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Postscript: Technology, Stability, and Social Theory

John Law and Wiebe E. Bijker

On Heterogeneity and Explanation

All relations should be seen as both social and technical—this is one of the basic themes that runs through the studies in this book. Purely social relations are found only in the imaginations of sociologists, among baboons, or possibly, just possibly, on nudist beaches; and purely technical relations are found only in the wilder reaches of science fiction. This, then, is the postulate of heterogeneity—a postulate suggesting that both social determinism and its mirror image, technological determinism, are flawed. This is because neither the (purely) social nor the (exclusively) technical is determinant in the last instance. Indeed, what we call the social is bound together as much by the technical as by the social. Where there was purity, now there is heterogeneity. Social classes, occupational groups, organizations, professions—all are held in place by intimately linked social and technical means.

But what does this suggest about explanation? Can we have *no* recourse to the commonsense categories of society, technology, agency, and the rest? Several reponses to these questions suggest themselves. Thus it is perfectly possible to elevate the issue to a matter of principle. For instance, in the introduction we mentioned Bloor's (1976) principle of symmetry—the demand that true and false beliefs (or, in the case of technology, both devices that work and those that fail)—should be analyzed in the same terms. On the other hand, we also mentioned Callon's radical (1986a) extension of this principle—his controversial[1] view that the social, the technical, and indeed objects in the natural world should be analyzed in the same terms. Many, perhaps most, English-speaking students of sociotechnology reject this view because it is incompatible with the Wittgensteinian and Winchian (1958) tradition of studying cultures as forms of life: machines, it is argued, cannot possibly create their own cul-

ture (Collins and Yearley 1991). Callon and Latour (1991) counter by arguing that it is wrong to privilege humans, that a properly symmetrical analysis will consider relations and interaction without assuming that certain entities—people or their beliefs—are the prime movers of those relations.

It is therefore possible to take a principled epistemological stance on these issues—but it is also possible to avoid doing so. The studies gathered here suggest that despite such differences, there are large areas of overlap and commonality among those committed to the idea that sociotechnology may be seen as a heterogeneous and seamless web.[2] If this is so, then the practical problem is how we might discern patterns and regularities in the sociotechnical, without falling back on the old distinctions between the social, the technical, and the cultural.

One way of thinking about this is to note that if groups and organizations are held in place by mixed social and technical means, we cannot assume that they are stable and unitary. Indeed, they may change or dissolve as those means and their effectiveness changes. Their success or otherwise is a contingent matter, not one of necessity, which means (as we suggested in the introduction) that neither technologies nor social institutions move along inexorable trajectories. Indeed, we have seen Law and Callon make ironic use of the notion of trajectory and stress the uncertain and contingent progress of projects on just these grounds. In a similar mode, Bijker's chapter suggests that innovation does not necessarily precede diffusion: the two may take place simultaneously. The basic point, of course, is that sociotechnical ensembles—facts, artifacts, societies—are interpretively flexible (Pinch and Bijker 1987). Only when the self-evident and unambiguous character of such ensembles has been deconstructed does the quest for the origins of their obduracy become relevant.

But what should be made of this contingency? Does it mean that all is so complicated that description displaces explanation? Is the analysis of sociotechnology restricted to "how" questions? Are questions about why some sociotechnical combinations become obdurate and are institutionalized while others do not simply impossible to tackle because of their complexity? Again, the contributors to this volume offer a variety of views. For instance, Latour very deliberately seeks to elide "how" and "why" questions. Elsewhere (Latour 1988c) he has argued that such constellations as classes, countries, kings, or laboratories should not be treated as the *cause* of subsequent events, but rather as a set of *effects*. In other word, they should be seen

as the consequence of a set of heterogeneous operations, strategies, and concatenations. In this view, the job of the investigator is not to discover final causes, for there are no final causes. Rather, it is to unearth these schemes and expose their contingency. There is also a moral point here. Latour assumes that those who are powerful achieve that power by boxing others in, borrowing from them, and misrepresenting them. The object is to uncover these strategies of misrepresentation. In his approach, "why" questions are thus converted into "how" questions.[3]

Another possibility is to press deconstruction still further. Here the investigator takes apart not only the strategies, operations, and concatenations of those under study, but also deconstructs the analogous strategies, operations, and concatenations that generate his or her own account. The point, of course, is that if the coherence and consistency of those under study is the product of discursive or nondiscursive methods, then the same is equally true for the univocality of the analyst. There are several possible reasons for attempting this reflexive deconstruction. On the one hand, it is a way of undermining the privilege that attaches, explicitly or otherwise, to the analyst's description. The latter becomes just another account. This can be a particularly effective method for emphasizing the way in which what appears to be a simple phenomenon or object—for instance, a test or an artifact—may be quite differently interpreted by different observers.

