12

Don't Throw the Baby Out with the Bath School! A Reply to Collins and Yearley

Michel Callon and Bruno Latour

Mademoiselle de l'Espinasse: "Voilà ma toile; et le point originaire de tous ces fils c'est mon araignée"

Bordeu: "A merveille"

Mademoiselle de l'Espinasse: "Où sont les fils? Où est placée l'araignée?"

Diderot, *Le Rêve de d'Alembert*

Harry Collins and Steve Yearley (from now on C&Y) are satisfied with the state of social studies of science. Most of the problems have been solved, important discoveries have been made, sociology is firm enough on its feet to study the natural sciences. Thus, according to them, there is no fundamental reason to switch to other frames of reference—and there is still less reason to let "bloody foreigners" dabble in a field where the British have been firmly in command for so many years. Wherever we go, C&Y have already been there, have given satisfactory explanations, have developed an adequate methodology, and have solved the empirical problems. Even if they recognize that there might be some residual difficulties—the problems of reflexivity, that of symmetry, the potential conflict between relativism and social realism—their solution is to shun these intellectual traps by a process of alternation, another name for blithe ignorance, and an appeal to common sense and professional

Harry Collins and Steve Yearley had the generosity to host a one-day informal seminar to play chicken "live." Steve Woolgar and Bruno Latour were the contestants and Gerard de Vries and Wiebe Bijker the referees. Members of the Bath School, David Gooding and David Travis, abstained in a gentlemanly way from pushing the contestants under the traffic. The final dinner was in a Lebanese restaurant, but we decided not to take this as an omen of future civil strife. We benefited enormously from this one-day discussion but restrict ourselves in this paper to the published materials. Many useful comments by Gerard de Vries, Steven Shapin, and Mike Lynch could not be used, since we had agreed not to alter our respective papers so much as to make them movable targets. On the whole we felt it was a welcome and clarifying debate. We thank Gabrielle Hecht and Michael Bravo for their comments and corrections.

loyalty. Sociology is good enough to do the job, and if it is not, then let them be like their brave ancestors and say, "right or wrong, my discipline." The overall tone of C&Y implies that if all those bizarre ideas were left to thrive, sociologists of science might have to retool some of their concepts, start reading new people, maybe even philosophers of the pre-Wittgenstein era, or worse, economists of technical change, political philosophers, semioticians, and while we are at it, why not novelists or technologists or metaphysicians? No, whatever other schools have to offer, none of them is better than the good old sociology we have at hand, and instead of helping the French to overcome their deficiencies, it is better to throw them out with the bathwater.

We disagree with this assessment of the field. We are dissatisfied with the state of the art, which is now in danger of dismantlement after fifteen years of rapid advance (see Latour, in press, a, for a diagnostic). We think it is about time to change the bath water, but contrary to our colleagues, we do not want to throw the baby out with it, and especially not the Bath school. We learned a great deal from Collins's work—the study of active controversies, the meticulous application of symmetry in the treatment of parasciences, the emphasis put on local skills, the careful study of replication, the dismantlement of epistemologists' hegemony, the stress on networks and entrenchment mechanisms, and above all, his crisp and witty style of reasoning. However, we do not believe that the microsociology of the Bath school has put an end to the history of the field. We are also dissatisfied with our own network theory, but contrary to C&Y, we do not see this as a reason to put our head in the sand and pretend that sociology of science is "business as usual." Our deficiencies spur us to go on looking for alternatives, original methods, and yes, a still more radical definition of the field. The domain is young. The topics of science and society have barely been touched.

For their sometimes condescending but on the whole earnest critique of the "Paris school," C&Y have chosen two papers which are explicitly "ontological manifestos" out of a production of six books, five edited volumes and about sixty articles. Fair enough. In our reply we will stick to those two papers and will abstain from using other materials, although we will cite many others for the benefit of readers interested in following through. If we agree to restrict the dispute to those two papers, then in return C&Y have to acknowledge that we wrote them in a peculiar style. We recognize that the empirical basis of those two papers and their methods are rather idiosyncratic, but their goal is to transform the definition of entities as it is accepted in the field of social studies of science by doing two ontological experiments, one on nature, the other on technology. Each of them is followed by scores of methodological and empirical papers that C&Y have the right to ignore, although the accusation of a poverty of methods, of lack of rigor, and of a failure to provide explanations would have been more compelling had a slightly larger corpus been chosen.

The major criticism made by our colleagues is that even if our position is philosophically radical and justified, its practical effect on the use of empirical material is prosaic, reactionary, and dangerously confusing. The justification for this judgment is that in spite of what we claim, we are accused of going back to the realist position to explain scientific facts and to technical determinism to account for artifacts. Since in Paris and Bath we all agree that the touchstone of any position is its empirical fruitfulness, we concede that if indeed the empirical evidence is proven messy, we waive forever the right to appeal either to the quality of our philosophy or to the purity of our intentions.

In intellectual controversies one good way to assess the quality of claims is to see which side understands not only its own position but also that of the other side (another, lighter, touchstone is checking to see which side reads the other's production completely). We feel that the exasperation of C&Y is not only respectable but understandable and important for the future of the field, and that we are able to explain both why they are wrong and why they can't help misinterpreting us in the very way they do. The yardstick they are using to qualify any given piece of work as "advanced," "radical," or "reactionary" is the following (see fig. 12.1). There is one line going from the nature pole to the social pole, and it is along this line that schools of thought may be logged. If you grant a lot of activity to nature in the settlement of controversies, then you are a reactionary, that is, a realist; if, on the contrary, you grant a lot of activity to society in settling controversies, then you are a constructivist or a radical, with various nuances which may only be logged along this line. Although the philosophical foundation of this yardstick is crucial, we will not go into that, since the debate only hinges on the empirical use of this philosophy; but see Latour 1990, in press, a.

The claim of C&Y is that social studies of science (or SSK, as they choose to call it) is engaged in a fight, a tug-of-war between two extreme positions, one which they label "natural realism" which starts with the existence of objects to explain why we humans agree about them; and the other, which they label "social realism," which starts, on the contrary, from the firm foundation of society in order to account for why we collectively settle on matters of fact. The

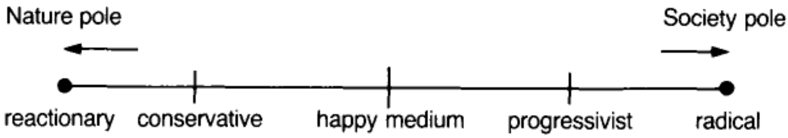

Figure 12.1 Positions in science studies debates are aligned along one line only, going from Nature to Society and using terms which are politically laden.

alternation they advocate is that we should switch from natural realism when we are scientists to social realism when we play the role of sociologists explaining science. This point is very important, because it is this alternation that C&Y call "symmetry." In this tug-of-war, any sociologist who stops being a social realist would be a traitor, since he or she would abandon the fight or, worse still, help out the other side. We in Paris are viewed as such traitors, because we give back to nature the role of settling controversies. The reflexivist is seen as less of a pest, since she places herself behind all the teams to plague them; but she is traitor nonetheless because she especially delights in bugging the "social" team with her endless bites and kicks (fig. 12.2). (But she is good enough to fend for herself [Ashmore 1989], and we will not plead on her behalf in this paper).

