culture as abstract cultural practices that impinge on the practices of the likes of Cavendish.

Acknowledgments
I want to express my gratitude to Andy Pickering and to Jed Buchwald for comments that greatly improved this paper.

in: Buckwald, Jed Z. (ed.)
Scientific practice: theories and stories of doing physics.
Chicago: Chicago University Press,
1995, p. 123-148

SIX

FOLLOWING SCIENTISTS THROUGH SOCIETY? YES, BUT AT ARM'S LENGTH!

Yves Gingras

> All science would be superfluous if the outward appearance and the essence of things directly coincided.
>
> *Karl Marx,* Capital, *volume 3*

OVER THE LAST 15 YEARS, sociologists of science have produced a great number of fascinating, fine-grained analyses of scientific practice, emphasizing the variability of methods and practices of scientists and engineers as well as the contingency of the results or conclusions they obtain. The main agenda of this research program was to counter the then dominant conception that scientific knowledge was simply the end product of the application of logic, observation, and experimentation, the results of which were somehow an adequate representation of the real world existing outside and independently of the scientist. The microanalysis of scientific experiments and controversies has shown that these practices are in fact quite complex and should not be taken to constitute an *unproblematic* bedrock on which science is once and for all securely founded. It is a great merit of the constructivist approach to have shown philosophers that simple models based on an abstract "rationality of science" must be abandoned in favor of much more contextualized and dynamic models of scientific practice.

That being said, it is important to note that one of the striking features of the constructivist[1] literature, particularly in recent years, has been the proliferation of code words or buzzwords and "principles" supposedly necessary to understand scientific practice: "seamless web," "heterogeneous engineering," "actor-network," "actant," "black box," and, to mention an example that has probably not yet reached the English-speaking

1. Though one should distinguish between many species of "relativists" and "constructivists," the term "constructivist" will be retained here for the sake of simplicity. It applies to all the literature discussed in this paper.

world, "*investissement de forme.*" As for the "principles" invoked, they are considered self-evident, and mentioning that they have been *transgressed* in an analysis now seems sufficient to justify discarding its results without further comment. The best-known example is certainly the "principle of symmetry." Whereas it has been widely discussed and is very useful as a *heuristic* device in sociological practice (though its *epistemological* status is controversial), "extensions" of it (such as the symmetry between animate and inanimate or between nature and society) appear without much discussion or justification, like the curious idea that "the explanation has to be at least as rich as the content" that is to be explained (Latour 1988a, 258). Frankly, I just do not see any reason to limit a priori the kind of explanation to be offered and would not be averse to accepting a "simple" explanation for a "complex" phenomenon if it were convincing. The case of chaos theory is a perfect example of the possibility of explaining the complex behavior of systems by using simple dynamical equations (see, e.g., May 1976). Latour's statement is even more problematic if we keep in mind that the categories of "rich" and "complex" and "simple" are not given or self-evident but, if I may say so, "socially constructed." Finally, other kinds of statements in the literature appear as principles but are actually more like incantations—for example, the habit of stating that "the technical and the social cannot be distinguished." These statements are often found in the introductions and conclusions of papers that describe case studies making many such supposedly impossible distinctions.

The problem about all this is not that we do not need specific concepts to study science and technology from a sociological point of view. It is rather that the dynamic of exchange in this field has been such that there is now a cacophony of discourse and ideas that makes it difficult to pinpoint, understand, and evaluate the various views put forward by different "schools." In their criticisms of Latour and Callon, Collins and Yearly also note this problem of interpretation. They even confess: "Over the years [they] have found difficulty in taking seriously the more flamboyant statements of the Actant Network School at face value but, *fearing to appear foolish,* [they] have kept quiet" (1992, 370, emphasis added).

This confusion partly results from the tendency to juxtapose many of these buzzwords, principles, and incantations without regard to their consistency. One of the objects of this paper is to show that consistency and clarity have not always been characteristics of the pronunciamentos found in the papers which use the "actor-network" or "heterogeneous-engineering" language. To do so, I will look at the way these notions are presented, defended, and used in the narratives of empirical case studies. After this critical part, which takes up the first five sections of the paper, I will propose a sociological model of the practice of science that is reflexive, is consistent with our present knowledge of the microdynamics of re-

search, and takes into account structural constraints on scientific practice that are invisible at the microlevel of analysis. Only in this way can *invariants* be recognized in the infinite variations observed in the many microhistories of controversies and of scientific practice.

1. Heterogeneous or Homogeneous?

Nowadays, no "serious" paper in sociology of science can begin without *stating* (not *arguing*) that "technical, social, economic, etc., factors are inextricably bound together." The most developed positions in this regard are no doubt the so-called actor-network and heterogeneous-engineering approaches, which try to take seriously the impossibility of discriminating between the kinds of objects and factors that enter into the dynamic of scientific activity. For Callon and Latour, for example, not only can we not distinguish the "social" from the "technical" but even the distinction between animate and inanimate actors or objects is arbitrary—a turn to hylozoism brilliantly analyzed by Simon Schaffer (1991).

The first point to make concerning the proclaimed "indistinctness" of the entities usually referred to as "science" and "technology" is that if it is to be taken seriously, then one can hardly see how one could even talk about science and technology. It should go without saying that it is impossible to write or even think without making distinctions and that even if in the "real world" everything were in everything, any analyst (as the term suggests) would have to make distinctions to describe, analyze, or comment on a situation. This is why vocabularies devised to try to convey the idea that "everything is in everything" engender some confusion and, as we shall see, do not really reflect the content of the case studies they preface (or follow).

Take the word "heterogeneous." In conventional dictionaries it refers to objects that are different, distinct, and separated. A heterogeneous mixture of liquids is one in which the two liquids do not mix together—oil and water, for example. So it is more than curious to see those who want to convey the idea that social, technical, animate, and inanimate cannot be distinguished a priori since they form an "organic whole" (Callon 1987, 84) use the term "heterogeneous engineering," which suggests exactly the contrary. Rather, one would have guessed that they would have come up with, say, something like "homogeneous engineering." The use of "heterogeneous engineering" can only mean that all the factors involved are *distinct* and brought together in a whole that may *then* become a "seamless web." On the Cartesian scale of "clear and distinct ideas" Thomas P. Hughes (1983) is more consistent than Law or Callon for he explicitly distinguishes between animate, inanimate, political parties, engineering companies, and so on, and shows how all these components have been *linked and transformed into* a "seamless web" in which it has become difficult to disconnect the social from the technical. In other words, for Hughes, the seamless web is the *result* of the process of constructing systems.

Since one simply could not write without making distinctions, it is not surprising to see that in all the narratives constructed to understand any scientific or technological development, authors constantly distinguish between factors that are not supposed to be distinct. Since Law and Callon have been the most forceful to voice this "indistinctness" between various categories (social, technical, political, and now economical), it is fair to look at their joint paper devoted to the analysis of a British military aircraft project to see how they manage to hold to their "principles."

