critically at their work. But this critique can in part be an effect of ignorance. In Merton's case, I was unaware not only of the context of his early writings, as I have described it, but also of his trajectory: the man I had seen – in an international conference where he was king – as an elegant, refined Wasp was in reality a recent Jewish immigrant who exaggerated an adopted 'British' elegance (in contrast to Homans, a pure product of New England, who struck me, at a dinner at Harvard, as without any mark of aristocracy – no doubt an effect of the ignorance of an outsider who is unable to recognize a certain relaxed casualness as the sign of 'real distinction'); and that disposition towards hypercorrectness, very common in first-generation immigrants undergoing integration and eager for recognition, was probably also at the root of his scientific practice and his exaltation of the 'profession' of sociology, which he wanted to establish as a scientific profession.

One sees there, it seems to me, the whole interest of the sociology of sociology: the dispositions that Merton imported into his scientific practice were at the root of his insights and his oversights – against which a true reflexive sociology could have protected him; and when one has seen this, one gains access to the ethico-epistemological principles for making (selective) use of his contributions, and more generally for subjecting authors and works of the past, and one's own relation to the authors and works of past and present, to a critical treatment that is at once epistemological and sociological.]

In an optimistic form of reflexive judgement, the scientific analysis of science as Merton practises it justifies science by justifying scientific inequalities, by showing scientifically that the distribution of prizes and rewards is in accordance with scientific justice since the scientific world proportions scientific rewards to scientists' scientific merits. It is also in order to ensure the respectability of sociology that Merton tries to make it a real scientific 'profession', modelled on the bureaucracy, and to endow the structural-functionalist spurious paradigm that he helped to construct with Parsons and Lazarsfeld with the spuriously reflexive and empirically validated crowning discipline which is the sociology of science treated as an instrument of sociodicy.

[I should like to conclude with a few observations about the scientometry which is based on the same foundations as Mertonian structural functionalism and which takes for its aim the control and evaluation of science for the purposes of 'policy-making' (the scientometric temptation hangs over the whole history of the sociology of science, as a 'crowning' science capable of awarding certificates of science; and the most modernist, and nihilist, of the new sociologists of science are not immune to this). Scientometry relies on quantitative analyses which take account only of products, in short, on compilations of scientific indicators, such as citations. The bibliometers are realists who hold that the world can be sampled, counted and measured by 'objective observers' (Hargens 1978). They provide scientific administrators with the apparently rational means of governing science and scientists and of giving scientific-looking justifications to bureaucratic decisions. One would need to examine in particular the *limits* of a method that relies on strictly quantitative criteria and which ignores the very diverse modalities and functions of citation (it can even go so far as to disregard the difference between positive and negative references). The fact remains that, despite the dubious (and sometimes deplorable) uses made of bibliometry, these methods can be used to construct sociologically useful indicators, as I did in *Homo Academicus* (1988a) to obtain an index of symbolic capital.]

2 Normal science and scientific revolutions

Although he started out as a historian of science, Thomas Kuhn radically changed the space of theoretical possibles in the sociology of science. His main contribution was to show that the development of science is not a continuous process, but is marked by a series of breaks and by the alternation of periods of 'normal science' and 'revolutions' (Kuhn 1962). Kuhn thereby introduced into the Anglo-American tradition a discontinuist philosophy of scientific development at odds with the positivist philosophy which regarded the progress of science as a continuous movement of accumulation. He also developed the idea of the 'scientific community', arguing that scientists form a closed community whose research bears on a well-defined range of problems and who use methods adapted to this work: the actions of scientists in the advanced sciences are determined by a 'paradigm', or 'disciplinary matrix', that is to say, a state of scientific achievement which is accepted by a significant proportion of scientists and which tends to be imposed on all the others.

The definition of the problems and the research methodology used flow from a professional tradition of theories, methods and competences which can only be acquired through a long training. The rules of scientific method as set out by logicians do not correspond to the reality of scientists' practices. As in other professions, scientists take for granted that the existing theories and methods are valid and they use them for their own purposes. They work not to discover new theories but to solve concrete problems, regarded as 'puzzles': for example, measuring a constant, analysing or synthesizing a compound, or explaining the functioning of a living organism. In order to do so, they use as a paradigm the traditions existing within the domain.

The paradigm is the equivalent of a language or a culture: it determines the questions that can be asked and those that are excluded, the thinkable and the unthinkable; being both 'received achievement' and a starting-point, it is a guide for future action, a programme for research to be undertaken, rather than a system of rules and norms. Consequently the scientific group is cut off from the external world so that one can analyse many scientific problems without taking account of the societies in which the scientists work. [Kuhn in fact introduces, though without developing it as such, the idea of the autonomy of the scientific universe. He thus comes to assert that this universe lies purely and simply beyond the reach of social necessity, and therefore of social science. He fails to say that in reality (and this is what the notion of the field makes it possible to understand) one of the paradoxical properties of very autonomous fields, such as science or poetry, is that they tend to have no other link with the social world than the social conditions that ensure their autonomy with respect to that world, that is to say, the very privileged conditions that are required in order to produce or appreciate very advanced mathematics or poetry, or, more precisely, the historical conditions that had to be combined to produce a social condition such that the people who benefit from them can do things of this kind.]