The reason why we may use the word "treason" is that C&Y's paper is a moral and deontological paper. The field of science studies has been engaged in a moral struggle to strip science of its extravagant claim to authority. Any move that waffles on this issue appears unethical, since it could also help scientists and engineers to reclaim this special authority which science studies has had so much trouble undermining. This is a serious claim and we cannot take it

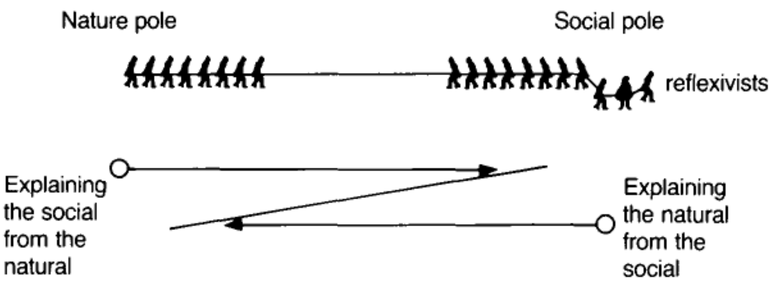

Figure 12.2 The tug-of-war between realists, on the left, and constructivists, on the right; reflexivists are those who hamstring the players of the Social team. More seriously, the sources of the explanation may come from two contradictory repertoires.

lightly. Here are the two more damning accusations of high treason, the first for science:

> Far from adding to our understanding it seems to us that the resulting account [of Collins's gravity waves phrased in Callon's ways] would look just like the account of a conventional historian of science—except that the historian wouldn't talk of allegiances with gravity waves and failures of negotiations with gravity waves, but of discoveries and failures of experimental technique. The language changes but the story remains the same.

and the second for technology:

> The consequences of the semiotic method [of Latour] amount to a backward step, leading us to embrace once more the very priority of technological, rule-bound description, adopted from scientists and technologists, that we once learned to ignore.

This is not a misreading of our position. Neither is it an anti-French prejudice, or a peculiar blindness to others' ideas, or even tunnel vision: it is a necessity of C&Y's cold war waged against realists. Our position is for them unjustifiable since it helps the traditional and conventional technologists and scientists to win the day over SSK's discoveries. The whole accusation now hinges on two questions the jury is asked to settle: did Callon and Latour commit the crime of granting to nature and to artifacts the same ontological status that realists and technical determinists are used to granting them? If so, did they commit this crime in intention or in effect, or both? The second possibility is more damning than the first and the only one that really counts for our discussion.

We have to confess that in C&Y's frame of reference—and for that matter in the whole Anglo-American tradition of science studies—the answer has to be "yes." We are guilty on both counts, and we understand why our position is bound to be read this way by social realists.

Why is this reading by C&Y so inevitable? Because they cannot imagine any other yardstick for evaluating empirical studies than the one defined above, and they cannot entertain even for a moment another ontological status for society and for things. All the shifts in vocabulary like "actant" instead of "actor," "actor network" instead of "social relations," "translation" instead of "interaction," "negotiation" instead of "discovery," "immutable mobiles" and "inscriptions" instead of "proof" and "data," "delegation" instead of "social roles," are derided because they are hybrid terms that blur the distinction between the really social and human-centered terms and the really natural and object-centered repertoires. But who provided them with this real distribution between the social and the natural worlds? The scientists whose hegemony in defining the world C&Y so bravely fight. Obsessed by the war they wage against "natural realists," they are unable to see that this battle is lost as soon as we accept the definition of society handed to them under the name of "social realism." This is now what we have to demonstrate and we will show that if there are to be traitors in this world (which might not be necessary) they might be the ones sticking to social realism, not us.

Let us first examine the yardstick we use to decide who is reactionary and who is not, and then examine what difference it makes empirically. We have never been interested in playing the tug-of-war that amuses the Anglo-American tradition so much, and C&Y are right in saying that we are born traitors, so to speak, from the early days of *Laboratory Life* and of the electric-vehicle saga (Latour and Woolgar [1979] 1986; Callon 1980a, b; Callon 1981; Callon and Latour 1981). There are many reasons for this—one of them being that realism as a philosophical tradition has never been important on the Continent (see Bowker and Latour 1987 for other factors). But the main reason is that since, like C&Y, we wish to attack scientists' hegemony on the definition of nature, we have never wished to accept the essential source of their power: that is the very distribution between what is natural and what is social and the fixed allocation of ontological status that goes with it. We have never been interested in giving a social explanation of anything, but we want to explain society, of which the things, facts and artifacts, are major components. If our explanations are prosaic in the eyes of C&Y, it is OK with us, since we have always wanted to render our texts unsuitable for the social explanation genre. Our general symmetry principle is thus not to alternate between natural realism and social realism but to obtain nature and society as twin results of another activity, one that is more interesting for us. We call it network building, or collective things, or quasi-objects, or trials of force (Callon 1980b, 1987; Callon, Law, and Rip 1986; Latour 1987, 1988, 1990, in press, a; Law 1987); and others call it skill, forms of life, material practice (Lynch 1985; Shapin and Schaffer, 1985).

To position such a symmetry, we have to make a ninety-degree turn from the SSK yardstick and define a second dimension (see fig. 12.3). This vertical dimension has its origin, 0, right at the center of the other dimension. All the studies which are at the top of the stabilization gradient are the ones which make an a priori distinction between nature and society, that is, the ones that lack symmetry (in our sense) or that muddle the issue or try to hedge out of it. All the studies that are down the stabilization gradient do not make any assumption about the social or natural origin of entities. Such is our touchstone, the one that allows us to read most of SSK as "reactionary," because they start from a closed definition of the social and then use this repertoire as an explanation of nature—most of the time to no avail. For us they are exactly as reactionary as one who would start from an a priori unconstructed definition of nature in order to explain the settlement of controversies. On the contrary, we take as progressive any study that simultaneously shows the coproduction of society and nature. The phenomenon we wish to describe cannot be framed from the two extremes on the SSK yardstick—nature out there and society up there—since on the contrary, "natures" and "societies" are secreted as by-products of this circulation of quasi-objects (Shapin and Schaffer 1985; Callon 1981, 1987; Latour 1987, 1990).