As usual, the introductory manifesto notes that "any attempt to separate the social from the non-social not only breaks the methodological principle of following the technologists. It is also, quite simply, impossible because the social runs throughout the technical and thus cannot be separated from it" (Law and Callon 1988, 285). The first problem here is that we are never told if the impossibility of separating these factors comes from the fact that *in practice* (at the ontological level) they are linked or mixed together and/or that these *analytical tools* are not useful in understanding this reality (the epistemological level). One can frequently *distinguish* factors that cannot always be *separated.* The second problem with this position is that the authors in fact describe the TRS 2 project and explain that its future "depended on *two factors.* On the one hand, it was important to *demonstrate the technical competence* of the project and the best way to do that was to have a *successful maiden flight.* . . . On the other hand, the *outcome of the general election was also vital.* Conservative success would assure the future of the project. Labour victory would call it into question" (293, emphasis added). For an "average" political scientist or sociologist this analysis would pose no problem, for it is quite traditional. These potential (and ideal) readers would also see nothing unusual in the fact that the experimental flight "was highly successful, the aircraft handled well and there was no hint of the destructive resonance that had plagued the engine" (293). They could also easily understand that opposing political parties had different priorities so that once elected, the Labour Party, "beset by *economic* problems, quickly ordered a detailed scrutiny of the various military aircraft projects" (293, emphasis added). They would find the detailed description of the events and negotiations that led to the cancellation of the program to be very interesting: the Treasury Board was against the project, the minister of defence was in favor of an American F-111, and so on. In short, they would recognize in the narrative the usual objects and actors making up society (Labour Party, Treasury Board, cabinet ministers, etc.) and conclude that there was

nothing unusual or startling in all that. And even "traditional" sociologists or political scientists would accept their conclusion that "the project went through *different phases,* some of which were *more technical in character,* while others were *more political*" (295, emphasis added). Finally, they would also probably be convinced that, given all the factors involved, "the development of the project was contingent." At least I was and cannot see why the authors suggest in their introduction that their analysis will "lead to a conclusion that is *counter-intuitive for many sociologists*" (284, emphasis added). Again, beyond the self-proclaimed "radical" or "counter-intuitive" character of the actor-network approach, the narratives produced are fairly traditional.

To an outsider to the ongoing debates among sociologists of science, the traditional character of these descriptions and narrations (and other case studies would show the same structure) is plain, and the extracts quoted above are sufficient to show that the authors constantly make clear-cut distinctions between many kinds of objects and actors in order to make sense of the "world" they analyze.[2] Thus, not only do they locate two "political parties" in their story but they also isolate "two factors" important for the future of the project, one being "a general election," an object quite familiar to political scientists and even sociologists. Since this choice of factors was made among what is a priori a great number of possible elements that should not be distinguished, the authors did not apply their "methodological adage" (Law and Callon 1988, 285) very well and even forgot to remember Callon's order that none of the ingredients "can be placed in a hierarchy or distinguished according to its nature" (Callon 1987, 86). Moreover, in the *temporal succession* of events, where discontinuity is admittedly more problematic than between kinds of actors or objects, they even distinguished between "more technical" and "more political" phases.

In his paper on the Portuguese maritime expansion, John Law also shows his capacity to constantly distinguish (at least analytically) the social from the technical: talking about the set of relatively stable associations between the cannon, the ship, the master, etc., Law writes that "some of [the] hostile forces were physical (the oceans), while others were social (the Muslims)" (Law 1987a, 247). Note also that physical reality

2. Of course, one is always open to the charge of quoting out of context—the usual and easy argument. To this argument I can only respond by suggesting that the reader read slowly and carefully the papers referred to, applying to them the method of highlighting the traditional distinctions that are constantly made in the course of the narrative, as well as the words used to denote causality or to identify the subject of action; all this without paying attention to or being impressed by the introductory manifestos, which try to orient the reader by already imposing on the text an interpretation that is in fact inconsistent with its grammatical and semantic content.

("oceans") is introduced as an *explanatory* factor on the same level as social factors in the good old realist tradition, which has never been shy of combining social and technical factors to explain a given situation.

So, if there is anything "counter-intuitive" in their paper it is probably the fact that the reader is uneasy in the face of statements of principles that are contradicted in the main body of the text. How are we to explain the insistence in some quarters to repeat constantly, in introductions and conclusions, statements of principles that are not applied in the main body of the text? Either the authors mean what they say or they don't. Let us suggest some possible interpretations.

2. Do They Mean What They Say?

A charitable interpretation of this apparent contradiction is to suppose that they do not really mean what they say and that the contradiction between the content of the analysis and the introductory statements can be attributed to a lack of clarity: by saying that one cannot distinguish between all the elements intervening in the dynamics of science or technology, they only mean that science and technology are not done in a vacuum but with many other "tools" and that one can never say in advance how the actors will combine these different (thus "heterogeneous") elements; they just want us to see that there are complex relations between objects and actors and that neither technological determinism nor social determinism can explain the development of science and technology. To take the analogy of a cake, it is clear that once it is baked the chocolate cake is perfectly homogeneous, and a child may be convinced that it is not composed of several "heterogeneous" ingredients mixed in definite proportions.[3] But the cook knows that despite appearances it is indeed the case and could show it by a chemical analysis of the cake or, more simply, by making another one in front of the child and showing how mixing the ingredients in the right manner and in the right proportions (and, of course, with all the tacit knowledge of the true chef!) gives this homogeneous appearance.

The lack of precision of many statements concerning the role played by different factors in the analysis of science and technology also comes from a shift in the meaning of the terms and expressions used. When Law writes, for example, in the conclusion of his paper on Portuguese expansion that his approach "makes use of a vocabulary that does not distinguish among the social, the scientific, the technological, the economic and the political, and makes no a priori assumption that one of these carries

3. Thanks to Philippe Faucher for the analogy of the cake, which I developed according to my own taste. . . .

greater explanatory weight than all the others" (1987a, 252), we are not told whether the lack of distinctions refers to ontological, epistemological, or methodological levels. Does he simply mean that the *list* of possible explanatory (or at least intervening) factors is open or that trying to distinguish between these factors is not legitimate and, consequently, that no such list, however long, exists? Whereas the second part of the sentence suggests the first interpretation, the first part suggests the second. What is clear, as we have shown above, is that despite affirmation to the contrary the empirical description does in fact make the distinctions between all the enumerated factors. And if they were not distinguished, how could one talk about the *relative* explanatory weight of factors? Only if one first distinguishes them can talk about relative contribution make any sense. Shifts in meaning of the terms used or in the levels of analysis are frequent in heterogeneous-engineering and actor-network language and probably explain the difficulty of knowing exactly the sense of the interpretations offered, as Collins and Yearly (1992) have noticed. Olga Amsterdamska (1990) has also shown how deftly Latour constantly moves back and forth between semiotic and commonsense definitions of terms, thus making it impossible to attribute any definite position to the author. When Callon writes that all factors "are inextricably bound up in an organic whole" (1987, 84), he cites as confirming evidence Pinch and Bijker (1984), who, according to him, "also show the *impossibility* of separating the *definition* of *technical* problems from the *socioeconomic context* to which the investigators associate them" (Callon 1987, 102, emphasis added). Here Callon conflates two different problems: the *analytical* distinctions between the *factors* involved and the role played by these factors in the *definition* of problems. Saying that all *factors* form an organic whole amounts to denying the possibility of distinguishing them, whereas saying that the *definition* by social agents of technical problems is contingent on social situations is not equivalent to saying that the social is not distinguished from the technical. Quite the contrary: only by first distinguishing the "socioeconomic context" can one say that this context affects the way the technical problem is defined.