On the other hand, it can also be used as a heuristic device. Thus if, as seems possible, both we and those we study use broadly the same methods to achieve a degree of solidity, we may learn about our methods when we study others, and learn about their methods while studying ourselves. These, at any rate, are two of the conclusions that can be drawn from the paper by Pinch, Ashmore, and Mulkay. Thus the authors skillfully show that the "social technology" of clinical budgeting means substantially different things to different people—or indeed to the same people under different circumstances. They also show how two discursive styles, which may be displayed as inconsistent in one account, can be treated as complementary within a strategy that tends to reinforce a technology. A similar conclusion can also be drawn from Bowker's piece on patents, whose utility depended on the use of two registers—one internalist and Whiggish, for deployment in a legal context, and the other externalist and deployed, albeit secretly, in an organizational context.

If the desire to avoid reduction leads authors such as Latour and Pinch et al. to dissolve the distinction between description and expla-

nation, this is not the only possibility. Thus some of the contributors to this volume assume that certain social groups are stable enough to be used as a kind of explanatory scenery, under certain circumstances. The clearest example of this approach is to be found in de la Bruhèze's study of the development of AEC policy toward nuclear waste. In this, the various branches of the AEC are assumed to have relatively stable sets of interests, which in part reflect their existing working practices.[4] Perhaps all empirical studies depend upon some such backdrop—its strategic use might be an example of the way in which new, asymmetrical (if somewhat more local and variable) distinctions may be used to explain the seamless web. After all, even in the reflexive studies, everything cannot be deconstructed simultaneously. However, the contributors to this volume are cautious about the status of the social groups that make up their explanatory scenery. They all, for instance, assume that the actors or groups in question are affected by the unfolding dramas in which they are involved: that at the end they may not be the same as when the story started. In short, such authors assume that the backdrop is a partial function of the events that take place in front of it. The aim, then, is to follow Marx's much-quoted if sexist adage that men make history, but not in circumstances of their own choosing.

As a century of Marxist debate has shown, it is difficult to avoid toppling off the fence in one direction or the other. Typically, either the "circumstances" or the "people" come to dominate explanation. It is difficult to achieve a dialectic in which they are balanced and the way they interact is defined. Indeed, recent discussion in the social analysis of technology addresses precisely this issue.[5] More structurally inclined analysts have argued that the kind of approach exemplified in this volume is excessively actor-oriented and pays insufficient attention to the constraints imposed by structure. This kind of debate is common in sociology (Elias 1978) where, however, its terms reflect an increasing tendency to refuse what Giddens (1984) calls the dualism of structure and agency and instead treat agency, like social relations, as a set of strategically and recursively generated transformational propensities.[6]

The specifics of the sociological jargon are not important here. Actors and structures are *both* products, and they are created and sustained together: to create an actor is also to create a structure, and vice versa. We cannot review the sociological debates here. Nevertheless, the concern with sociotechnical stabilization that runs through this volume is close to—we suggested in the introduction a version of—the problem of securing the social order. Accordingly,

we wish to consider the relevance of the work described in the preceding chapters for questions of order, control, and structure. We consider agents and their strategies before turning to the more structural dimensions of obduracy.

On Strategies of Obduracy

Let us start with the observation that much of the time people try to devise arrangements that will outlast their immediate attention. That is, they try to find ways of ensuring that things will stay in one place once those who initiated them have gone away and started to do something else. They also—and this amounts to the same thing—try to find ways of doing things simply (Callon and Latour 1981). The deceptively naive fable offered by Latour illustrates both points. It is simpler to pass through a door than a wall. It is simpler, that is, to delegate the process of creating and closing an opening to an artifact than it is to knock down and rebuild the wall each time—simpler, but not so very simple. The problem is that the delegates have to be kept in place. It is no good delegating tasks to artifacts or people if the effort of making sure that they perform as they should is greater than the original effort. The problem, then, is dual. First, it is necessary to delegate. And second, it is important to find ways of efficiently policing the delegates.