We understand from reading this diagram (fig. 12.3)—admittedly crude, but in these matters the basic frameworks are always crude—why it is that a point A on the zigzag line which we try to study, once projected in A′ on the SSK yardstick is inevitably read as "reactionary," that is, as granting agency back to nature as defined by scientists. Conversely, we understand why point B, once projected

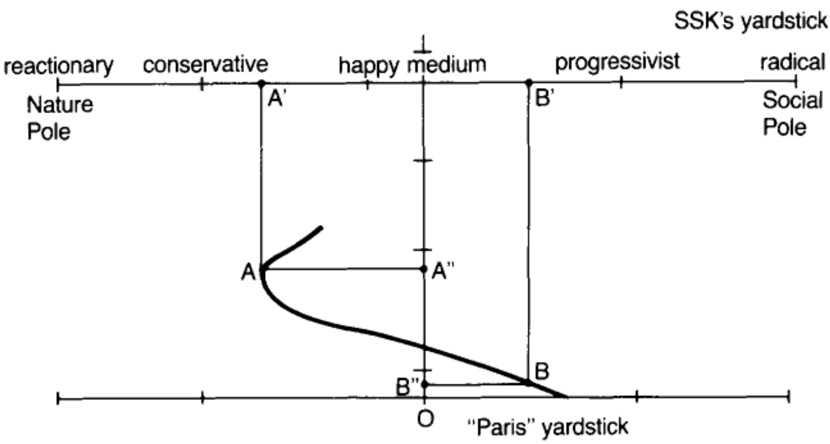

Figure 12.3 The one-dimensional yardstick of figure 1 is allowed to position any entity along the object-subject line (their longitude). The two-dimensional yardstick allows us to position objects and subjects according to their degree of stabilization as well (their latitude), and thus to offer for each entity two coordinates. Of each entity we would not only ask if it is natural or social (projected in A′ and B′ on the SSK yardstick) but also if it is unstable or stable (projected in A″ and B″ on the "Paris" yardstick).

in B′, is read, this time by realists, as a blatant proof of social constructivism, that is, of society defined by social scientists. The perfect symmetry in the misreading of our work by "natural realists" and by "social realists" alike is a nice confirmation that we are in a different, although for them unthinkable, position. After scores of criticisms coming from the left side of the diagram, we welcome C&Y's critique coming from the right side, because the two together triangulate our stand with great accuracy.

Here are the four main points of contention that make this stand unthinkable for the two squabbling schools:

1. With the horizontal yardstick, there are two and only two known and fixed repertoires of agencies which are stocked at the two extremities—brute material objects, on the one hand, and intentional social human subjects, on the other. Every other entity—gravitational waves, scallops, inscriptions, or door closers, to name a few—will be read as a *combination* or mixture of these two pure repertoires. On the contrary once the two axes are drawn together, there is an indefinite gradient of agencies which are not combinations of any pure forms—although the purification work may be also documented (Latour 1988, especially part 2). We do not have to start from a fixed repertoire of agencies but from the very act of distributing or dispatching agencies.

2. The horizontal yardstick is either human-centered or nature-centered with alternation between them. The vertical axis, however, is centered on the very activity of shifting out agencies—which is, by the way, the semiotic definition of an actant devoid of its logo- and anthropocentric connotations. The very distinction between "action" and "behavior" that seems so obvious to C&Y is exactly the sort of divide that no student of science is allowed to start from (chap. 4); the only possible starting point is the attribution of intention or the withdrawal thereof, two activities different in the effects they produce, but identical in the amount of work they require. It is as difficult to turn an object into "mere matter" as it is to grant intentionally to the action of a human—on this point again, Shapin and Schaffer (1985) have made the essential moves.

3. Along the horizontal axis, explanations flow from either or both extremes toward the middle. In the other frame of reference, explanations start from the vertical axis. This is because in the first frame, nature and society are the *causes* that are used to explain the delicate content of scientific activity. It is the opposite in our frame, since the activity of scientists and engineers and of all their human and nonhuman allies is the cause, of which various states of nature and societies are the consequences. It is highly probable that we will

never again get the extremes of the nature and society poles. Scientists and engineers never use them as complacently as C&Y imagine, and this is because they are much more original, daring and progressive social philosophers and social theorists than more social scientists are. Recapturing the scientists' and engineers' social innovations—for instance those of Pasteur (Latour 1988) or French electrochemists (Callon 1987)—is what we believe we should be credited for.

4. The definition of observables is entirely different in the two frames. In the first one, social scientists were allowed to use an unobservable state of society and a definition of social relations to account for scientific work—or to alternate by using an equally unobservable state of nature. In the other frame the only observables are the traces left by objects, arguments, skills, and tokens circulating through the collective. We never see either social relations or things. We may only document the circulation of network-tracing tokens, statements, and skills. This is so important that one of us made it the first principle of science studies (Latour 1987, chap. 1). Although we have not yet fully articulated this argument, it is the basis of our empirical methods.

Since the goals and methods are so far apart, is it a mere accident that our work has been likened to those of social relativists?

There would be no reason to even discuss our position and yardstick with "social" students of science if we were interested in incommensurable objects. However, our claim is that it is utterly impossible to achieve the social students' very goals—disputing scientists' hegemony, explaining the closure of controversies, applying Bloor's principle of symmetry, calculating the entrenchment mechanisms of cognitive networks—without shifting from the horizontal axis to the vertical one, that is, without *completing* their symmetry principle with ours. We did not come to this position for the fun of it or to play the deadly game of chicken, as we have been accused of doing, but because the field is cornered in a dead end from which we want to escape (Latour, in press, a). This debate occurs in social studies of science and technology and only there, since this is about the only place in social science where the number of border cases between "nature" and "society" is so great that it breaks the divide apart. Classical social theory, or philosophy of science, never faced this problem, since they ignored either the things or the society. C&Y claim to be able to study the fabulous proliferation of borderline cases without changing the yardstick that was invented in order to keep the pure forms as far apart as possible (Shapin and Schaffer 1985). We believe this to be philosophically ill-founded and empirically sterile. And this is why the discussion should now go from ontological framework to empirical evidence. C&Y think their empirical treatment of the controversies is sufficient and progressive, and that ours is reactionary and muddled. We believe that they will not—and have never—delivered the goods they claim to have delivered, and that our methods, although unsteady and incomplete, at least begin to approach the question we are all interested in. We claim that the former symmetry principle spoils the data obtained by all of the case studies by erecting in the middle a Berlin Wall as violent—and fortunately as fragile—as the real one.

The empirical disagreement, the only one that really matters, is visible in science, and still more in technology. C&Y have read and indeed have rewritten Callon's rendering of the network of scallops, scientists, and fishermen to prove that it is "reactionary"—in their frame of reference. What would they have done instead? (Let us remember that they call "symmetry" the alternation between the two poles of their frame of reference, and that inside ours we call it "asymmetry.")

> As a social account of the making of knowledge [Callon's scallop story] is prosaic because the story of the scallops themselves is an asymmetrical old-fashioned scientific story. A symmetrical SSK-type account would analyze the way it came to be agreed, first that the scallops did anchor, and second—at a later date—that they did not anchor. Into the analysis the question of whether or not the scallops complied would not enter. The informing assumption would be that whether there were more or fewer scallops anchoring early and late in the study did not affect the extent to which the scallops were seen to be anchoring early and late. No SSK study would rely on the complicity of the scallops; at best it could rely on human-centered accounts of the complicity of the scallops.

The whole field of social studies of science pioneered by Collins and several other social realists hinges on this: nonhumans should not enter an account of why humans come to agree what they are.