This tendency to confuse *relations* with *identities* is frequent. To give a last example: in an otherwise excellent analysis of the complex series of negotiations which led to the definition of the technical characteristics of guided missiles, Mackenzie and Spinardi succumb, in their conclusion, to the usual incantation about the indistinctness of, in their case, "politics" and "technology." They say: "So if we start our analysis from 'technology' we are led towards 'politics'; if we start from 'politics' we are led towards 'technology.' In this maze we indeed begin to find how difficult it is to distinguish the two" (Mackenzie and Spinardi 1988, 611). My uneasiness about that kind of argument can be made clear by the following transposition. Lieutenant Columbo—of my preferred television series—is on the trail of a murderer that he cannot localize and, after some research, summarizes his findings: "So, if we start our analysis from Paris, we are led toward London; if we start from London, we are led toward Paris. In this maze I indeed begin to find how difficult it is to distinguish the two." In fact, this "maze" is only the result of confusing a relation with an identity: why not conclude, as Columbo actually would, that "Paris" is not "London" and that what his research suggests is simply that there is a relation between "Paris" and "London" and that his job is to find the exact nature of this relation. In fact, what Mackenzie and Spinardi's analysis shows is simply that "a technological enterprise is *simultaneously* a social, an economic and a political enterprise" (Mackenzie 1987, 198, emphasis added). The fact that it is through their interaction that the factors take the shapes they have does not lead to the negation of their distinctness. Quite the contrary, it is only by starting from the distinctions of factors that one can talk about their simultaneous presence and interaction.

Thus, to summarize our charitable interpretation, if we limit ourselves to the core of most case studies, we can conclude that their authors do not really mean that "politics," "economy," and "science" *are* the same thing or are not distinguishable but simply argue that there is no definite hierarchy among the various factors involved, that this hierarchy and the nature of the relationships between elements change from one situation to another, and finally that no predefined and fixed number of factors can be identified and applied for all cases. It is a worthwhile plea against social reductionism. This seems to be the position of John Law when he writes that "sociologists of science tend to limit both the type and number of explanatory social factors" (Law 1987a, 229). The message that is transmitted in the introductory and concluding statements of the studies analyzed here thus seems to be that analytical distinctions should not be reified into natural kinds and that we should keep in mind that the Labour Party and the Treasury Board, for example, did not exist ten thousand years ago and are the result of a contingent historical process. This I think is perfectly right and important to remember as long as one adds that for a *given* time and place, however, there is a definite distribution of objects, institutions, and actors and that it is from this distribution that the analysis proceeds to show how these entities *are* interwoven into something new that will then become the "given" for a future situation. If this is what is meant by the "radical" pronunciamentos in the introductory sections of so many papers, then there is nothing really revolutionary here, for this understanding corresponds to the practice of the "average" sociologist.[4]

4. Since some colleagues thought I was suggesting that the "average" sociologist is necessarily right, I should say here that reference to this figure is ironic: it is used only to point out that the self-proclaimed "novelty" of some of the analyses of science and technology is far from borne out by the results presented, which, notwithstanding the pronunciamentos, are fairly "traditional."

And when we look at the way these notions are used by other sociologists, we see that this more "conservative" interpretation is frequent (see, for examples, Sorensen and Levold 1992; Misa 1992; Westrum 1989).

If one takes seriously the temporal aspects of change, it is clear that Latour's argument that "since the settlement of a controversy is the *cause* of Society's stability, we cannot use Society to explain how and why a controversy has been settled" (Latour 1987, 144, emphasis added) is empty, for it is clear that a *previous* state of the distribution of actors and institutions can be used in the explanation. In addition to leaving out the temporal dimension of change, Latour speaks as if it was usual to use Society with a capital *S* as an explanatory resource, whereas in fact most sociologists refer to concrete groups and institutions, which is a completely different matter; if society cannot be the subject of action, particular institutions and actors can.

3. Back to Realism!

Under the guise of extending the list of explanatory factors, Law—as well as Callon and Latour—reintroduces reference to nature by insisting that physical phenomena must be taken into account to fully explain technological change. I have nothing against this form of realism as long as it is acknowledged as such and not presented under the guise of actor-network language. Though this return to realism has already been well analyzed by others (Amsterdamska 1990; Collins and Yearly 1992; Shrum 1988), it is worth noting that using metaphors of "associations," "strength," and "force" *rhetorically* unifies nature and society but hardly hides the realist position that lies behind these terms.

John Law writes that "to try to reduce an explanation of the Portuguese system to a limited number of social categories would fail to explain the specificity of the *volta*, the *caravel* or the *Regimento*. Portuguese views of the sun and the adverse winds are needed to make the explanation work" (1987b, 131). Since all the categories named in these two sentences refer to commonsense physical reality, these two sentences suggest that it is not so difficult, after all, to make a priori distinctions between the social and the technical (thus contradicting the conclusion of his paper that his approach "makes use of a vocabulary that *does not distinguish* among the social, the scientific, the technological, the economic and the political"; (emphasis added), and I think it would be difficult to disagree with these statements. Quite the contrary, a "traditional" sociologist would find rather curious that someone could have *ever* tried to explain the Portuguese expansion (or the flight to the moon for that matter) without talking about boats and sails (or computers and rockets) or at least taking them for granted in the explanation! Again, a "traditional" sociologist may with good reason be considered too conservative, but it is clear that he or she will not see anything revolutionary in Law's text on the Portuguese expansion. He or she may just become a little upset by the grandiose style. As Collins and Yearly (1992) have already observed about Callon and Latour, this position is a return (others would say "retreat") to a "traditional" realist position. After all, taking into account the form of the sail and the direction of the wind and water currents has always been the way traditional historians of science and technology have explained Columbus's and other great voyages on the sea. In Maurice Daumas's encyclopedic *History of Invention* (surely not an example of avant-garde research) one can read: "In the Mediterranean, ships with triangular lateen sails suspended from oblique yardarms had long been in existence; the use of this sail plan *permitted* beating to windward. . . . Off the coast of Africa, south of the equator, the ships had both wind and current against them. This fact *explains* the new use of the caravels" (1969, 362–63, emphasis added). One can surely rewrite all this in more trendy language and say that the lateen sail and caravel help the explorer to "ally himself with" or to "enroll hostile forces," but one can hardly miss the fact that behind this varnish the basic realist explanation is left unchanged. Contrary to Collins and Yearly (1992), I do not object to this kind of realism, but I do find it a bit irritating to see it presented as "new" or "counter-intuitive." It may well be that a strategy of reversal, of negating a relativist position (seen as having become standard) to present a "new" position, has simply had the unintended effect of falling back on the realist position that was first negated by the relativists: a double negation is an affirmation and by declaring himself against "social reductionism," John Law was bound to reintroduce "winds," "oceans," and other "material" objects in his analysis so that the final result looks very much like old wine in a new bottle.