Kuhn's merit, as I have said, is that he has drawn attention to the discontinuities, the revolutions. But because he is content to describe the scientific world from a quasi-Durkheimian perspective, as a community dominated by a central norm, he does not seem to me to put forward a coherent model for explaining change. It is true that a particularly generous reading can construct such a model and find the motor of change in the internal conflict between orthodoxy and heresy, the defenders of the paradigm and the innovators, with the latter sometimes being reinforced, in periods of crisis, by the fact that the barriers between science and the major intellectual currents within society are then removed. I realize that through this reinterpretation I have attributed to Kuhn the essential part of my own representation of the logic of the field and its dynamic. But this is also, perhaps, a good way to show the difference between the two visions and the specific contribution of the notion of the field.

Having said this, if one sticks to the letter of what Kuhn writes, one finds a strictly *internalist* representation of change. Every paradigm reaches a point of intellectual exhaustion; the disciplinary matrix has produced all the possibles it was capable of generating (a theme that was also found, with reference to literature, in the Russian formalists), like a Hegelian essence that has realized itself, in accordance with its own logic, without external intervention. But certain 'puzzles' remain and do not find a solution.

But I should like to dwell for a moment on an argument of Kuhn's which seems to me very interesting – again, no doubt, because I reinterpret it in terms of my own model – that of 'essential tension', from the title he gave to a collection of articles (Kuhn 1977). The 'essential tension' of science is not that there is a tension between revolution and tradition, between conservatives and revolutionaries, but that revolution implies tradition, that revolutions are rooted in the paradigm: 'Revolutionary shifts of a scientific tradition are relatively rare, and extended periods of convergent research are the necessary preliminary to them. . . . Only investigations firmly rooted in the contemporary scientific tradition are likely to break that tradition and give rise to a new one' (Kuhn 1977: 227). 'The productive scientist must be a traditionalist who likes playing intricate games by pre-established rules in order to be an effective innovator who discovers new rules and new pieces with which to play them' (Kuhn 1977: 237). 'Though testing of basic commitments occurs only in extraordinary science, it is normal science that discloses both the points to test and the manner of testing' (Kuhn 1977: 272). In other words, a (true) scientific revolutionary is someone who has a great mastery of the tradition (and not someone who sweeps away the past or, more simply, just ignores it).

Thus, the 'puzzle-solving' activities of 'normal science' are based on a commonly accepted paradigm which defines, among other things, in a relatively undisputed way, what counts as a correct or incorrect solution. But in revolutionary situations the background framework which alone defines 'correctness' is itself in question. (This is exactly the problem that, in painting, Manet raised by performing a revolution so radical that it called into question the principles in whose name it could have been evaluated). This is when one is confronted with the choice between rival paradigms, and the transcendent criteria of rationality are lacking (no conciliation or compromise is possible – this is the much-discussed theme of the incommensurability of paradigms). And the emergence of a new consensus can only be explained, according to Kuhn, by non-rational factors. But from the paradox of 'essential tension' it can be concluded, reinterpreting Kuhn very freely, that the revolutionary is necessarily someone who has capital (this follows from the existence of a price of entry to the field), in other words a great mastery of the accumulated collective resources, and who therefore necessarily conserves what he supersedes.

Thus everything takes place as if, in pushing to the limit the questioning of the universal standards of rationality already prefigured in the philosophical tradition that had evolved from a Kantian-style 'transcendental' universalism to an already relativized notion of rationality – in Carnap (1950), for example, as I shall show later – Kuhn were rediscovering, with the notion of the paradigm, the Kantian tradition of the a priori, but taken in a relativized, or, more precisely, sociologized sense, as in Durkheim.

Because what appeared as the central theme of his work, namely the tension between the establishment and subversion, was in tune with the 'revolutionary' mood of the day, Kuhn, who was in no way revolutionary, was adopted, somewhat in spite of himself, as a prophet by the students of the University of Columbia and integrated into the 'counterculture' which rejected 'scientific rationality' and proclaimed the supremacy of imagination over reason. Similarly, Feyerabend was the idol of the radical students of the Freie Universität in Berlin (Toulmin 1977: 155–6, 159). The invocation of such theoretical references can be understood when one sees that the student movement took its challenge right onto the terrain of scientific life, in a university tradition where the separation between 'scholarship' and 'commitment' is particularly sharp; the aim was to liberate thought and action from the control of reason and conventions, in the social world as a whole, but also in science.