There are four empirical mistakes in this position that are increasingly serious:

First, the scientists Callon portrays are constantly trying to bring the scallops to bear on the debates among colleagues and among fishermen; they simultaneously entertain dozens of ontological positions going from "scallops are like that, it is a fact"; to "you made up the data"; through positions like "this is what you think the scallops do, not what they really do"; or "some scallops tend to support your position, others don't"; to "this is your account, not what it is." To pretend that to document the ways scientists bring in nonhumans, we sociologists should choose *one* of these positions—that scallops do not interfere at all in the debate among scientists striving to make scallops interfere in their debates—is not only counterintuitive but empirically stifling. It is indeed this absurd position that has made the whole field of SSK look ridiculous and lend itself to the "mere social" interpretation (Star 1988). The only viable position is for the analyst not to take any ontological position—especially social constructivism—and to observe how the importation of various scalloplike entities modifies the controversy. Of course C&Y cannot accept that, because their yardstick forces them either to go toward the "natural realism" that they "had learned to ignore" or to embrace "social realism." The agnostic symmetric position—in our sense—is for them unreachable. This is why they make the additional empirical mistake of believing that scientists must be "naive realists" in order to do their job. If scientists were naive realists about the facts they produce they would not produce any: they would just wait (Latour and Woolgar [1979] 1986; Latour 1987; Lynch 1985; Callon 1989; chap 2; chap 4; Pickering 1984). To portray scientists as bench realists is a revealing mistake. It could be understandable from sociologists who have never met or studied science in the making, but C&Y have, so it is not out of ignorance they make this blunder but out of the impossibility of their entertaining any status for entities other than these two: either the scallops are out there and force themselves on naive realists, or they are in there made of social relations of humans talking about them. The attribution of naive realism to scientists is the mirror image of the attribution to themselves of what we should call "naive socialism." With this divide of the data they entirely forget that scallops exist under various forms at the same time (probably none of them resembles "out-thereness") and that all the scientists are busy *not* limiting their discussion to social relations but devise hundreds of ways—yes inscriptions are one among many—to mobilize the various forms of scallops. Scientists never exist simply as people talking among people about people.

The second mistake is of a greater magnitude, since it bears on our attempt to overcome the first mistake. Since it is impossible to take only one of many ontological positions in order to account for the way scientists bring in nonhumans, we the analysts have to entertain the whole range. One way to do this is to extend our principle of symmetry to vocabulary and to decide that whatever term is used for humans, we will use it for nonhumans as well. It does not mean that we wish to extend intentionality to things, or mechanism to humans, but only that with any one attribute we should be able to depict the other. By doing this crisscrossing of the divide, we hope to overcome the difficulty of siding with one, and only one, of the camps. How do C&Y debunk this enterprise? By rewriting Callon's articles and breaking the symmetry of vocabulary Callon wants to use. In their notes, C&Y limit themselves to the "object pole" of their obsessional yardstick. They rewrite only what they see as the scallops side and triumphantly argue that, once rewritten, it makes no distinction at all between the old account of historians granting agency to things in themselves and Callon's account that crisscrosses the whole gradient of agencies by not limiting things to their "out-thereness." No wonder that if they rewrite "negotiation" as "discovery," or "actant" as "actor," it seems to make no difference. But the writing was crucial in allowing the passage of words through the Great Divide and back. Of course it is not crucial for C&Y, since they believe that they possess the right metalanguage to talk about science making—the language of things in themselves alternating with the language of humans among themselves—but it matters enormously to us since we believe the symmetric metalanguage should be invented that will avoid the absurdities due to the divide of two asymmetric vocabularies (divide which has been imposed to render the very activity of building society with facts and artifacts unthinkable). Of course our two articles would have been better if instead of using the same vocabulary for the two sides we could have used an unbiased vocabulary. But is it our fault that it does not exist? If "enroll" smacks of anthropomorphism, and "attach" of zoomorphism or of physimorphism? In the future we will forge and use this symmetric vocabulary, but in the meantime we wish to avoid the deleterious effect of alternation by borrowing what is acceptable on one side to show how it can be acclimatized on the other. Here again, actors are smarter than social scientists. The repertoires they use are hybrid and impure, whether they concern catalysts which become "poisoned," researchers who are "deprogrammed," or computers which are "bugged." One of the basic tasks for future studies of science and technology is to establish a symmetrical vocabulary. We should be credited with having tried to do so, and when no other solution was available, to have chosen a repertoire which bears no insult to nonhumans.

The third mistake is still more momentous. Our colleagues see the straw in our eye but not the beam in their own eyes. C&Y accuse us of not playing their game and of limiting the task of deciding what nature is to humans, and only to humans. This implies that they, or at least Collins (since Yearley had done discourse analysis before recanting it), are able to do this for their own case studies.

Such is the irony of their attack on our symmetry principle that Collins has never been able to live up to his own rule of the game. Gravity waves (Collins 1985) do indeed often appear in the settlement of controversies about them, but how do they appear? They leak surreptitiously through the account, as we will show in the last section. Collins alternates between an account where only humans talk among themselves about gravity waves and an account, supposedly left to the scientists, where gravity waves do most of the talking, or at least the writing. Extremely good at showing the opening of controversies, the indefinite negotiability of facts, the skill necessary to transport any matter of facts, the infinite regress of underdetermination, Collins has nothing to say about the closing of controversies, the non-negotiability of facts, and the slow routinization that redistributes skills; he simply shifts the burden to the Edinburgh school. No wonder, since he rejects all these problems as belonging to the natural realist—the other side of his alternation mechanism. Alternation is supposed to be the answer, but it is the most damning solution of all. This "Don Juanism of knowledge," as Nietzsche called it, cannot posture as a highly moral position. Don Juanism is a convenient way of avoiding the constraints of marriage and forgetting in one frame every tenet that was learned in the other; it cannot pass for a solution, not a moral one at least. We prefer not to alternate at all. Ironically, it is Collins's belief that he has achieved results different from those of the traditional historian which gives him the courage to dismiss our work, work which simply tries to achieve Collins's goal not only in intention but in effect. What is our position? We do not want to accept the respective roles granted to things and to humans. If we agree to follow the attribution of roles, the whole game opens up. In practice, no one is able, and Collins no more than any one else, to deny for good the presence of nonhumans in achieving consensus (natural realism), but neither can we make them play the part of a final arbiter who settles disputes for good (social realism). So why not modify the scenario once and for all? Nonhumans are party to all our disputes, but instead of being those closed, frozen, and estranged things-in-themselves whose part has been either exaggerated or downplayed, they are actants—open or closed, active or passive, wild or domesticated, far away or near, depending on the result of the interactions. When they enter the scene they are endowed with all the nonhuman powers that rationalists like them to have, as well as the warmth and uncertainty that social realists recognize in humans. But symmetrically, humans, instead of acting like humans-among-themselves whose part has been minimized or exaggerated, are granted all the powers of discussion, speech, and negotiation sociologists like them to have, but in addition they endorse the fate of all the nonhumans for whom rationalists and technologists are so concerned. The choice is simple: either we alternate between two absurdities or we redistribute actantial roles. It is not a question of asserting that there is no perceptible difference. The point is methodological. If we wish to follow a controversy through and to account for its possible closure in ways other than having recourse to the Edinburgh sociologists, then it must be accepted that the distribution of roles and competences should be left open. Are we to speak of intentionality, of behavior, of social competences, of interest or attachment? The answers are to be found mainly in the hands of scientists and engineers. Their work is exactly that of organizing and stabilizing these attributions and the classifications they lead to. Male baboons were seized by aggressive impulses before Strum arrived on the scene (Strum 1987); afterwards they were seen as manipulating social networks. To take the scientists' place in deciding on the distribution of actants' competences instead of following them in their work of constructing these competences is a methodological mistake and worse, a serious error of political judgment. Since differences are so visible, what needs to be understood is their construction, their transformations, their remarkable variety and mobility, in order to substitute a multiplicity of little local divides for one great divide. We do not deny differences; we refuse to consider them a priori and to hierarchize them once and for all. One is not born a scallop; one becomes one. A parallel could be drawn with studies on social classes or on gender differences. Who would dare to promote the idea that there are no differences between men and women or between the working class and the upper-middle class? Should these be considered differences of kind to be expressed in different repertoires? The recognition of the historicity of differences, their irreversibility, their disintegration, and their proliferation passes by way of a bitter struggle against the assertion of one great ahistorical difference.