4. The Unconscious Search for a "Total" Sociology

On second thought, the problem with the charitable reading of actor-network and heterogeneous sociologists is that it may not sound "radical" enough. I have observed a frequent use of this term in papers presenting "new" views on the dynamics of science and technology. As far as I can see, the mere use of the term "radical" is supposed to be in itself a proof that the visions so characterized are somehow inherently superior or preferable to other ones characterized (implicitly or explicitly) as "traditional" without further argumentation. This frequent reference to supposedly "radical" interpretations seems to confirm Bourdieu in his suggestion that for intellectuals "extremes are always more chic" (Wacquant 1989, 25).

Since the above interpretation of the heterogeneous-engineering discourse is probably too traditional and not innovative enough, we are left with the possibility that they really mean that it is impossible to distinguish the social from the technical (recall Callon's "organic whole"). The apparent inconsistencies discussed above would then have their origin in the "traditional" disciplinary language, which tend to reify concepts into things and thus do not provide adequate tools to analyze the specificity of scientific practice. This lack of appropriate language would explain the contradiction between the content of the narratives and the methodological position defended, and this state of affairs would only be solved by finding a new language transcending the canonical disciplines. This seems to be the road suggested by Pickering when he writes:

> My suspicion is that scientific practice has its own unity and integrity that cuts very deeply across present disciplinary boundaries. . . . And thus the deployment of existing disciplinary concepts and categories is liable to a serious misunderstanding of what science is like. These concepts and categories have typically been formulated and refined with an eye to the delineation of autonomous disciplinary subject matters, and the danger of insensitivity to the nature of phenomena at disciplinary boundaries is evident. I do not know whether it is inevitable, but the upshot of disciplinary analyses of science has typically been the construction of disciplinary master-narratives in which a schema drawn from a single discipline constitutes an explanatory backbone around which all else revolves. (1990a, 710)

If it is the case that our problem is one of finding a "nondisciplinary" language, then we will have to wait for a new dictionary (and a new ontology?) before arguing this point further.[5] In the meantime, one may nonetheless ask how it will be possible in this "great whole" to talk about science, technoscience, or even technoeconomics (why not scientifico-techno-economics?) or anything similar. Grouping science and technology together still leaves too much else besides that should be taken into account. I am not sure that simply adding "network" after a usual word (like "money-network" or "text-network" in Callon 1991) really illuminates anything. What strikes me about this insistence in finding new words is that it suggests that those who use "traditional" disciplinary categories cannot but succumb to their reification and are bound to forget that they are simply analytical distinctions which are limited and will never catch

5. For a recent attempt to construct a new vocabulary by which to analyze society, see Boltanski and Thévenot 1987. Callon's own dictionary can be found in Callon 1991.

all the aspects of the phenomena. After all, it is not only science that can be considered to have "its own unity and integrity": a chocolate cake also has unity and integrity, and we can nonetheless analyze it to know its composition.

An example of the sterility of discussions on the "essence" of words is provided by Pickering's contribution to the present volume where he attacks my cherished word "constraint" (Gingras and Schweber 1986; Gingras and Gagnon 1988) for being supposedly "asymmetric" and wants to replace it by "resistance." First, contrary to what Pickering asserts without explanation, there is nothing *inherently* asymmetric and anticonstructivist in the word "constraint," for it can be used to refer to social, cognitive, and material constraints. And constructivism should not be conflated with relativism, for one can have the first without the second. The most important semantic difference, it seems to me, between "constraint" and "resistance" is that the latter has a more active connotation, which is an advantage, whereas the former suggests a structural aspect absent from "resistance," which is also an advantage, for it conveys the idea that not all trajectories are equally probable and not only because of external resistance but also because of internal limitations like the habitus of agents and the hierarchical distribution of institutions in the scientific field. This lack of any structural aspect to Pickering's "dialectic of resistance and accommodation" is evident in his narrative of Morpurgo, who is described as working alone in his laboratory confronted with and confronting his apparatus. As an antidote to this linear view, one should go back to Jean Piaget, who (already in 1950) had a similar but much more structural model of the dialectic of "assimilation" and "accommodation" (Piaget 1950, 1985). Pickering also opposes "resources" and "constraints," even though it is easy to understand that there is nothing absolute in constraints, for what is a constraint for a given actor in a given situation is a resource for another actor in the same situation (Gingras and Trépanier 1993). This being said, once we have accepted, with Pickering, that we live "in the material world," there should be no objection to the use of both terms according to circumstances and context (Pickering 1989b).

More important than the obsession with words that are rarely definite concepts and the search for a nondisciplinary narrative is the not always conscious dream of a *total* history of scientific activity (in the sense that Fernand Braudel [1972], for example, did not write a "social" or "economic" history but a "total history" of the Mediterranean Sea). The road toward this "total history" however is not in the direction of the negation of distinctions but of their integration. Contrary to Pickering, Callon, and Law, I do not think it will be possible to solve the problem of the "wholeness" or "integrity" of science or technology (or the chocolate cake) by

simply inventing new words to make an artificial unity around terms like "entity," "force," "strength," and so on. Though it will not sound "radical" enough—and I apologize for that—I think it is more "realistic" to try to do a sociology of science and technology on the basis that it is not only possible but necessary to distinguish, and hence define (however loosely), the tools and concepts used to offer explanations of a given problem or situation. In other words, the discipline constructs its object and its tools. It often happens that in the course of research one has to redefine the object to enlarge its scope and to add tools constructed by neighboring fields (economics, sociology of work, anthropology, etc). In fact, what the "social construction" current has done is exactly that: import concepts that have been in use for a long time in anthropology, sociology of work, or ethnomethodology to enable a fresh look at scientific practice. This practice has opened the way to a "social" analysis of science and technology that now tends toward a "total history," trying to take into account aspects that were left out by previous studies. Over the last 15 years the tendency of microanalysis of science has thus been toward an integration of economic and political factors that were left out of the analysis in the first wave of ethnographic studies, rather than toward the dissolution of these macrosocial categories. This however supposes that one still wants to do sociology, a question that we now have to address.

5. Choosing Your Discipline: Engineering or Sociology?

Despairing of sociology, or simply making another attempt at the strategy of negating what is perceived as a dominant tradition,[6] Callon and Latour

6. After having been among the first to talk about the "social" construction of science in their now "classic" book *Laboratory Life: The Social Construction of Scientific Facts*, published in 1979, B. Latour and S. Woolgar have deleted the word "social" from the title of the Princeton edition published in 1986; the new subtitle now being *The Construction of Scientific Facts*. Latour's last moves—for those who like to follow them—are the replacement of "reflexivity" by "infra-reflexivity" (Latour 1988b) and of "postmodernism" (itself a "reaction" to "modernism") by "amodernism." This reminds me of Claude Lévi-Strauss's description of the habit he learned at the Sorbonne for solving any problem "grave or trivial" by using a "method that never varies": "You first establish the traditional 'two views' of the question. You then put forward a commonsense justification of the one, only to refute it by the other. Finally you send them both packing by the use of a third traditional interpretation, in which both the others are shown equally unsatisfactory. Certain verbal manoeuvres enable you, that is, to line up the traditional 'antitheses' as complementary aspects of a single reality: form and substance . . . appearance and reality . . . and so on. Before long the exercise becomes the merest verbalizing, reflection gives place to a kind of superior punning, and the "accomplished philosopher" may be recognized by the ingenuity with which he makes ever-

want to convince us that engineers are better sociologists than "professional" sociologists. They suggest that "social sciences [should] in some way or another make use of the *astonishing faculty engineers possess* for conceiving and testing sociological analysis at the same time as they develop their technical devices" (Callon 1987, 99, emphasis added). They think that "scientists and engineers . . . are much more original, daring and 'progressive' social philosophers and social theorists than most social scientists" (Callon and Latour 1992, 351). Such remarks certainly make sense as pep talks for would-be engineers in a school of mines but hardly contribute to a sensible discussion about the kinds of tools and concepts needed in sociology of science.