In short, this scholarly thinking owed its social force not so much to the content of the message itself – except perhaps the title, 'The structure of [. . .] revolutions' – as to the fact that it appeared in a historical context in which an educated population, that of students, was able to appropriate it and transform it into a *specific* revolutionary message, against academic authority. The movement of 1968 carried onto the very privileged terrain of the University a challenge that tended to call into question the deepest and most deeply undisputed principles on which the University was based, starting with the authority of science. It used scientific or epistemological weapons against the academic order which owed part of its symbolic authority to the fact that it was an *instituted episteme*, and that it was ultimately founded on an epistemology. This failed revolution shook up some essential things in the academic order, in particular the cognitive structures of those who dominated the academic and scientific order. One of the targets of the 'contestation' was orthodoxy in the social sciences and the efforts of the Capitoline triad – Parsons, Merton, and Lazarsfeld (who never got over it) – to assign itself the monopoly of the legitimate view of social science (with the sociology of science as its false closure and reflexive crown).

But the main force for resistance to the American paradigm was to appear in Europe, with, in Britain, the Edinburgh school, David Bloor and Barry Barnes, and the Bath group, with Harry Collins, and in France my article of 1975 on the scientific field (Bourdieu 1975a).

3 The 'strong programme'

David Bloor (1983) draws on Wittgenstein to ground a theory of science in which rationality, objectivity and truth are local socio-cultural norms, conventions adopted and imposed by particular groups: he takes over the Wittgensteinian concepts of 'language game' and 'form of life' which play a central role in *Philosophical Investigations*, and interprets them as referring to sociolinguistic activities associated with particular socio-cultural groups in which practices are regulated by norms conventionally adopted by the groups concerned. Scientific norms have the same limits as the groups within which they are accepted. I shall borrow from Yves Gingras a synthetic presentation of the four principles of the 'strong programme': 'In his book *Knowledge and Social Imagery*, published in 1976, with a second edition in 1991, David Bloor sets out four major methodological principles which have to be followed in order to construct a conclusive sociological theory of scientific knowledge: (1) causality: the explanation proposed must be causal; (2) impartiality: the sociologist must be impartial as regards the "truth" or "falsehood" of the assertions made by the actors; (3) symmetry: this principle states that "the same types of causes" must be used to explain both beliefs judged to be "true" by the actors and those judged to be "false"; and (4) reflexivity requires the sociology of the sciences to be subject in principle to the same treatment it applies to the other sciences. In the many case studies based on these principles, causality has been interpreted broadly enough to include the idea of understanding (thus avoiding the old dichotomy of explanation vs. understanding). Whereas the principle of impartiality is self-evident in methodological terms and has not really given rise to debate, philosophers have much debated the precise meaning and validity of the principle of symmetry. Finally, the principle of reflexivity in fact plays no part in the case studies and has only really been taken seriously by Woolgar and Ashmore, who have thus been led to study the sociology of science and its writing practices more than the sciences themselves' (Gingras 2000). I would entirely endorse this presentation and the comments it contains, only adding that in my view one cannot speak of reflexivity with respect to analyses of (other people's) sociology of the sciences which belong more to polemics than to 'the polemic of scientific reason', inasmuch as, as Bachelard suggested, this is first directed against the researcher himself.

As for Barry Barnes (1974), who makes explicit the theoretical model underlying Kuhn's analysis, he fails (like Kuhn) to raise the question of the autonomy of science, even if he refers primordially (if not exclusively) to internal factors in his search for the social causes of the belief-preferences of scientists. Social interests generate tactics of persuasion, opportunistic strategies and culturally transmitted dispositions that influence the content and development of scientific knowledge. Far from being unequivocally determined by 'the nature of things' or by 'pure logical possibilities', as Mannheim supposed, scientists' actions and the emergence and crystallization of scientific paradigms are influenced by intra- and extratheoretical social factors. Barnes and Bloor (1982) stress the *underdetermination of theory by data* (theories are never completely determined by the data they invoke, and several theories can always point to the same data); they also emphasize the fact (a commonplace for the continental epistemological tradition) that observation is oriented by theory. Controversies (made possible, once again, by underdetermination) show that the consensus is fundamentally fragile; many controversies come to an end without having been resolved by evidence alone, and stable scientific fields always contain malcontents who attribute the consensus to pure social conformism.

Harry Collins and the Bath school lay stress not so much on the relationship between interests and preferences as on the processes of interaction between scientists in and through which beliefs are formed, or, more precisely, on scientific controversies and the non-rational methods that are used to settle them. For example, Harry Collins and Trevor Pinch show that in a controversy between establishment scientists and parapsychologists both sides use strange and dishonest procedures; everything takes place as if the scientists had set up arbitrary frontiers to keep out ways of thinking and behaving that are different from their own. Collins and Pinch criticize the role of 'replication' (or conclusive experiments) in experimental science. When scientists try to reproduce other scientists' experiments, they often modify the original experimental conditions, equipment and procedures, to pursue their own programmes, whereas a perfect replication presupposes interchangeable agents (the confrontation between Pasteur and Koch would need to be analysed in this light). Moreover, without very great familiarity with the problem in question, it is very difficult to reproduce experimental procedures from a written report. Scientific accounts aim to respect the ideal norms of scientific protocol rather than describe what really happened. Scientists may repeatedly obtain 'good' results without being able to say how they got them. When other scientists fail to 'replicate' an experiment, the original researchers may object that their procedures have not been correctly observed. In fact, the acceptance or rejection of an experiment depends on the credence given to the competence of the experimenter as much as on the strength and significance of the experimental proofs. It is not so much the intrinsic strength of the true idea that carries conviction as the social strength of the verifier. So, the scientific fact is made by the person who produces and proposes it, but also by the person who receives it (another analogy with the artistic field).