But the fourth mistake is the most important, since it reveals the sheep behind the wolf's clothing. Several times in the paper, C&Y reject our appeal to a variety of hybrid nonhumans because we lack the scientific credentials:

> If we are really to enter scallop behavior into our explanatory equations, then Callon must demonstrate his scientific credentials. . . . There is not the slightest reason for us to accept his opinions on the nature of scallops if he is any less of a scallop scientist than the re-

> searchers he describes. In fact, we readers would prefer him to be *more* of a scallop expert than the others if he is to speak authoritatively on the subject. Is he an authority on scallops? Or did he merely report the scientists' views on the matter. . . . Certainly we do not have a study that can offer us any surprises about the natural world, or one that clarifies the credibility and authority of science. . . .
>
> . . . This backward step [of Latour] has happened as a consequence of the misconceived extension of symmetry that takes humans out of their pivotal role. If nonhumans are actants, then we need a way of determining their power. This is the business of scientists and technologists; it takes us directly back to the . . . conventional and prosaic accounts of the world from which we escaped in the early 1970s.

Callon is accused of not being a marine biologist, and he thus should not be able to talk about scallops at all—only about humans, his only terrain as a sociologist; Latour is accused of not knowing anything about technology; he should restrict himself to humans. In addition he is accused of not using the part of privileged knowledge he might have *qua* sociologist in the field of expert systems. This accusation coming from the heads of the scientific establishment is frequent. Why is it launched by sociologists of science? If they were Mertonian it would be acceptable, since Merton's tenet is to limit ourselves to the sociology of scientists and to leave science safely in the hands of the experts. But the accusation is leveled at us by sociologists who have fought for years against this limitation of sociology to social aspects, and who claim to explain the very content of science. Not only this; they also claim that they have to fight the hegemony of scientists' definition of nature! We have reached the limit of absurdity, and C&Y should be thanked for demonstrating so frankly that their fight against the hegemony of scientists over the definition of nature may be a game just as gratuitous as chicken. They never seriously believed that it was feasible. On the contrary, they accept 98 percent of the Great Divide: to the natural scientists the things, to the sociologists the remainder, that is, the humans. Either they are so deeply scientistic in their worldview that this whole enterprise is a way of defending science against attacks on their hegemony—but then what are their grounds for attacking us for doing what is an equally "reactionary" task?—or they really believe that they threaten scientists' privilege. How can this privilege be destroyed without granting sociologists the right to question the scientists' own definition of nature? Either C&Y are sheep in wolves' clothing or they chicken out of the fight. The most extravagant claim is that scientists' accounts of their own field are prosaic and boring. Have they ever seen a scientific field, ever approached a controversy, ever measured the lack of consensus, or ever felt the agitation and ranges of alternatives of professional engineers? We lack the scientific credentials, but there is one thing we can do: preserve the minority views for the benefit of the scientists themselves and preserve for the benefit of the outside public the range of alternatives on which scientists thrive. This is a much more efficient strategy for disputing hegemony than alternating between a mere social account and the condescending view that scientific practitioners are mere scientists. The beauty of studying science in action is that there is always enough dissent to let outsiders in and to offer observers with no scientific credentials a way of capturing the chaos of science. Strangely enough, we thought (until C&Y's paper came out, that is) that we had learned this lesson from Collins.

How can there be such a deep misunderstanding? How can they dismiss our work, which tries to get at the content of science and does not accept the privilege of the scientific definition of nature? Because that would mean abandoning their privilege, and that of social scientists in general, of defining the human world, the social world. And since, with their unidimensional yardstick, there is no other solution but alternating violently between two unsatisfactory explanations, they feel trapped, and their only way out is to deny that there is any difficulty or to make sure that alternative definitions are not endorsed by new students. We are not talking of intentions here, but of use and of effects, as C&Y rightly ask us to do. In effect, they are forbidding sociologists to document the vast diversity of positions entertained by scientists, either because scientists are supposed to have special access to nature and to be naive realists, or because sociologists have no scientific credentials and should stick to the human realms; this is an extraordinary step backward—since backwardness appears to be the issue. Forbidding such documentation is a serious error concerning the nature both of society and of scientific activity.

Technology is the shibboleth that tests the quality of science studies, because every mistake made in the science studies appears more blatant when we are studying technology. Like Callon's article on scallops, Latour's piece on mundane artifacts (In press b) aims at circulating through the Great Divide and deploying the whole gradient of entities from "pure" social relations to "mere" things, without giving any privilege to the two extremes. Like the piece on scallops, it is an ontological manifesto and a point about social theory. Just as scientists and fishermen in St. Brieuc Bay orchestrate a whole series of scallop-like entities, engineers and consumers delegate a whole gradient of social attributes to either humanlike entities or nonhuman entities. In the former article disputed by C&Y the point of departure is firmly positioned on the vertical axis of our diagram, which allows us to focus not on humans or nonhumans but on the activity of shifting, delegating, and distributing competences. In both articles the intention was not to say that scallops have voting power and will exercise it, or that door closers are entitled to social benefits and burial rites, but that a common vocabulary and a common ontology should be created by crisscrossing the divide by borrowing terms from one end to depict the other. Both articles carefully follow the large range of expressions, metaphysics, social theories, used by humans to account for the human-nonhuman associations; and both show that this gamut of expressions is much larger, more interesting, and more profound than the two vocabularies of things-in-themselves and humans-among-themselves that sociologists and technical determinists believe are necessary. But C&Y interpret the second article the same way as they did the first: Latour is accused of playing into the hands of the hated—but are they really hated?—technical determinists. He is also accused, and rightly so, of using the counterfactual method. Thought experiment is about the only way with which we can estrange ourselves from total familiarity with mundane artifacts. We agree that this cannot be the solution and that many better methods should be developed, and indeed have (Akrich 1987; Latour, Mauguin, and Teil, in press; Latour, in press, c), but the point of the paper is clearly how to see and position artifacts—and this is indeed what most of the critiques of C&Y address.