To say that "engineer-sociologists" "develop explicit sociological theories" (Callon 1987, 98) in order to create new technology and that they should become "the model to which the sociologist turns for inspiration" (Callon 1987, 99) constitutes another example of the typical strategy of inversion, which, like that which brought Law back to a classic realist analysis of technology, simply leads to the position expressed, for example, by American engineers during the "technocratic movement" of the inter-war years. As Akin (1977) has shown, these engineers were consciously trying to transform society with their technical knowledge. Moreover, the suggestion that engineers are sociologists plays on the different senses given to "sociology" by shifting from the actors' categories reinterpreted in terms of sociological theory and *then* saying that this theory is explicitly developed by them.[7] It would also be easy to write the same kind

bolder play with assonance, ambiguity, and the use of those words which sound alike and yet bear quite different meanings" (1967, 54). Though the Sorbonne may be the best at inculcating this habitus, it is certainly not impossible to acquire it in other French institutions as well.

7. Callon's paper comparing the sociologies of Touraine and Bourdieu with the activities of the engineers of Renault and EDF is entirely based on a metaphorical use of the term "sociology" and the *attribution* of an explicit sociological discourse to the engineers, which is then compared with what the chosen sociologists *would have said* in the situation. After a totally artificial reconstruction of Bourdieu and Touraine, Callon concludes that in "his [Bourdieu's] explanation of car users' preferences he [Bourdieu as re-created by Callon] omits most of the elements that make up and influence these preferences. . . . Although Bourdieu happens to be right and Touraine wrong, this is quite by chance. . . . the discovery of a cheap catalyst as a substitute for platinum might have proved Bourdieu wrong and rehabilitated Touraine's sociological theory after all" (Callon 1987, 97). This analysis is quite surrealist: (1) Bourdieu's "omissions" are in fact Callon's, for he is the one who constructed this "Bourdieusian explanation"; (2) the realist argument that the discovery of new catalysts would have changed the situation is trivially true and is just plain commonsense realism; it amounts to saying that the landing of a flying saucer or, the explosion of an atomic bomb would affect sociological explanations. What would one think if instead of analyzing papers written by Callon I would write what Callon would have written and then show that "his" analysis is inadequate?

of metaphorical rhetoric about politicians, prime ministers, or presidents and say that they are "better" political scientists than professional ones. Who denies that politicians do experimental economy, political science, or sociology? They vote on laws instituting medieval taxes, eliminating unions, and so on. To observe that some categories "escape completely from the vocabulary of sociology or economics" and that these vocabularies cannot "describe the relationships between fuel cells and the electric motor" goes without saying for anyone who believes in the existence of material reality. Who doubts that the relationship between fuel cells and electric motors cannot be described in "terms other than those of electric currents and electromagnetic forces" (Callon 1987, 95)? Again, insisting that traditional sociological categories leave out many concepts—which is true by definition—leads to the absurd position that if it were not legitimate to stop the analysis at some point, one would be obliged to reconstruct society (or, more exactly, "the great whole") from scratch every time one writes a paper on any subject. Even those interested in a "total sociology" (or a "total technology") could not take all these factors *explicitly* into account in their analysis, for it would be endless and hardly readable.

An argument frequently presented to show the impossibility of distinguishing the social from the technical is to say that the actors' actions "cut across" these very distinctions and ignore them. This raises the question, never really addressed by the proponents of heterogeneous engineering, of the distinction between the categories of the actors and those of the analyst. The fact that in their discourse actors use categories that are different from those used by sociologists is perfectly normal, but this fact does not show that the latter categories are not adequate to their purpose. After all, the role of the sociologist is to analyze actors' discourses and practices, and this can never be done by simply *repeating* the actors' points of view. The fact that what is labeled "technical" by one actor is labeled "political" by another actor certainly raises the question of why they do apply these different classifications. But that actors do not agree on these categories does not mean that the categories used by the analyst cannot explain why they disagree; and the existence of conflicting categories among actors does not mean that distinctions are not made but simply that they are made differently. So I agree with Mackenzie and Spinardi (1988, 612) that an actor's distinctions between the "technical" and the "political" cannot be adopted by the analyst, as long as one adds that it is not because the analyst does not make these distinctions but because the analyst makes them differently so as not to take sides with one group of actors against the other and in order to understand the reasons for (or behind) their disagreements. But whether he or she uses actors' categories or some other analytical categories, the sociologist of science always makes distinctions in the "great whole of society" in order to formulate an analysis. Once it is admitted that no one seriously pretends that these distinctions refer to a Platonic world of pure and unalterable categories, there is no reason to impute to authors who use a given set of analytical categories simplistic ideas about "political ontology" or similar anathemas, except as a strategy to avoid answering criticisms.[8]

The syncretic portrayal of sociologists offered by Callon, Latour, and Law suggests that they are more interested in engineering than in sociology. It is perfectly legitimate to prefer engineers to sociologists, but they should then realize that in so doing they have in fact "followed" engineers and scientists to such a point that they have "joined" them and have become their "representatives" and spokespersons instead of "following" them by staying at arm's length in order to look at them and observe them. No surprise then that Collins and Yearly (1992) perceive their work as traditional and in the "old" realist mode of describing science and technology, while Callon and Latour perceive theirs as trying to "*debunk* scientists' hegemony" (1992, 365, emphasis added). They simply talk at cross-purposes: the first group talks like sociologists, whereas the second group talks like engineers.

So choose your discipline and your social group: I choose sociology, not to "debunk," but to understand the complex and changing relationships between science, technology, and society *from a different point of view* from the one taken by engineers. What sociologists have to recognize to do their job is simply that the various distinctions they make in order to understand the dynamic they investigate are *analytical* constructions and not natural kinds and that only the empirical analysis will show the role played by each of them in the different case studies. And these concepts must be clearly set out in relation to (and distinguished from) the categories used by the actors themselves.