In short, like Bloor and Barnes, Collins and Pinch emphasize that experimental data are not in themselves enough to determine the extent to which an experiment counts as validating or invalidating a theory, and that it is the negotiations within a 'core set' of interested researchers that determine whether a controversy is closed. These negotiations depend to a large extent on judgements about questions of personal honesty, technical competence, institutional affiliation, style of presentation and nationality. In short, Popperian falsificationism gives an idealized image of the solutions provided by the 'core set' of scientists in the course of their disputes.

The great virtue of Collins is that he reminds us that a fact is a collective construct and that the attested, certified fact is constructed in the interaction between the person who produces the fact and the person who receives it and tries to 'replicate' it so as to falsify or confirm it; and that he shows that processes similar to those I discovered in the world of art are also found in the scientific world. But the limits of his work result from the fact that he remains enclosed within an interactionist vision which seeks the principle of agents' actions in the interactions between them and ignores the structures (or objective relationships) and the dispositions (generally correlated with the position occupied within these structures) that are the real principle of actions and, among other things, of the interactions themselves (which may be the mediation between structures and actions). Remaining within the confines of the laboratory, he does not at all consider the *structural* conditions of the production of belief, with for example what might be called ‘lab capital’, brought to light by the Mertonians, who showed, for example, as we have seen, that if a discovery is made in a reputed laboratory at a prestigious university it has more chance of being validated than if it emerges in another, less well-regarded one.

4 A well-kept open secret

Laboratory studies have a clear importance inasmuch as they have broken with the rather distant and undifferentiated vision of science and moved closer to the sites of production. They thus represent an undeniable contribution, which I will sum up in the words of a member of this school, Karin Knorr-Cetina: ‘Scientific objects are not only “technically” manufactured in laboratories, but are also inextricably symbolically or politically construed, for example, through literary techniques of persuasion such as one finds embodied in scientific papers, through the political stratagems of scientists in forming alliances and mobilizing resources, or through the selections and decision translations which “build” scientific findings from within’ (Knorr-Cetina 1992: 115). Among the ‘pioneers’ of laboratory studies, I would like to mention the work of Mirko D. Grmek (1973) and Frederic L. Holmes (1974), who made use of Claude Bernard’s laboratory notebooks to analyse various aspects of his work. One sees there how even the best scientists dismiss unfavourable results as aberrations which they exclude from their official accounts, how they sometimes transform equivocal experiments into decisive results, or modify the order in which experiments were conducted, etc., and how they all comply with the common rhetorical strategies that are required in the shift from private laboratory notes to publications.

But here I must quote Medawar, who sums up very well the distortions that result from relying purely on published accounts: ‘findings appear more decisive and more honest; the most creative aspects of the research disappear, giving the impression that imagination, passion, art have played no part in them and that the innovation results not from the passionate activity of deeply committed hands and brains but from passive submission to the sterile precepts of the so-called “Scientific Method”. This impoverishment leads to the ratification of an old-fashioned and naïve empiricist or inductivist view of research practice’ (Medawar 1964).

On the basis of a study of a laboratory in which she minutely studied the successive drafts of a document which led to a publication after passing through sixteen different versions, Karin Knorr-Cetina analyses in detail the transformations of the rhetoric of the text, the work of depersonalization carried out by the authors, etc. (One's only regret is that, rather than going in for long theoretico-philosophical debates with Habermas, Luhmann, etc., she does not give the strictly sociological information about the authors and their laboratory that would enable one to relate the rhetorical strategies to the position of the laboratory in the scientific field and the dispositions of the agents engaged in the production and circulation of the drafts.)

However, the most accurate and complete account of the achievements of this tradition that I have found is that by G. Nigel Gilbert and Michael Mulkay (1984). They show that scientists' discourse varies according to the context, and they distinguish two 'repertoires' (it seems to me it would be better to say two rhetorics). The 'empiricist repertoire' is characteristic of formal experimental research papers which are written in accordance with the empiricist representation of scientific action: the style must be impersonal and minimize reference to social actors and their beliefs so as to produce all the appearances of objectivity; references to the dependence of the observations on theoretical speculations disappear; everything is done to mark the scientist's distance from his model; the account given in the 'methods' section is expressed in the form of general formulae. Then there is the 'contingent repertoire', which coexists with the first: when scientists speak informally, they stress their dependence on an 'intuitive feel for research', which is inevitable given the practical character of the operations in question (Gilbert and Mulkay 1984: 53). These operations cannot be written out and they can only really be understood through close personal contact. The authors speak of 'practical skills', traditional knacks, 'recipes' (researchers often make comparisons with cooking). Research is a customary practice, learned by example. Communication is set up between people who share the same 'background' of problems and technical assumptions. It is remarkable that, as the authors point out, scientists spontaneously return to the language of the 'contingent repertoire' when they talk about what other people do or offer their sceptical reading of other people's official protocols.