Apart from their witty critique of counterfactual methods, our colleagues show still more clearly in their analysis of the second article their scientistic worldview. They start with an absolute dichotomy between purposeful action and mindless material behavior. Then they state that "it is clear that the interpretative method [used for intentional humans] is unusable, since doors have no social life in which we could participate." The matter-of-fact tone of this extraordinary claim could not be more clearly at odds with the social theory we have developed over the years (Callon 1980b, 1987; Callon and Latour 1981; Strum and Latour 1987; Law 1987). There is no thinkable social life without the participation—in all the meanings of the word—of nonhumans, and especially machines and artifacts. Without them we would live like baboons (Strum 1987). Technology is not far from the social realm in the hands of the technologists: it is social relations viewed in their durability, in their cohesion. It is utterly impossible to think for even a minute about social relations without mediating them with hundreds of entities. Of course these nonhuman entities may be dismissed—they are indeed ignored by most social theorists, even by those like Barnes (1988), who should know more about science studies—but our point is that the activity of dismissing them, of disattributing meaning and will, is as difficult, as contentious, and as revealing as the attribution of meaning, will, and intentionality to humans. Although we can waffle on the complete unification of nature and society, which we claim is our only object of study, there is no possible hesitation when dealing with artifacts, since they are man-made. Scientists may be realists on the cold and established part of their science, but engineers are constructivists about the artifacts they construct. The weight of efficiency is much lighter than that of truth—and has a less prestigious philosophical pedigree. Hence the prolongation of the use of the unidimensional yardstick in technology is less easy to forgive than in science, where after all, we cannot ask sociologists to undo the enormous preparatory work philosophers of science have done for them.

This is not the view taken by C&Y, to say the least. They take it as their brief, and moral high ground, to differentiate clearly between what humans are able to do—purposes, intentions, common sense, negotiating the rules, infinite regress—and what the machines have always been limited to doing—lacking common sense, brutish, material, asocial, and rule-bound. This is a respectable position if we are engaged in a humanistic fight against the technologists' hypes, but is uninteresting as an empirical tool to describe the daily negotiation of engineers to redistribute these very characterizations via the artifacts. As long as social scientists safely stuck to social relations—power, institutions, classes, interactions, and so forth—they might have considered artifact making as a sort of borderline case which could be put out of the picture of society. But how can we do this with sociotechnical imbroglios where every case is a borderline case? Either C&Y want to keep their yardstick alternating from mere matter to intentional humans, in which case they should study a domain other than technology, or they are interested in accounting for this activity and should abandon the worst possible standard to size it up. If they dare to say "perish the case studies as long as the moral and humanistic yardstick that allows us to extirpate social relations from mere things is safe," they can't possibly accuse us for looking for other empirical programs.

Our empirical program does not claim either that humans and artifacts are exactly the same or that they are radically different. We leave this question entirely open. A speed bump—aptly called a sleeping policeman—is neither the same as a standing policeman, nor is it the same as a sign Slow Down, nor is it the same as the incorporated caution British drivers are supposed to learn culturally from birth. What is interesting, though, is that campus managers decided to shift the program of action "slow down cars on campus" from a culturally learned action to a mere piece of behavior—the physical shock of concrete bumps on the suspension of the cars. The program of action: "Slow down please for the sake of your fellow humans" has been translated into another one: "protect your own suspension for your own benefit." Are we not allowed to follow this translation through? Who made the move from action to behavior, from meaning to force, from culture to nature? We the analysts or they, the analyzed? Who or what is now enforcing the law, the standing or the sleeping policeman? Who are supposed to have sociality embedded in themselves, the talking humans or the silent road bumper? To claim that only the humans have meaning and intentionality and are able to renegotiate the rules indefinitely is an empty claim, since this is the very reason why the engineers, tired of the indiscipline and indefinite renegotiability of drivers, shifted their program of action to decrease this pliability. By insisting on alternation, Collins can no more explain the closure of technical controversies than the closure of scientific controversies. If engineers as well as scientists are crisscrossing the very boundaries that sociologists claim cannot be passed over, we prefer to abandon the sociologists and to follow our informants.

Exactly as for science, C&Y claim that every time you appeal to the artifacts' action you have to use the technological-determinist vocabulary. This is not only a wrong interpretation of our work, it is wrong of engineers. There is a constant thread in C&Y's papers that if you document only scientists' and engineers' accounts it will be prosaic, conventional, unsurprising, uninformative, and merely technical and rule bound. Again, this portrayal of scientific activity would not be surprising from a Habermassian philosopher or from an Ellulian technophobe, but it is very surprising from social scientists who have intimate knowledge of scientific controversies. If there is one striking element in science studies—and if there is one piece of news in what we have all written—it is the amazing diversity, the liveliness, and the heterogeneity of science and engineering (even in its most deadly tasks, as can be seen in MacKenzie 1990). It is precisely because there is no such thing as "a science" with authority and complete prosaic totalitarian dominion over nature that it is so easy for us as social scientists to tread into and to demonstrate the lack of hegemony and the rich confusion between the humans and nonhumans that make up our collective.

We do not claim that our theories are right. We are looking for collaborations with English and American scholars to make them better, and in doing so we will help to achieve their goals as well as ours. But C&Y resist this enrollment; they feel they have the right to dismiss our work because they have already provided an explanation, and that our attempts are belated and muddled. This is why they accuse us of merely rephrasing the problems through the catchall network vocabulary and of not providing an explanation of the closure of controversies. This implies that they have explained something in science studies.

The accusation of not explaining things is always tricky in social science, because it ends up in a Lebanese situation, with everyone looking at the strength of the other's explanation and destroying it. In SSK it is still more difficult, because the whole pattern of "providing an explanation through the use of causes" has been largely disputed for the natural sciences (Lynch 1985; Woolgar 1988b; Collins 1985; chap. 2; Latour 1987), which makes their reimportation into the sociology of science a rather difficult job. Moreover, explanations might not be desirable after all (Woolgar 1988a). A complete description of network dynamics might provide a better explanation, in the end, than the delusive search for causes (Latour, Mauguin, and Teil, in press). Although it would take too long to argue those points, it is possible to compare our pattern of description with Collins's, especially his most elaborate work, *Changing Order* (1985), to see if he really has the grounds to discount our offer to help him out of his quandary.

Like us, Collins is better at description than explanation, but in the end of his book he feels obliged to provide a closure mechanism, and it is not uncharitable to find out how much better he is than us. His intellectual resources come from a network theory, which is not without resemblance to ours, the only difference being that we have taken ten years to document, quantify, justify, and argue it (Callon, Law, and Rip 1986; Callon, Courtial, and Lavergne 1989; Callon, Laredo, and Rabeharisoa, in press) and that Collins uses a few pages of metaphors to get rid of the problem. After describing for a hundred pages the experimenter's regress—which is a nice exemplification of Duhem's thesis that there is no *experimentia crucis*—Collins ties Weber's decision to quit the controversy to Mary Hesse's network theory: "a kind of spider's web of concepts" (1985, 131). Hesse's networks have the interesting property of explaining the choice of a theory through the notion of "entrenchment" (Law and Lodge 1984). Collins adds to this the important metaphor of "reverberation": "The point is that the whole network is mutually supporting since everything is linked to everything else. But, by virtue of the way that everything is connected a change in one link might reverberate through the whole of the network." (1985, 131)

Although this might sound like the diagnosis of one of Molière's physicians, it is OK with us if it means that in the end the solidity of a claim will be the exact measure of the resistance to a test of strength of the whole network. Hacking (chap. 2) uses a similar argument, although he provides a much richer vocabulary than Collins to account for the reverberation and the entrenchment of a claim. Once we abandon the twin resources of nature and society, we are all, it seems, looking for the same "explanation"—the stabilization of Hesse's or some other associationist network—but we disagree on what a network is made of and how to empirically calculate or account for the test.