We want to suggest that many, perhaps most, strategies for delegating and policing involve two fundamental moves. First, *a distinction is made between inside and outside* and a set of exchanges between the two is defined and regulated (which amounts to the same thing). And second, those who are outside find themselves compelled to participate in those exchanges: what is produced by the inside, and so the inside itself, becomes what Callon calls an *obligatory point of passage*.

To put it this way is to put it very abstractly. How does this work in practice? Consider a simple example—Akrich's description of the use of photoelectric lighting kits in less-developed countries. These kits, which were sent to Polynesia, were designed—as those who conceived them saw it—to be idiot-proof. The inside of the kits was hermetically sealed from the outside. Possible points of entry were minimized. The designers did not want unauthorized people fiddling around: their plugs were nonstandard; batteries were watertight, and exchanges between the kits and their users, were limited and regulated. A docile user was, as it were, designed to be attached to the kit—a user that the designer assumed would be compelled to use

the kit in the approved way because of his or her need for electric light.

Here, then, we see a physical attempt to distinguish between inside and outside and regulate the exchanges between them. And we also see a theory about the needs and resources of users—the notion that they would be compelled to use the lighting kit in the approved manner because they needed the light and did not know enough about electricity to subvert the intentions of the designers. In this way, then, a theory about the behavior of actors—Akrich calls this a script—was built into the artifact. As Akrich indicates, the first of these assumptions was correct, but the second was not. People very soon learned how to subvert the cut-out and obtain "unauthorized" electricity. The script was not played out.

The case of the photoelectric lighting kit is an elementary example: with control of the inside, and a theory about how the outside will react to its products, the actor who seeks to build an institution has some hope of attracting and regulating outsiders. The scientific or technical laboratory offers us another, more sophisticated example of the same strategy at work. Here again an inside is distinguished from an outside. The inside achieves a kind of autonomy, at least for a time, because exchanges between inside and outside are regulated: money and resources are, for instance, exchanged for innovations, or the promise of innovations. But here the inside-outside distinction plays another important role, because the autonomy granted to the laboratory is also temporal. Theories about the environment are not, as in the case of a piece of kit, set in concrete. Rather, they are adaptable. Thus it is often possible to run simulations in the laboratory much faster than it is possible to do in real time.[7] Just as we can run a dozen possibilities through our heads in a second before alighting on the best, so dozens or hundreds of trials and errors can be run in a laboratory before a satisfactory option is found.

Unlike the device itself—for instance, the photoelectric lighting kit—the laboratory is thus a kind of time machine. The photoelectric lighting kit cannot jump forward through time to see whether it is attractive to its users and to check that it is not being "misused." The theory of the environment built into it is either right or wrong. There is no possibility of adaptation. By contrast, theories about the future behavior of the environment created in the laboratory may be explored, tested, and altered. The laboratory, and any other analogous space, has many chances to attract and regulate those who use its products. It also makes its mistakes in private, which means that its credibility is less likely to be undermined in the eyes of outsiders.

But how is the inside distinguished from the outside? How are their exchanges controlled? How are outsiders kept in place? The answers to these questions, as the chapters in this volume suggest, are empirically diverse. So far as the barriers are concerned, the case discussed by Akrich reveals the importance of *physical exclusion*. Here, for instance, there was no way of getting into the batteries, which were sealed. Physical exclusion is also important on a larger scale. Thus industrial companies seek to maintain the security of their research efforts in part by means of walls and chain-link fences.