6. Structural Constraints and the Dynamics of Scientific Change

A striking consequence of the wave of microanalysis of scientific practice has been the tendency to describe actors as if they were absolutely free to move in any direction, to elaborate any argument, or to reject any kind of objection. Callon and Latour, for example, frequently suggest that scientists and engineers move continuously from the lab to the industry or to the minister's office. Their notion of "translation" is supposed to help the analyst follow the creation of associations, but it does not explain why

8. For a typical example of such an easy strategy, see Cambrosio et al. 1991.

associations fail or succeed and why some scientists (or engineers) do go into the minister's office while others do not. In more concrete terms: why do engineers graduating from a French *grande école* or from a *faculté* not seem to have the same professional trajectories or the same access to ministries?[9]

Arguments that scientists *could* have done otherwise than they did are often adduced to suggest this freedom of movement. Most of the time, however, we are never told if the possible counterargument has effectively been voiced by the actor or if the argument is an after-the-fact rationalization offered by the analyst. Constantly repeating that "things could have been otherwise" does not explain why in fact things were as they were and not otherwise. This abstract logicism is in good part due to the fact that constructivists direct their arguments more at philosophers than at sociologists in order to counter a kind of positivist determinism by insisting on the contingency of action. Up to a point, this was a good strategy to destabilize old philosophical models of science, but from a sociological point of view it is not very illuminating. If we agree to play the game of sociology seriously, we will have to develop a *model* of scientific practice that can explain the fact that in given circumstances, scientists and engineers probably could not have acted *much* differently than they did. The constant use of counterfactuals does not show that *they* could have done otherwise but that *we* can now imagine other solutions than the one achieved by the actors in given historical circumstances.

In addition to being an effect of debating with philosophers, the insistence on the contingency of action is also in direct relation to the scale of observation. At the level of microanalysis, attention to explanation has often been neglected in the face of the complexity of the interactions: observing and describing many games of chess or Go, one cannot escape the feeling that each game is different and can only be understood in the interactions between the players and their relatively arbitrary decisions. At the microlevel it is inevitable that the open-endedness of these games strikes the analyst as fundamental. In the end, however, accepting the total contingency of action is an extreme form of phenomenalism which leaves no place for any explanation of the dynamics of knowledge production; the analyst has then no other choice but to follow each case empirically to observe the result, like so many different and unique games of chess or Go. However, as these games impose minimal constraints on the movements of the pieces and yet still leave an infinite number of different possible outcomes, so I think that all the empirical studies stemming from the constructivist and ethnographic approach only make sense within a model of

9. For those interested in this question, see Bourdieu 1989.

scientific activity that explicitly recognizes structural constraints and is reflexively applicable to sociology of science itself. But far from leading to a kind of self-destructive solipsism,[10] our conception of reflexivity embodies a practical ethic of discussion and exchange as a *social* condition for the growth of knowledge.

In order to understand why and in which circumstances scientists or engineers can move from the laboratory to the minister's office, one must start from the observation that scientists are subjected to a disciplinary training that gives them a set of tools that define an intellectual horizon. Social actors socialized to live in a particular field can rarely transfer easily their skills and knowledge directly to another field (Bourdieu 1991).[11] To use Pierre Bourdieu's concept, their habitus is the product of a trajectory in a particular field and is best "adjusted" to function inside it. Each of these fields and the relations between them are the product of a past history of social relations and are, in this sense, a social construction. Thus, far from being homogeneous, the social space must be seen as composed of many relatively autonomous fields having their own logic: the plurality of fields is a plurality of worlds. It is this heterogeneous social structure that forces actors who want to circulate outside their native field to adapt, and thus transform, their discourse and practices to the implicit rules of the new field in which they want to circulate. In the case of science, this means that scientists who must appeal to the political field to get money to realize their projects must argue in a way that refers to the specific stakes of this field by talking, for example, of the "national interest" or "economic impact" of their projects in order to convince politicians and administrators. This suggests a strong sense in which to talk of "translation of interests," but this sense completely reverses the use of this term by Callon and Latour, who tend to smooth the passage from one field to another as if there were no "discontinuities" between fields (Latour

10. The best example of an approach leading to such solipsism is provided by Woolgar (1988). It is no surprise that within this kind of narcissistic reflexivity the author concludes his book by suggesting: "Self [should become] a strategic target for social science" (108). Having (textually!) "deconstructed" everything, Woolgar is finally left contemplating his own image in a mirror that is itself the projection of his own Self.

11. For those who prefer to talk about "forms of life" instead of "fields," let us note that Barnes's definition of "forms of life" is equivalent to Bourdieu's conception of practice defined as the relation between a habitus and a field. According to Barnes, "To participate competently and successfully in a form of life requires a profound and comprehensive training at the level of practice, and where the form of life is highly standardized and uniform the associated training must be highly ordered, intense and systematic . . . to provide shared perceptions and shared experiences" (1988, 79). The advantage of the notion of "field" over that of "form of life" is that the former is much more explicitly structured, the latter being, in the hands of too many writers, hardly more than a vague motto. For a comparison between Bourdieu and Wittgenstein, see Taylor 1990.

1987b, 132–44). In other words, they use the notion of translation to insist on the continuity of action without ever taking into account the heterogeneous fields that make those very "translations" possible and necessary—thereby explaining them. The existence of distinct subcultures corresponding to different fields and habitus generating particular kinds of cultural and social capital suggests that there is a barrier (and a cost) to entry in any field (Bourdieu 1979).

The heterogeneity of skills needed to circulate in more than one field also helps explain, for example, the fact that in the era of "big science" the personality of the "manager" and man of public relations took precedence over the myth of the shy and socially misfit scientist. Whereas the latter could easily survive and hide himself in the field of science, only the former could introduce himself into the field of politics (Gingras and Trêpanier 1993). The transformation of the structure of the field was thus accompanied by a transformation of the habitus required to play in the field.[12]

In addition to the limits imposed on action and strategies by the habitus and the amount of social and intellectual capital possessed by the actors, the dynamics of knowledge production *in the scientific field* is also guided by criteria of communicative action. These minimal conditions can be established by starting reflexively from our own practice of sociology of science and are in fact grounded in the principle of nonperformative contradiction as enunciated by Karl-Otto Apel (1980, 1990).

Sociologists (constructivists included) who agree to play in the field of sociology by writing papers, presenting communications, and submitting arguments in favor of (or against) a given theoretical position try to be as convincing as they can. They do their best to avoid contradictions or non sequiturs in their arguments (though they do not always succeed) and track these flaws in the presentations of their opponents. In so doing they *already* accept as a practical a priori two tenets: (1) the principle of noncontradiction and (2) a rule of inference like "p implies q," which makes it possible to move from statements to statements and link some of them together. These minimal conditions are *sociologically* necessary for having meaningful communications between human actors, but we do not have to invoke their "universality" (as some philosophers do by using them as Kantian "regulative ideals"), for they in fact do not characterize all fields of activity to the same extent (for a logical analysis of these conditions, see Cherniak 1986). Though "minimal," these conditions are not trivial, for there exist different fields in which they do not operate in the same way—if at all. Though this is not the place to go into any detail,

12. For examples of historical studies of the constitution of fields, see Bourdieu 1971, 1987; Viala 1985; and Gingras 1991.

the field of politics offers a good contrasting example of a field in which the constraints of performative noncontradiction and self-consistency are very weak. Whatever the "rules" specific to a field, one thing is clear: "The only absolute freedom the game leaves is freedom to withdraw from the game, by a heroic renunciation which—unless one manages to set up another game—secures tranquility only at the cost of social death, from the point of view of the game and the *illusio*" (Bourdieu 1981, 316).