In short, scientists use two linguistic registers: in the 'empiricist repertoire', they write in a conventionally impersonal manner; by minimizing the references to human intervention, they construct texts in which the physical world seems literally to act and speak for itself. When the author is authorized to appear in the text, he is presented either as forced to undertake the experiments, or to reach the theoretical conclusions, by the unequivocal demands of the natural phenomena he is studying, or as rigidly constrained by rules of experimental procedure. In less formal situations, this repertoire is complemented and sometimes contradicted by a repertoire which stresses the role played by personal contingencies in action and belief. The asymmetrical account which presents the correct belief as springing indisputably from the experimental proof and the incorrect belief from the effect of personal, social and generally non-scientific factors, reappears in studies of science (which most often largely rely on formal accounts).

What sociology brings to light is in fact known and even belongs to the realm of 'common knowledge', as economists call it. Private discourse on the private aspect of research seems almost designed to recall to modesty the sociologist who might be tempted to think that he is discovering the 'inner workings' of science, and should in any case be treated with much reflexion and delicacy. It would take great quantities of refined phenomenology to analyse these phenomena of dual consciousness associating and combining – like all forms of *bad faith* (in the Sartrian sense) or self-deception – knowledge and the refusal to know, knowledge and refusal to know that one knows, knowledge and refusal to let other people say what one knows, or worse, *that* one knows. (One would have to say the same of career 'strategies' and, for example, choices of specialty or object of study, which cannot be described in terms of the ordinary alternatives of awareness and unawareness, calculation and innocence.) All these games of individual bad faith are only possible in a profound complicity with a group of scientists.

But I should like to mention in more detail the last chapter, entitled 'Joking apart'. The authors point out that when they go into laboratories, they find, often pinned to the walls, curious texts such as a 'Dictionary of useful research phrases', which circulate from lab to lab, and give examples of the ironic and parodic discourse about scientific discourse which scientists themselves produce: 'Post-prandial Proceedings of the Cavendish Physical Society', 'Journal of Jocular Physics', 'Journal of Irreproducible Results', 'Review of Unclear Physics'.

Along the lines of the 'do say...don't say ...' lists found in language manuals, the authors draw up a comparative table contrasting two versions of what goes on, the one that is produced for formal presentation, and the informal account of what really happened. On the one hand, 'What he wrote', on the other, 'What he meant' (Gilbert and Mulkay 1984: 176):

What he wrote	*What he meant*
(a) It has long been known that . . .	I haven't bothered to look up the reference.
(b) While it has not been possible to provide definite answers to these questions . . .	The experiment didn't work out, but I figured I could at least get a publication out of it.
(c) The W-PO system was chosen as especially suitable . . .	The fellow in the next lab had some already prepared.
(d) Three of the samples were chosen for detailed study . . .	The results on the others didn't make sense and were ignored.
(e) Accidentally strained during mounting . . .	Dropped on the floor.
(f) Handled with extreme care throughout the experiment . . .	Not dropped on the floor.
(g) Typical results are shown . . .	The best results are shown, i.e. those that fit the dogma.
(h) Agreement with the predicted curve is:	
Excellent	Fair
Good	Poor
Satisfactory	Doubtful
Fair	Imaginary
(i) Correct within an order of magnitude . . .	Wrong.
(j) Of great theoretical and practical importance . . .	Interesting to me.
(k) It is suggested that . . . it is believed that . . . it appears that . . .	I think.
(l) It is generally believed that . . .	A couple of other guys think so too.
(m) The most reliable results are those obtained by Jones . . .	He was my graduate student.
(n) Fascinating work . . .	Work by a member of our group.
(o) Of doubtful significance . . .	Work by someone else.

This table produces a humorous effect by exposing the hypocrisy of the formal literature. But the dual truth of the experience that agents may have of their own practice has something universal about it. One knows the truth of what one does (for example, the more or less arbitrary or in any case contingent character of the reasons or causes which determine a judicial decision), but to keep in line with the official idea of what one does, or with the idea one has of oneself, this decision must appear to have been motivated by reasons, and by reasons that are as elevated (and juridical) as possible. Formal discourse is hypocritical, but the propensity to 'radical chic' leads people to forget that the two truths coexist, with more or less difficulty, in the agents themselves (this is a truth that I took a long time to learn and that I learned, paradoxically, from the Kabyles, perhaps because it is easier to understand other people's collective hypocrisies than one's own). Among the forces that support social rules there is the imperative of *regularization*, manifest in the fact of 'falling into line with the rule', which leads people to present practices which may be in complete transgression of the rule as being performed in accordance with the rule, because the essential thing is to save the rule (and this is why the group approves and respects this collective hypocrisy). It is a matter of saving the particular interests of a particular scientist who broke his pipette; but also, and at the same time, of saving the collective belief in science which means that, although everyone knows that things do not happen the way people say they happen, everyone carries on as if they happened that way. And this raises the very general problem of the function or effect of sociology, which, in many cases, makes public the 'denied' things that groups know and 'do not want to know'.