However, the example of the scientific laboratory points to another important possibility: the ability to scale up and down, which in turn relates to *shifts in materials and media*. Thus the kind of modeling work that we mentioned above operates on objects that are more docile and manipulable than the entities they represent. Thoughts are more docile than people. Drawings, algebraic expressions, and a handful of colored pebbles are more malleable than real dikes. Tons of water can be flooded into a model of the Dutch estuary a hundred times more quickly than the North Sea is able to do this in the real world. Such technologies, which generate echelons of depictions and descriptions of ever-increasing simplicity, homogeneity, and docility, are crucial to many strategies for distinguishing between the inside and the outside.[8]

Such distinctions are, however, reinforced and reproduced by a third set of methods for building barriers. These are *organizational arrangements*, which may be of a legal or quasi-legal basis. Chain-link fences tend, after all, to break down and allow unregulated exchanges between inside and outside unless they are policed. In addition, many metaphorical barriers between inside and outside are inscribed in legal, organizational, discursive, or professional arrangements. Consider, for instance, the Bessemer Steel Association described by Misa. This was a patent-pooling agreement that licensed steel producers in the United States. "Inside" the barrier were all the patents needed to make steel. These might be used by steelmakers outside, in exchange for the payment of appropriate royalties. Steelmakers were drawn to the association because they had no alternative: the apparatus of patent law would have extracted punitive damages had any steelmaker chosen to ignore the patents in question. Accordingly, the Bessemer Steel Association attracted clients. It was a successful arrangement that became an obligatory point of passage for steelmakers.

To create this organizational barrier between inside and outside, the Bessemer Steel Association made strategic use of patents. But as

Bowker shows in his study of the geophysical firm Schlumberger, patents do not stand alone. Though they rest on a distinction between inside and outside, they also help to reproduce these inside/outside divisions. Thus, in a legal context they rest on fictions about priority and the immaculate character of the processes and devices that they purport to describe. At the same time, at least in the case described by Bowker, Schlumberger sought to protect them in court not because it thought they could be turned into an obligatory point of passage, but because for various institutional reasons—primarily delay—it believed that such litigation would give Schlumberger the opportunity to work closely with the oil companies and so entrench itself more firmly in the field.

The Bessemer Steel Association and Schlumberger (if not its patents) were obdurate end points—barriers, or a set of arrangements that distinguished between those who were entitled to sell and those who were obliged to buy. Indeed, much of the process of barrier building has to do precisely with distinguishing between who will be inside and who will be outside. It concerns, that is, the allocation of rights and duties. Often these have to do with rights to speak, or the duty to keep silent—a process that involves disenfranchising those who find themselves on the wrong side of the barrier. To the extent that those outside depend on or have an interest in the product, the product and its producers become an obligatory point of passage.

Thus in the course of his discussion of the proper place for fluorescent lighting in the United States before the Second World War, Bijker touches upon a proposal that all fluorescent light fixtures be certified before sale. More successful, and of greater historical significance, is the example described by de la Bruhèze. Here the question concerned the treatment and storage of nuclear waste in the United States. A number of organizations and divisions had putative rights to speak on this topic, and de la Bruhèze describes the way in which they struggled to impose their own views about the substance of the matter and about those who should have rights to participate in the decisionmaking process. This was a messy bureaucratic battle. However, in the end it led to the creation of a barrier between the inside—those who were competent to speak and make decisions—and the outside—those who were not. In part this was organizational. Different committees were, for instance, empowered with different competences. In part, however, it was professional. Certain experts and specific forms of expertise were enfranchised while others, most notably the general public, were disenfranchised.

De la Bruhèze's study illustrates another feature of the way in which barrier building and the regulation of transactions across the boundary can lead to stabilization. Outsiders may find themselves bound not so much by products created within the boundary and exchanged across it, but by the promise of future products. Thus in the case of the treatment and disposal of nuclear waste, the professionals empowered to investigate and recommend a solution to the problem not only differed among themselves. They also took the view that further research was needed if the problem was to be solved. This, however, was all that was needed to keep the public and outside skeptics in their place. The AEC commissioners, who had the power to decide whether or not to allow the development of nuclear power as a source of energy, were satisfied with the promise of a future solution—even though, two decades later, the difficulties are more intractable than ever.