Here Collins makes his first crucial mistake. Instead of seeking a genuine network theory—and testing the strength of a claim by operationalizing Hesse's qualitative arguments as we have done through hundreds of pages of programming language (see especially the programs Leximappe™, Lexinet™, Candide™) he reintroduces the division between social and cognitive nets with no better metaphor than that of a coin. "And just as social relations can be described in terms of social networks, their cognitive counterparts can be described in terms of Hesse net. The Hesse net and the network of interactions in society are but two sides of the same coin. To understand each, one must understand both" (132). Although the whole task is to pay the philosophical, sociological, economic, and computer price of this fusion of the two types of network—cognitive and social—Collins hedges the issue by saying that they are both different and the same and that they furthermore reflect each other, as in the crudest Marxist reflection theory.

But the second mistake is more damning, since the three notions of entrenchment, reverberation, and wider network are now used to explain the stabilization from the outside:

> The scientists, then, are faced with a choice (albeit, a highly constrained choice); at what level of inference, or externality, do they report their results? The more inferences they make the more interesting the results are to a wider and wider audience—the more they rattle the spider's web of concepts, as it were. But, if the results are not likely to preserve everyone's "socially acceptable conceptualizations of the natural world," then the more inferences they make, the

more bits of taken-for-granted reality they are threatening, and the more trouble they are going to cause" (138).

It is amusing that C&Y deride our technical use of the notion of obligatory passage point and deem the rattling of spiders a better explanation. But it is not amusing at all to see that the good old society is imported—through the spider metaphor—to brutally close the infinite experimenter's regress: "Networks ramify continuously so that reverberations induced within science have their effects outside just as influences from outside the scientific profession feed back into science proper. Science and technology are affected in quite straightforward ways by political climate." (165)

Merton would have been much more specific, much more mediated, much less "straightforward." Are these authors the same ones who mock our translation theory, which accounts with precision for the successive shifts from one repertoire (exoteric) to the others (esoteric) (Latour 1987); the same ones who deride the "quali-quantitative" work that enables us to follow in detail how politics and science might "reverberate" in each other? Yes, and they prefer empty metaphors of spider and coin to network theory because this is the only way to save their classic view of society as what abruptly puts a stop to the indefinite negotiability of scientists—a nice case of entrenchment indeed.

As long as he is in the laboratory looking for replication procedures, Collins is like Woolgar—stressing the indefinite pliability and endless negotiability of everything—but when he wishes to finish his book and closes Weber's story, he has no other issue but to jump to an Edinburgh type of interest theory: the winner will be the one who reverberates less (or more) through the entrenched interests of the wider society. (It is because of this contradiction that Collins attacks Woolgar in the same paper where he attacks us.) Duhem or Woolgar for the inner core, Edinburgh and Marxism for the outside and in the middle a free decision that scientists make for no reason at all, in the most complete arbitrariness. What scholars like Law, Lynch, Knorr, Hacking, Jardine, Schaffer and us have shown over and over again, that is, the slow accumulation of calibrated gestures, black boxes, and routinized skills which are more and more difficult to modify, is transformed by Collins in a sudden decision to give up in a fight where nothing had force. Instead of being slowly beaten by uppercut after uppercut, the boxer Weber is touched by feathers, none of them with any weight, and suddenly he falls knocked out without any reason whatsoever, since he could have gone on indefinitely negotiating with his adversaries: "In retrospect, Weber would have served his case better to have maintained his refusal to use electrostatic calibration—not just because the results proved unfavorable but because the assumptions taken on board by the act of calibration and the restrictions of interpretation imposed as a result" (103).

Such sentences, which combine Whiggish history ("in retrospect"), natural realism ("results proved unfavorable"), and decisionism ("would have maintained his case better") are an indicator that Collins, because he is unable to solve the link between laboratory negotiation and the wider society, may never have described in a satisfactory way what we all credit him for, that is, controversies. And this is why he is so unable to understand us. We take up the job where he leaves off. All our work aims at defining the thread in the spider's mouth, the dozens of intermediaries that slowly make Weber unable to move, the uppercuts that one after another bring him down. Instead of the empty claim that Weber should have maintained his refusal—which is like chiding a boxer after the countdown for not having stood up—we multiply the texts, the inscriptions, the instruments, the skills, the nonhumans, none of which has a decisive weight, it is true, but all of which, mobilized together, woven together, are enough to transform the indefinite pliability of a situation into an irreversible fact. Wherever we devise a hybrid that carries *some* weight—the mass spectrometer of the TRF story, the immutable mobiles, the spokesperson, the texts—Collins misunderstands us and accuses us vehemently of bringing nature back in. No, we are explaining in detail what he is unable to explain, how and why a spider makes a web, how and why one scientist is better than another, how and why a boxer is knocked out by another one. We do not want to alternate between negotiation and entrenchment. We do not feel that Collins has yet delivered the goods he claims to have delivered which allow him to get rid of us. We still want to help him out and study with him not only what he is good at: the beginning of controversy when reversibility is large and skills uncodified, but also what he is so bad at: the gradual closing of controversy and the reshuffling of the networks. We feel that if we worked together we might begin to sketch a description of society-science.

Why can't Collins understand us? Here is the core of our ethical and political disagreement. The only way for Collins to debunk scientists' hegemony is to portray them through this alternated three-stage situation:

1. Indefinite negotiation
2. Complete social determinism (delegated to the Edinburgh sociologists)
3. Free decision.

It is only if scientists are portrayed in this way that they lose the right to a special relation with nature and thus free social scientists from the domination of the natural sciences. "Do as you like, but if you close a dispute, it is not because of nature, but by alternation between free decision and social constraints. Thus you have no upper ground to invade our social realm. Stay where you are, we will stay where we are. You do not have nature on your side, so do not criticize us." Collins's solution is a good old Kantian divide. By contrast, our paradigm is twice reactionary in the eyes of Collins: first, we believe that scientists close controversies for many other reasons than arbitrariness or social entrenchment; second, we do not believe that social scientists should be left to themselves. Since we believe that there are many other ways to dispute scientists' hegemony—the first one being to dispute the very distribution of agencies between the things-in-themselves and the humans-among-themselves—we cannot be content with this resurrection of Kant's justice of peace (see Latour, in press, a, for details).

From this disagreement, however, we do not draw the same conclusion as C&Y do. They claim that our program is entirely misguided, reactionary, if not in intent, at least in use, and that it should be not followed all the way. We believe, on the contrary, that although it is experimental, uncertain, and incomplete, it should be carried all the way, with the help of the many clever and excellent scholars inspired by the various science studies schools, and that this new move will vindicate most of Collins's discoveries and insights by freeing them from their most blatant limitations. They want to throw us out. We want to change the water, but to keep the Bath baby in, since it is also *our* baby. After all, having children, even through Don Juan's immoral alternating strategy, is more fertile than playing chicken.

REFERENCES

Akrich, Madeleine. 1987. Comment décrire les objets techniques. *Technique et Culture* 5:49–63.