One would therefore be tempted to ratify the – it seems to me, fairly indisputable – conclusion reached by Gilbert and Mulkay, or Peter Medawar, if it were not most often associated with a philosophy of action (and a cynical vision of practice) which is fully developed in most of the writings devoted to 'laboratory life'. For example, while it is no doubt true that, as Karin Knorr-Cetina says, the laboratory is a place where actions are performed with a view to 'making things work' (she quotes Lynch: 'The vernacular formulation of "making it work" suggests a contingency of results upon "skilled production"... Making it work entails a selection of those "effects" that can be traced to "rational" sets of contingencies and a discarding of "attempts" that are bound to fall short of such "effects"' (Lynch 1982: 161, quoted by Knorr-Cetina 1983: 120)), it is impossible to accept the idea she expresses in the sentence I cited earlier, where she slides from the assertion, which is at the centre of my first article, of the *inseparably scientific and social* character of researchers' strategies, to the assertion of a symbolic *and political* construction based on 'techniques of persuasion' and 'stratagems' aimed at building alliances. The simultaneously scientific and social 'strategies' of the scientific habitus are envisaged and treated as *conscious*, not to say *cynical*, *stratagems*, oriented towards the glory of the researcher.

But I must now turn, to conclude, to a branch of the socio-philosophy of science that has developed mainly in France, but which has enjoyed some success on the campuses of English-speaking universities: I mean the works of Latour and Woolgar and, in particular, *Laboratory Life*, which gives an enlarged image of all the aberrations of the new sociology of science (Latour and Woolgar 1979). This current is very strongly marked by the historical conditions, so that I fear I shall find it difficult to distinguish, as I have for the previous currents, the analysis of the theses in question from the analysis of their social conditions of production. [For example, in a would-be benevolent 'summary' of *Laboratory Life*, one reads: 'The laboratory deals with inscriptions (in Derrida's terms), utterances (in Foucault's terms); constructions which make the realities they evoke. These constructions are imposed through the negotiation of the small groups of researchers concerned. Verification (assay) is self-verification; it creates its own truth; it is self-verifying because there is nothing to verify it with. *Laboratory Life* describes the process of verification as a process of negotiation'.]

It is posited that the products of science are the result of a process of manufacture and that the laboratory, itself an artificial universe, cut off from the world in countless ways, physically, socially and also by the capital of instruments that is handled there, is the site of the construction, even the 'creation', of the phenomena with which we build up and test theories and which would not exist without the instrumental equipment of the laboratory. 'The artificial reality that the participants describe as an objective entity, has in fact been constructed.'

Starting from this observation, which for anyone familiar with Bachelard is hardly stunning, it is possible, by playing on words or letting words play, to move to apparently radical propositions (calculated to make big waves, especially on American campuses dominated by the logical-positivist vision). By saying that facts are artificial in the sense of manufactured, Latour and Woolgar intimate that they are fictitious, not objective, not authentic. The success of this argument results from the 'radicality effect', as Yves Gingras (2000) has put it, generated by the slippage suggested and encouraged by a skilful use of ambiguous concepts. The strategy of *moving to the limit* is one of the privileged devices in the pursuit of this effect (I remember the use made in the 1970s of Illich's thesis of 'deschooling society' to counter the description of the reproductive effect of the educational system); but it can lead to positions that are untenable, unsustainable, because they are simply absurd. From this comes a typical strategy, that of advancing a very radical position (of the type: the scientific fact is a construction or – *slippage* – a fabrication, and therefore an artefact, a fiction) before beating a retreat, in the face of criticism, back to banalities, that is, to the more ordinary face of ambiguous notions like 'construction', etc.

But to produce this effect of 'derealization', the authors do not simply stress the contrast between the improvised character of real laboratory practices and experimental reasoning as rationally reconstructed in textbooks and research reports. Latour and Woolgar highlight the very important role of *texts* in the *fabrication of facts as fiction.*

They argue that the researchers they observed during their ethnography at the Salk Institute did not investigate things in themselves; rather, they dealt with 'literary inscriptions' produced by technicians working with recording instruments: 'Between scientists and chaos there is nothing but a wall of archives, labels, protocol books, figures, and papers ...' 'Despite the fact that our scientists held the belief that the inscriptions could be representations or indicators of some entity with an independent existence "out there", we have argued that such entities were constituted solely through the use of these inscriptions' (Latour and Woolgar 1979: 245, 128). In short, the researchers' naïvely realist belief in a reality external to the laboratory is a pure illusion, from which only a realist sociology can rid them.