A similar process is described by Law and Callon in the TSR.2 aircraft project. Like a laboratory, this project attracted clients that granted it resources in exchange for the expectation of a future return. And here again that future return was, at least in some views, not forthcoming. The consequence was that the barriers between inside and outside—the carefully regulated crossing points between the project and its environment—ultimately evaporated, along with the project's clients. Institutionalization was followed by dissolution. An example of greater success is provided by Bijker's case of the fluorescent high-intensity lamp. This was designed by a group of managers to allay the fear of the utility companies that the new lamp would threaten their sales of electricity. The lamp was especially effective because of the promise it entailed: it was not yet possible to make such a lamp, but if at some future point this turned out to be possible, then it would certainly consume a large amount of electricity.

The question of who has a right to speak is important in strategies of stabilization and appears in a number of guises, one related to the issue of interpretive flexibility.[9] As indicated, this is the notion that any object, institution, or process may mean different things to different people. As is clear from a number of studies—for instance, those of Bijker, de la Bruhèze, and Pinch et al. in this volume, and Callon (1980) and MacKenzie (1990a)—what appears as a successful innovation from one perspective may be a failing artifact from another. The example of the contraceptive pill given by Bodewitz et al. (1987) is colorful but to the point: in a recent edition of the Spanish *Pharmacopoeia*, estrogen-progesterone combinations were

described as a drug for regulating the menstrual cycle, which had the serious side effect of preventing pregnancy.

If those outside, who are skeptical about an innovation, are to be bound either to that innovation or to the organization from which it emerges, then those who are inside have two main options. Either, as we have seen, they have to disenfranchise the skeptics, or they need to transform the outsiders' perceptions of the innovation, enroll them to the inside, and have them subscribe to that "inside reality." There are several examples of the second option among the case studies. Thus Bijker describes the way in which a "science of seeing," which had to do with subjective perception of artificial illumination, was adapted to conform to the interests of the utilities and used as a tool to persuade the public that there was good sense in trying to create higher intensities of artificial lighting. Again, de la Bruhèze talks of the way in which one of the committees on nuclear waste disposal played a role in "educating" the public about the tractability of the problem. And, finally, as a special technique for both transforming perceptions and disenfranchising skeptics, there is the process of authorized technological testing. Thus, as Pinch et al. remind us, just as what counts as a fact of nature is often ambiguous, so too is what should count as a working technology. The success of a device or process is often a matter for dispute. One way of ensuring that the product is successful is to disenfranchise those who might consider it otherwise. This was the strategy pursued in the case of clinical budgeting. Recognizing that this is a highly controversial "social technology," those responsible for its experimental introduction to the British National Health Service arranged to have the results of their experiments judged by the National Evaluation Group—a committee of high-status professionals whose judgment would, or so it was hoped, carry weight. We witness here, then, the social equivalent of the tradition of testing water turbines described by Constant (1983).

On the Frameworks of Obduracy

We have argued that strategies for realizing obduracy comprise efficient combinations of delegating and policing the delegates. The dialectic of action and structure turns on this double requirement. If the strategies for delegating and controlling are successfully deployed, an institution results, an arrangement is stabilized, a structure emerges. Institutionalization cannot, therefore, be detached from the strategies of actors, but neither can it be reduced to these,

because the delegates that an actor seeks to array and hold in place are drawn from a structured environment. That structure, like actors or institutions, may be seen as a contingent set of heterogeneous relations. From the standpoint of any particular actor, the structure and the actors defined within it represent a more or less accurately pictured geography of enablement and constraint. Thus, some relations are much easier to create and maintain than others. They are ready to be drawn on and can be utilized simply and economically. Others are expensive, awkward, and time consuming. Structure, then, is something like a system of transport. The network of paths, tracks, roads, railway, and airlines mean that it is easy to get from some places to others. They are close, either figuratively or literally. On the other hand, other locations are far removed from one another. Maintaining links between them is time consuming, tedious, expensive, or downright impossible.

If the relations that make up structure are an emergent consequence of actors' strategies and unmotivated actions and events, then structure is liable to change in ways that are sometimes unpredictable. However, any particular agent can only hope to act in a way that has more than a random chance of success, if the geography of structural relations displays some degree of predictability.[10] We have touched on one of the consequences in an earlier section: for certain purposes, even those who insist on the contingency of structure are able to treat it in practice as a more or less invariant scenery that shapes, but is relatively unshaped by, the action that takes place on the stage. Thus the notion that certain agencies have locally stable interests or practices finds its way into the accounts of a number of our contributors. Accordingly, though most of the authors are at pains to argue that such interests are subject to change,[11] they tend to work on the assumption that actors have a (relatively stable) concern to preserve the structure of their existing practice. This is the backdrop to which we referred earlier.