Ashmore, Malcolm. 1989. *The Reflexive Thesis: Wrighting Sociology of Scientific Knowledge.* Chicago: University of Chicago Press.

Barnes, Barry. 1988. *The Nature of Power.* Cambridge: Polity Press.

Bijker, Wiebe E., Thomas Hughes, and Trevor Pinch, eds. 1987. *New Developments in the Social Studies of Technology.* Cambridge: MIT Press.

Bijker, W., and J. Law, eds. In press. *Constructing Networks and Systems.* Cambridge: MIT Press.

Bowker, Geof, and Bruno Latour. 1987. A Booming Discipline Short of Discipline. *Social Studies of Science* 17:715–48.

Callon, Michel. 1980a. Struggles and Negotiations to Decide What Is Problematic and What Is Not the Sociology of Translation. In K. Knorr, P. Krohn, and R. Whitley, 197–220.

———. 1980b. "The State and technical Innovation: A Case Study of the Electrical Vehicle in France" *Research Policy* 9:358–76.

———. 1981. Pour une sociologie des controverses techniques. *Fundamenta Scientiae* 2:381–99.

———. 1986. Some Elements of a Sociology of Translation: Domestication of the Scallops and the Fishermen of St. Brieux Bay. In Law 1986b, 196–229.

———. 1987. Society in the Making: The Study of Technology as a Tool for Sociological Analysis. In Bijker et al. In press, 83–106.

Callon, Michel, ed. 1989. *La science et ses réseaux: Genèse et circulation des faits scientifiques.* La Découverte Coll. Anthropologie de la science, Paris.

Callon, Michel, and Bruno Latour. 1981. Unscrewing the Big Leviathan: How Do Actors Macrostructure Reality. In Knorr and Cicourel 1981, 277–303.

Callon, M, John Law, and Arie Rip, eds. 1986. *Mapping the Dynamics of Science and Technology.* London: Macmillan.

Callon, Michel, Jean-Pierre Courtial, Françoise Lavergne. 1989. *Co-Word Analysis: A Tool for the Evaluation of Public Research Policy: The Case of Polymers.* Report for the NSF grant PRA no.85 12-982, Paris.

Callon, Michel, Phillippe Laredo, and Vololona Rabeharisoa. In press. The Management and Evaluation of Technology: The Case of AFME. *Research Policy.*

Collins, Harry. 1985. *Changing Order: Replication and Induction in Scientific Practice.* Los Angeles: Sage.

Knorr, Karin. 1981. *The Manufacture of Knowledge: An Essay on the Constructivist and Contextual Nature of Science.* Oxford: Pergamon.

Knorr, Karin, and Aron Cicourel eds. 1981. *Advances in Social Theory and Methodology toward an Integration of Micro and Macro Sociologies.* London: Routledge and Kegan Paul.

Knorr, Karin, Roger Krohn, and Richard Whitley, eds. 1981. *The Social Process of Scientific Investigation.* Dordrecht: Reidel.

Latour, Bruno. 1987. *Science in Action: How to Follow Scientists and Engineers through Society.* Cambridge: Harvard University Press.

———. 1988. *The Pasteurization of France.* Cambridge: Harvard University Press.

———. 1990. Postmodern? No Simply Amodern. Steps towards an Anthropology of Science: An Essay Review. *Studies in the History and Philosophy of Science* 21:145–71.

———. In press a. One More Turn after the Social Turn: Easing Science Studies into the Non-Modern World. In E. McMullin, ed. *The Social Dimensions of Science.* Notre Dame: Notre Dame University Press.

———. In press b. Where are the Missing Masses? Sociology of a Few Mundane Artefacts. In Bijker and Law,

———. In press c. *Aramis ou l'amour des Techniques.* Paris: La Découverte.

Latour, Bruno, and Jim Johnson. 1988. Mixing Humans with Non-Humans: Sociology of a Door-Opener. *Social Problems* (special issue on sociology of science, ed. Leigh Star) 35: 298–310.

Latour, Bruno, Philippe Mauguin, and Geneviève Teil. In press. A Note on Socio-Technical Graphs. *Social Studies of Science.*

Latour, Bruno, and Shirley Strum. Human Social Origins: Please Tell Us Another Origin Story! *Journal of Biological and Social Structures* 9: 169–87.

Latour, Bruno, and Steve Woolgar. [1979] 1986. *Laboratory Life: The Construction of Scientific Facts.* Princeton: Princeton University Press.

Law, John. 1986a. On the Methods of Long-Distance Control: Vessels, Navigation, and the Portuguese Route to India. In Law 1986b, 234–63.

———. 1987. Technology and Heterogeneous Engineering: The Case of the Portuguese Expansion. In Bijker et al. 1986, 111–34.

Law, John, ed. 1986. *Power, Action and Belief: A New Sociology of Knowledge?* Keele Sociological Review Monograph, Keele.

Law, John, and Peter Lodge. 1984. *Science for Social Scientists.* London: Macmillan.

Lynch, Michael. 1985. *Art and Artifact in Laboratory Science: A Study of Shop Work and Shop Talk in a Research Laboratory.* London: Routledge.

MacKenzie, Don. 1990. *Inventing Accuracy.* Cambridge: MIT Press.

Pickering, Andrew. 1984. *Constructing Quarks: A Sociological History of Particle Physics.* Chicago: University of Chicago Press.

Shapin, Steven, and Simon Schaffer. 1985. *Leviathon and the Air Pump.* Princeton: Princeton University Press.

Star, Leigh. 1988. Introduction: Special Issue on Sociology of Science and Technology. *Social Problems* 35: 197–205.

Strum, Shirley. 1987. *Almost Human: A Journey into the World of Baboons.* New York: Random House.

Strum, Shirley, and Bruno Latour. 1987. The Meanings of Social: From Baboons to Humans. *Social Science Information* 26: 783–802.

Woolgar, Steve. 1988a. *Knowledge and Reflexivity: New Frontiers in the Sociology of Knowledge.* London: Sage.

———. 1988b. *Science: The Very Idea.* London: Tavistock.

SCIENCE AS PRACTICE AND CULTURE

EDITED BY ANDREW PICKERING

THE UNIVERSITY OF CHICAGO PRESS
Chicago and London

ANDREW PICKERING is associate professor in the Department of Sociology and the Unit for Criticism and Interpretive Theory at the University of Illinois, Urbana-Champaign. He is the author of *Constructing Quarks: A Sociological History of Particle Physics*, published by the University of Chicago Press.

The University of Chicago Press, Chicago 60637
The University of Chicago Press, Ltd., London

Printed in the United States of America

00 99 98 97 96 95 94 93 92 5 4 3 2 1

ISBN (cloth): 0-226-66800-2
ISBN (paper): 0-226-66801-0

Library of Congress Cataloging-in-Publication Data
Science as practice and culture / edited by Andrew Pickering.
p. cm.
Includes bibliographical references and index.
1. Science—Social aspects. 2. Knowledge, Theory of.
I. Pickering, Andrew.
Q175.5.S3495 1992
303.48'3—dc20 91-28829

∞ The paper used in this publication meets the minimum requirements of the American National Standard for Information Sciences—Permanence of Paper for Printed Library Materials, ANSI Z39.48-1984.

CONTENTS