Once the final product has been worked out in circulation, the intermediate stages that made it possible, in particular the vast network of negotiations and machinations that have given rise to the acceptance of a fact, are forgotten, not least because researchers wipe away the traces of their research as they move on. Because scientific facts are constructed, communicated and evaluated in the form of written statements, scientific work is largely a literary and interpretative activity: 'A fact is nothing but a statement with no modality – M – and no trace of authorship' (Latour and Woolgar 1979: 82); the work of circulation will lead to the removal of the modalities, in other words the indicators of temporal or local reference (for example, 'these data *may* indicate that ...', 'I *believe* this experiment shows that ...'), in short, all 'indexical' expressions. The researcher must reconstruct the process of consecration and universalization through which the fact gradually comes to be recognized as such – publications, networks of citations, disputes between rival laboratories and negotiations among members of a research group (which means, for example, the social conditions in which the hormonal factor, TRF, was stripped of contentious qualifications); he must describe how a judgement was transformed into a fact and so freed from the conditions of its production (now forgotten both by the producer and the receivers).

Latour and Woolgar seek to adopt the point of view of an observer who sees what happens in the laboratory without sharing the researchers' beliefs. Making a virtue of necessity, they describe what seems to them intelligible in the laboratory: the traces, the texts, the conversations, the rituals, and the strange material (one of the high points of this work is the 'stranger's' description of a simple instrument, a pipette . . . – Woolgar 1988b: 85). They can thus treat natural science as a literary activity, and, to describe and interpret this circulation of scientific products, they draw on a semiological model (that of A. J. Greimas). They attribute the privileged status of the natural sciences not to the particular validity of their discoveries but to the expensive equipment and institutional strategies which transform natural elements into practically impregnable texts, with the author, the theory, nature and the public being so many 'text effects'.

The *semiological vision of the world* which induces them to emphasize the traces and signs leads them to that paradigmatic form of the scholastic bias, *textism*, which constitutes social reality as text (in the manner of some ethnologists, like Marcus (Marcus and Fischer 1986) or even Geertz, or some historians, who, with the 'linguistic turn' at about the same time, started to say that everything is text). Science is then just a discourse or a fiction among others, but one capable of exerting a 'truth effect' produced, like all other literary effects, through textual characteristics such as the tense of verbs, the structure of utterances, modalities, etc. (The absence of any attempt at prosopography condemns them to seek the power of texts in the texts themselves.) The universe of science is a world which succeeds in imposing universally the belief in its fictions.

The semiological prejudice is most clearly seen in *The Pasteurization of France* (Latour 1988), in which Latour treats Pasteur as a textual signifier inserted in a story which weaves together a heterogeneous network of agencies and entities, daily life on the farm, sexual practices and personal hygiene, architecture and the therapeutic regime of the clinic, sanitary conditions in towns and the microscopic entities encountered in the laboratory, in short a whole world of representations that Pasteur constructs and through which he presents himself as the eminent scientist. [I would like, as it were *a contrario*, to mention here a work which, being based on a meticulous reading of a good part of Pasteur's 'laboratory notebooks', gives a realistic and well-informed view, but without ostentatious display of gratuitous theoretical effects, of Pasteur's undertaking, but also (chapter 10) of the Pasteur 'myth': G. L. Geison, *The Private Science of Louis Pasteur* (1995).]

Semiologism combines with a naïvely Machiavellian view of scientists' strategies: the symbolic actions they perform to win recognition for their 'fictions' are at the same time influence-seeking and power-seeking strategies through which they pursue their own glorification. So the question is how a man named Pasteur built alliances and proselytized to impose a research programme. With all the ambiguity that results from treating semiological entities as socio-historical descriptors, Latour treats Pasteur as a kind of semiological entity who acts historically, and who acts as any capitalist would act (one can read in this light the interview entitled 'The last of the wild capitalists' (Latour 1983), in which Latour endeavours to show that the scientist aware of his symbolic interests is the most accomplished form of the capitalist entrepreneur, all of whose actions are guided by the pursuit of maximized profit). Rather than seeking the principle of actions where it really lies, in positions and dispositions, Latour can only try to find it in conscious (even cynical) influence and power strategies (thus regressing from a Mertonian collective finalism to a finalism of individual agents) – and the science of science is reduced to the description of alliances and struggles for symbolic 'credit'.

Having been accused by the advocates of the 'strong programme' of practising disinformation and using scientifically dishonest strategies, Latour, who, in all the rest of his work appears as a radical constructivist, has recently made himself the champion of realism, invoking the social role he gives to objects, and in particular manufactured objects, in his analysis of the scientific world. He proposes to do nothing less than challenge the distinction between human agents (or forces) and non-human agents. But the most astonishing example is that of the door and the automatic door closer, called in French a 'groom' by analogy with the human groom or butler, which Latour invokes in an article entitled 'Where are the missing masses?' (1993), with a view to finding in things the constraints that are missing (the 'missing masses', a chic scientific reference) in the ordinary analysis of the political and social order. Although they are mechanical objects, doors and technical devices act as constant constraints on our behaviour and the effects of the intervention of these 'actants' are indistinguishable from those exerted by a moral or normative control: a door lets us pass through a certain point in the wall and at a certain speed; a mechanical policeman controls the traffic like a real policeman, the computer on my desk requires me to write it instructions in a particular syntactic form. The 'missing masses' (analogous to those that explain the value of the rate of expansion of the universe, no less ...) lie in the technical objects that surround us. We delegate to them the status of actors and also power. To understand these technical objects and their power, do we need to study the technical science of their operation? (This is no doubt easier with a door or a pipette than with a cyclotron