It should be clear here that talking of noncontradiction is not invoking some nonsociological criterion. As the Polish logician Jan Lukasiewicz already observed in 1910, "the principle of contradiction has no *logical* value . . . but it possesses a *pratico-ethical* value which is even more significant. *The principle of contradiction is the only weapon against error and lie*" (1991, 30, emphasis in the original).[13] Its *explicit* formulation is thus necessary as part of the sociological understanding of the scientific field. Moreover, it does not forbid arguments between actors about a *given* contradiction or deduction, for *those very debates already presuppose the acceptance of the principle of noncontradiction.* It is this distinction that is often forgotten in suggestions that logic is not a constraint in scientific debates because an actor can decide not to see a given argument as contradicting his own position. This is what Trevor Pinch, for example, suggests when he writes that the existence of a contradiction was not a constraint in the debate on neutrinos, because one actor (Bahcall) maintained his confidence in his theoretical model of neutrino production for more than a year despite experimental results that were seen by others as conflicting (1986a, 207–11). However, Pinch confuses the logical sense of the term "contradiction" with the larger sense of "disagreement" between theory and data. In fact, far from proving Pinch's argument about the flexibility of logic, the central point about Bahcall's story is that he had arguments for resisting, but after a while, confronted with new arguments, instruments, and experiments, he finally admitted the problems faced by his model. The notion of interpretive flexibility introduced by Pinch is very useful and perfectly consistent with our criterion of communicative action. It is by constantly putting forward new arguments (theoretical or experimental) that actors try to diminish the interpretive flexibility of data and theory and thereby limit the possibility of alternative interpretations.

13. It is worth noting here that this concern with the practico-ethical aspects of life could be brought to bear on the discussions concerning the distinctions between human and nonhuman actors and the attribution of agency. An excellent reason why humans in general attribute special agency to humans as opposed to nonhumans—thus committing the sin of being "asymmetric"—is that they can then recognize the social responsibility of those who by their very actions save nonhuman actors like trees, whales, and so on. Everything suggests that it is not the scallops or the whales that "enroll" the humans but rather humans who for idiosyncratic reasons choose to dedicate part of their lives to becoming spokespersons for trees, whales, or birds, which, in all probability, will never know they may owe their lives to them. Though Latour believes that "the belief in causes and effect is always, in some sense, the admiration for a chain of command or the hatred of a mob looking for someone to stone" (1988b, 162), I think on the contrary that the search for causes is intimately related to an ethic of social responsibility, equity, and justice.

Even to reject new data one has to have arguments. What if Bahcall had continued to hold his position? Given the dynamics of the scientific field, one can easily predict that the rest of the community would have reacted by marginalizing him, with talk about his "silliness," his being "older," and the like. Such a social marginalization is well described by Rudwick (1985) in the case of the Devonian controversy. It is significant that in his criticism of that book Trevor Pinch has argued that "if it is the case that a scientist can still argue for a significantly different position from that embodied in the consensus, then it can be said that the empirical evidence does not *unproblematically* lead in one direction" (1986b, 711–12, emphasis added). The significance of this statement hinges of course on the word "unproblematically." It is clear from the analysis provided by Rudwick that the evidence in favor of the Devonian was far from being "unproblematic," for it took years to create a consensus. In order to insist on the contingent aspect of the consensus that emerged on the nature of the Devonian system, Pinch adds that "as far as Williams and Weaver [the two marginalized geologists in the debate over the nature of the Devonian] were concerned their own arguments were perfectly respectable," and he uses this argument to support the conclusion that since their arguments "were available when the Devonian interpretation was reached, any view in which the 'pull of the evidence' is seen as being decisive is unwarranted." What is striking about Pinch's analysis is that it is implicitly based on a subject-centered epistemology that does not take into account the sociological context. The sociological question here is: *for whom* were the arguments respectable? There can be little doubt that *their own* arguments were perfectly respectable *to themselves*—this is a tautology. The problem is that they could not convince *other geologists*. And this conclusion is not based on any general philosophical preconception about the a priori role of evidence in scientific research but on an analysis of the dynamics of the scientific field of the time. Curiously, a "philosopher" like Gaston Bachelard was already more sociological than the "sociologist" Trevor Pinch in his analysis of science when he wrote: "We propose to ground objectivity in the behavior of others [the peers]. . . . any doctrine of objectivity always submits knowledge of the object *to the control of others*" (1972, 241, emphasis added). As Mary Tiles aptly summarizes

Bachelard's view for the English-speaking reader, objectivity has an "essentially social dimension; objective knowledge is not the unique experience of an individual but that which can be agreed upon by all similarly placed rational subjects" (1984, 53). From this truly sociological point of view, there is no such thing as a private science. Ceasing to exchange arguments or to produce new data about experimental or theoretical results or procedures in the scientific field is ceasing to do science. A scientist can remain convinced for the rest of his life that he is right but his views have no social existence in the scientific field if they are not debated and accepted or rejected.

7. The Temporality of Scientific Debates

A brief analysis of a historical case study will help illustrate the principles discussed above. The tragic history of the French geologist Jacques Deprat dramatically illustrates the role of argumentation and the effects of its *time-situated* character in the acceptance or rejection of knowledge claims by a given scientific community.

In June 1919, a jury of French geologists, meeting in the geological laboratory of the Collège de France, declared one of their most esteemed and brilliant colleagues, Jacques Deprat, guilty of forgery. Expert in the geology of the south of China and the north of Vietnam, he had collected, between 1909 and 1916, a large number of fossils. However, in 1917, his close collaborator, the autodidact geologist Henri Mansuy, announced that Deprat had added fossils of European origin to his Asiatic collection. The crux of the argument put forward by the committee was that some of the trilobites were typically of European origin and current theory made it impossible that they be found in Asia. Though there was much personal acrimony between the protagonists of this affair and private interests were at stake, the crucial point is that current knowledge made it difficult for Deprat to explain the presence of these trilobites, and he could only repeat that he had never added these specimens to his collection and that they were really found during his fieldwork. With no convincing argument other than his own integrity, Deprat lost his job, was banned from the geological society, and wrote novels for the rest of his life, some of them even winning prizes (Durand-Delga 1990, 1991).

The interesting point in this affair is that Deprat's "honor" was recently restored by a historian who argued that over the last ten years geologists have reported the discovery in Asia of many trilobites of the same species that were in Deprat's collection (Durand-Delga 1991, 1346). What was thought impossible in 1917 now made sense in view of the theory of plate tectonics, according to which 400–500 million years ago Southeast

Asia and meridional Europe were closer together than they are today, thus explaining the similarity of the fossils. In view of this historical research, the French Geological Society posthumously reintegrated Deprat as a member on 10 June 1991.

The question is: was a mistake made in 1917 when Deprat was condemned? I think that the sociological answer must be no, for no convincing explanation could be offered at the time of the controversy to make sense of the presence of the contested fossils. Only now, over 70 years later, can one suggest that Deprat was not after all guilty. As this example suggests, debates, discussions, and decisions *are always located in time and thus bounded by a horizon of what is thinkable.* Only later in time can something become "obvious" and old "mistakes" be corrected. And even then, nothing prevents these "corrections" from being in turn corrected and leading, for example, to another condemnation of Deprat. The case of Deprat is not unique. The death and resurrection of Michael Polanyi's potential theory of adsorption also illustrate the crucial role of temporality in science (Polanyi 1969, 87–96).