This is not to say that actors are always, or even typically, aware of the structures within which they operate. Thus, though they have procedures and technologies for ordering and *representing* those structures—for instance, the model of the Dutch estuary system—such procedures are necessarily precarious. This is because they rest on a series of simplificatory assumptions—about the general character of the environment, how it is organized, and how it might be ordered or reordered. As writers from Simon (1969) onward have argued, simplification is a dangerous necessity, for there is no way of representing and handling complexity or nuance in full. Accordingly,

such assumptions may or may not turn out to be workable in practice next time around. Thus Carlson's chapter argues that Edison was acting within a specific simplificatory frame—that of producer culture—and that the character of this frame explains why Edison and his associates did not successfully participate in the growth of the mass movie industry. History had, as it were, moved on, and unlike the case of Schlumberger described by Bowker, or the Bessemer Association described by Misa, Edison's strategies did not directly shape the course of that history. Rather, it was the "bottom-up" entrepreneurs opening the nickelodeons in working-class towns who succeeded in operating in that part of the environment. Here, then, Carlson questions the model of calculative rationality that so often, albeit implicitly, underlies the analysis of sociotechnology. According to that model, some calculation of an actor's interests may explain subsequent events. The case of Edison's involvement in the motion picture industry suggests that such a model is at best incomplete and in some cases simply wrong.

If we want to eschew reductionism, what then can be said about the geography of constraint and enablement that makes up the environment? What can be said about the way in which this affects the success of actors' strategies? And what can be said about the circumstances that lead particular concatenations of sociotechnical elements to display particular obduracy in the face of their environments? We want to conclude by pointing to three lines of work that offer possible answers to these questions: first, the notion of *technological frame* as developed by Bijker; second, the notion of *technological momentum* developed by a number of social historians of science; and third (like the first, strongly represented in this volume), the distinction between inside and outside, which leads to the formation of what Law and Callon call *negotiation spaces*.

The notion of technological frame (see Bijker 1987) refers to the concepts, techniques, and resources used in a community—any community, not simply a community of technologists. Technological frame is thus a combination of the explicit theory, tacit knowledge, general engineering practice, cultural values, prescribed testing procedures, devices, material networks, and systems used in a community. It is—and this is what distinguishes it from such possible social analogues as Mary Douglas's (1973) notion of grid and group or Joseph Ben-David's (1960) concept of role hybridization—simultaneously social *and* technical. Actors' meanings, including those parts of their strategies that are explicitly articulated—the ways in which they react to and interpret structure—form a part of techno-

logical frame. But so, too, do relations of which the actors are not aware—relations that may be embodied, as in the case of skills, or form part of their environment, as in the case of such resources as the power supply or the details of software that they use to build their spreadsheets. Technological frame is thus concerned with structuring relations, whether social or technical. It is also a bridge between structure and action. And that bridge both points to ways in which structure may be influenced by action and makes it possible to predict that certain kinds of structure will lead to one kind of action, and other structures to alternative actions.

As an example of the way in which action may influence technological frame, consider the case of celluloid. Bijker (1987) describes the way in which the specific attempts of Hyatt, his collaborators, and his competitors to develop nitrocellulose plastics (such as the focus on solvents and in particular camphor as a key element in the invention of celluloid) had a direct impact on the technological frame of the next generation or celluloid engineers. As a result, the courses of action of the chemists subsequently working within that frame were further constrained—but also, of course, enabled. But the theory of technological frame also makes predictions about the style and origin of innovations. Thus under certain circumstances there will be one dominant group that is able to insist upon its definition of both the problems and the appropriate solution. Under such monopolistic circumstances, *conventional* innovations tend to arise. In particular, they do so when there is functional failure (Constant 1980), and they are judged in terms of their perceived adequacy in solving such failures.