. . .) If not, then what method must we use to discover the fact of 'delegation' and what is delegated to these remarkable 'actants'? We simply have to resort to the method, well known to economists, of 'counterfactual hypotheses', and thus, in seeking to understand the power of doors, to imagine what life would be like if they did not exist. You draw up a double-entry table: on one side, what you would have to do if there were no door; on the other, the slight effort of pulling or pushing which achieves the same result. So a big effort is turned into a smaller, and the operation thus brought to light by the analyst is what Latour proposes to call 'displacement or translation or delegation or shifting': 'we have delegated to the hinge the work of reversibly solving the hole-wall dilemma.' And to conclude, one arrives at a general law: 'every time you want to know what a non-human does, simply imagine what other humans or other non-humans would have to do were this character not present. This imaginary substitution exactly sizes up the role, or function, of this little figure.' All power to the (scientific) imagination. The trivial difference between human and non-human agents has disappeared (the 'groom' takes the place of a person and shapes human action by prescribing who can go through the door) and one can freely dissertate upon the way we delegate power to technical objects . . . To show that what might be seen as a mere literary game is in fact the expression of the 'methodological' approach of a 'school', I could also have mentioned Michel Callon (1986), who, in his study of scallops, places on the same footing fishermen, scallops, seagulls and the wind, as elements in a 'system of actants'. But I will leave it at that . . .

[I cannot help feeling some unease at what I have just done. On the one hand, I would not want to give this work the importance it gives itself and even risk helping to give it value by pushing the critical analysis beyond what this kind of text deserves, though I think it a good thing that there are people willing to devote time and energy to ridding science of the dire effects of philosophical hubris as Jacques Bouveresse (1999) has done for Régis Debray, or Yves Gingras (1995) for the same Latour. But, on the other hand, I have in mind a very fine article by Jane Tompkins (1988), who describes the logic of 'righteous wrath', the 'sentiment of supreme righteousness' of the hero of a Western who, having been 'unduly victimized', may be led to 'do to the villains things which a short while ago only the villains did': in the academic or scientific world, this sentiment can lead someone who feels invested with the mission of doing justice to commit a 'bloodless violence' which, while remaining within the limits of academic propriety, is inspired by a rage no less strong than that which led the hero to do justice himself. And Jane Tompkins points out that this legitimate fury may lead one to feel justified in attacking not only the faults and failings of a text but the most personal properties of the person. Nor will I conceal the fact that behind the 'discourse of importance' (an essential part of which is devoted to asserting the importance of the discourse – I'm referring to the analysis I made of the rhetoric of Althusser and Balibar (Bourdieu 2001b)), its incantatory and self-legitimating formulae (one is 'radical', 'counterintuitive', 'new'), its peremptory tone (designed to overwhelm), I was pointing to the dispositions statistically associated with a particular social origin (it is certain that dispositions towards arrogance, bluff, even imposture, the quest for the effect of radicality, etc., are not equally distributed among researchers depending on their social origin, their sex, or more precisely their sex *and* their social origin). And I could not refrain from suggesting that if this rhetoric has enjoyed a social success disproportionate to its merits, this is perhaps because the sociology of science occupies a very special position within sociology, on the ill-defined border between sociology and philosophy, so that it is possible there to avoid a real break with philosophy and with all the social profits associated with being able to call oneself a philosopher in certain markets; such a break is long and costly, presupposing the hardwon acquisition of technical instruments and many unrewarding investments in activities regarded as inferior, even unworthy. Socially constituted dispositions towards audacity and facile radicalism which, in scientific fields more capable of imposing their controls and censorship, would have had to be tempered and sublimated, have found there a terrain on which they can express themselves without any mask or constraint. Having said that, my 'righteous anger' has in my view a justification in the fact that these people, who often refuse the name and the contract of sociologists without really being able to submit to the constraints of philosophical rigour, may enjoy some success among new entrants and hold back the progress of research by disseminating false problems which waste much time, overall, by leading some into cul-de-sacs and others, who have better things to do, into an effort of critique, often somewhat desperately, such is the power of the social mechanisms capable of sustaining error. I am thinking in particular of the *allodoxia*, the erroneous conception of the identity of persons and ideas, which prevails particularly with respect to all those who occupy the uncertain regions between philosophy and the social sciences (and also journalism), and who, situated either side of the frontier – just outside, like Régis Debray, with his scientific metaphors mimicking the external signs of scientificity (Gödel's theorem, which provoked Jacques Bouveresse's 'righteous anger') and his pseudo-scientific label, 'mediology', or just inside, like our sociologist-philosophers of science – are particularly able and particularly well placed to inspire a misplaced credence, *allodoxia*, by playing on all the double games, guaranteeing all the double profits secured by the combination of several registers of authority and importance, including that of philosophy and that of science.]