This model of scientific change based on a dynamic of communicative action inside a structured field has the advantage of explicitly formulating principles that act as a priori constraints on practices—and are thus most of the time implicit—instead of accepting them *implicitly* by having them play important roles in discussions (among the scientists observed as well as in the texts produced by the sociologists) while at the same time trying to prove that they are not at play. In my view, argumentation is the motor of scientific advance. Alone on his island, Robinson Crusoe would not develop science and would be limited to personal opinions and practices that would rapidly stagnate. As Bachelard put it in his poetic manner: "Solitary science is qualitative. Socialized science is quantitative" (1972, 242). As G. E. R. Lloyd has admirably shown, the emergence of Greek science is closely tied with the importance of oral debates in Greek society, and experiments were used, at the beginning, more as a rhetorical device against competing theories than as an effective practice (Lloyd 1979). And a reading of *Polarity and Analogy* (Lloyd 1966) suggests that the very constitution and codification of logical rules are themselves the product of these debates.

Of course, the passage from experiment as rhetoric to full-fledged experimentalism in seventeenth-century Europe was made possible historically only by major and unforeseeable demographic, economic, technical, and institutional innovations like the growth of cities, the printing press, and the formation of scientific societies. We still lack a complete study of the formation and transformation of the scientific field over the last four centuries (but see Ben-David 1971), but even with its modern institutional and technical trappings, the scientific field can still be considered to have something in common with its Greek origins: the dynamic role of public debates. As Lloyd suggests in his analysis of the relationships between science and society in ancient Greece, "this very paradigm of the competitive debate may have provided the essential framework for the growth of natural sciences" (1979, 267).[14]

The dynamics of knowledge production is a historical product not defined in epistemological terms (as the recurrent debates between realism and relativism so often suggest) but in essentially sociological terms: to be right *at a given time* is to have arguments which, given the structure of the field and the current context of experimental and theoretical knowledge, cannot be convincingly contested and replaced by others which would win the assent of the majority of the scientists active in the field.[15] In experimental sciences, the arguments are most of the time *about* experimental data, procedures, or instruments, and these arguments are made within a certain structure of accepted knowledge and procedures (black boxes) that result from previous debates. One could even argue that the development of an effective experimental practice—as opposed to the rhetorical invocation of experiment analyzed by Lloyd (1979)—was a good strategy to oppose conflicting views: new experiments *do* modify the existing consensus and help change accepted theories or *force* opponents to produce new experiments. As Barry Barnes wrote, "natural knowledge is always learned in conjunction with the operations of manipulating and controlling material objects and physical processes and its terms are used to refer to such objects and processes in specific situations" (1988, 55–56).

The argumentative nature of the dynamics of scientific change gives time a central role, for *it always takes time* to experiment, argue, and counterargue. All this is a practical achievement that generates new experiments (acting on the world) and new theory (talking about the world). This "process of discursive rectification" gives rise to a "discursive objectivity" which for Bachelard grounds objectivity in the social control of the members of the "*cité savante*" (1972, 241–42). It should come as no surprise that in the neutrino story Bahcall could maintain for more than a year his point of view in the face of counterarguments. Interpretive flexi-

14. By labeling "persuasive argument" "a statics of logic," Pickering completely misses the point that the dynamic element comes from *debates with others*. This model is thus a far cry from his "dialectic of resistance and accommodation," which is still based on the subject-centered Cartesian epistemology in which a lonely scientist confronts nature with his instruments, leaving in the dark the collective aspect of knowledge growth (Pickering 1990, 720).

15. The study by Kim (1991) of the reception of Johannsen's genetic theory exemplifies clearly the role of argumentation in the rejection of Pearson's biometry.

bility is an aspect of this time-ordered dynamic of scientific exchange. Arguments trying to show that logic or experimental data do not constrain belief can seem convincing only by freezing time.

This importance given to time and arguments leads one to a historicist conception of knowledge which has no place for any kind of absolute criteria of truth. There is no need to reject any explicit reference to an external reality that constrains (or resists, if Pickering so prefers) knowledge claims about it in order to accept that it is the very social dynamics of a regulated exchange in the scientific field that is the condition for producing a type of knowledge that can transcend its conditions of production (Bourdieu 1991, 22–23; Bachelard 1975, 137). As Barnes put it: "we cannot know that a ball is a sphere simply by looking to other people, who will be looking to yet other people, and so endlessly. We must all first look to the ball and decide for ourselves as to its shape—and only then look to others to discern the agreed verdict, if there is one. We have practices for determining the shapes of things which we apply to the things themselves and which tell us (collectively) what shapes the things are" (1988, 179). So, without advocating any simplistic correspondence theory of truth, "there is a genuine sense in which natural knowledge can be said to have external referents," to quote Barnes again (1988, 56).

8. Conclusion

In conclusion, I would like to note that the ethic of communication built into this reflexive model of knowledge production is that in sociology of science—as in any science conceived as a practice regulated by the logic of a field—one condition for playing the game is to continue to argue and counterargue, experiment and counterexperiment (or, in history, do archival research), in order to show the shortcomings of the position of the "opponents." In other words, although, as Aristotle wrote a long time ago, "we are all in the habit of relating an inquiry not to the subject-matter, but to our opponent in argument" (quoted in Lloyd 1979, 267), we should replace straw-man rhetoric and vague reference to "sociology" or "society" as a whole by careful analysis of the actual content of the papers produced by colleagues. Only in this manner will our collective understanding of the "subject-matter" of the dynamics of science, technology, and society (to be short) make any advance. My critical analysis of some notions suggested to make sense of this dynamics only aimed at such a clarification. Faced with articulated arguments one can choose to enter the debate by addressing the specific issues raised (thus playing the game of the field), keep silent, or resort to sophistic techniques (again well analyzed by Aristotle) that avoid confronting the issues. Pace Shapin, "panache, charm and infectious wit" (1988a, 534) are characteristics that will never replace coherent argumentation and reflexive practice, for they are more attuned to the field of fashion than to that of sociology.

Acknowledgments
Since the first circulation of this paper in November 1991, I have benefited from many helpful comments, for which I want to thank Olga Amsterdamska, Donald T. Campbell, Stephen Cole, David Edgerton, Philippe Faucher, Benoît Godin, Sungook Hong, Pierre Milot, Robert Nadeau, Dominique Pestre, John Pickstone, Hans Radder, Claude Rosental, Jan Sapp, and Michel Trépanier. In order not to embarrass any one of them I insist that I am the only person responsible for the content as well as for the tone of the paper! It has also been read at the meeting of the Canadian Society for the History and Philosophy of Science in August 1992 and at the Department of History of Science, Technology, and Medicine of the University of Manchester in January 1993 and discussed in a public debate with Bruno Latour at the Cité des sciences et de l'industrie in Paris in March 1993. Thanks to Bruno Latour for his friendly discussions with me, though they have not yet convinced me to make substantial changes to the paper.

II

Stories