Under other circumstances, when there are two or more entrenched groups with competing technological frames, arguments that carry weight in one frame will carry little weight in the other. Under such circumstances criteria external to the frame in question may become important as appeals made to third parties, over the heads of the other social group. In addition, innovations that allow the amalgamation of the vested interests of *both* groups will be sought. Such innovations (the definition of steel and associated testing technologies present a case in point) are, so to speak, doubly conventional because they have to lodge within both technological frames.

The third situation considered by Bijker (and here his case is the early history of the bicycle) occurs when there is no single dominant group and, as a result, no effective set of vested interests. Under such circumstances, if the necessary resources are available to a range of

actors, there will be many different innovations. Furthermore, these innovations may be quite radical. More than in the other cases, the success of an innovation depends on the formation of a constituency, a group that comes to adopt the proposed technological frame.[12]

Bijker explains action by relating it to the way in which actors are shaped by and implicated in a network of relations. There are commitments, explicit or otherwise, to economic investments, normal practice, and skills. There is dependence—which is not remarked upon until things start to go wrong—on networks of resources that enable certain courses of action while more or less frustrating others. And there is the question of the differential availability of those networks of resources. Thus it was far more expensive to enter the electricity supply business when it started than to initiate the manufacture of bicycles. The result is a model not of the interests or commitments of specific social groups but of the *patterns* that arise when social groups are constituted and interact with one another in a range of different structural circumstances. It is, in other words, a predictive structural theory about the obduracy or certain sociotechnical circumstances and the malleability of others. It is, moreover, a theory that is neither socially nor technologically reductionist: the concept of "technological frame" is intrinsically heterogeneous.

Though we have mentioned the way in which Law and Callon ironicize the notion of technological trajectory, the concepts of *technological momentum* and the closely related notion of *life cycle* have been deployed with considerable success in the history of technology (see Hughes 1983, Staudenmaier 1985). The argument of such historians is that, at least for America between the 1880s and the 1930s, certain technologies—the cited cases are electricity supply and the motor car—and their carriers, which were malleable in their early stages, later developed to a point at which they were relatively insensitive to, but exercised great influence over, their environments.

Though it is possible that such analyses are historically contingent—they apply to the United States at a particular time, but cannot be applied elsewhere—it is nevertheless interesting to note that on the basis of his theoretical generalizations, Hughes (1986b) has made predictions about the development of the modern health care system. Hughes argues, for example, that at present health care is at a stage of development comparable to that of power systems between the two world wars. As with power systems at that earlier time, the medical systems' components are now heavily capitalized and institutionalized. Hence, the era has passed when independent inventors—for instance, physicians—with limited capital and insti-

tutional support could dominate research and development. This suggests that institutions with easy access to capital will take on key roles in the process of building systems. Obvious candidates for such a role are the medical equipment manufacturers, the pharmaceutical companies, and various multipurpose consulting firms and holdings. Drawing on an analogy between pharmaceutical firms in the health care system and petroleum companies in the electric power supply, Hughes predicts that the pharmaceutical companies will only be able to assume a central role in the health care system if they develop a holistic approach to medical problems. Otherwise they may find themselves on the periphery, as were the petroleum companies that were so involved in the automobile industry that integration into the power supply system was difficult.

It is interesting to note that such theories draw on similar intellectual roots as the theory of technological frame. That is, once again they rest on the extent to which actors are shaped by or otherwise implicated in particular networks of relations. Some of these are economic, hence Bijker's use of a vocabulary of investment when he talks of the "amortization" of vested interests.[13] Others take the form of commitments to expertise and embodied skills: the metaphorical investment of time and energy. In addition, however, there are patterned relations—for instance, the highway system in the United States, the character of public transport, the growth of new styles of consumption (such as out-of-town merchandising)—that depend on the maintenance (in this instance) of the automobile. Such patterned relations—what Staudenmaier calls the "maintenance constituency"—add to the obduracy or momentum of the sociotechnical system because they rest on an endless series of "side bets" (Becker 1964). This, however, is a contingency. If the side bets are lost, or reshaped, then the sociotechnology will be accordingly reshaped.[14]

The third approach to middle-range analysis of the way in which structure relates to action—an investigation of the distinction between inside and outside—takes us in rather a different direction. In the previous section we described the way in which strategies of obduracy frequently, if not always, turn first around the creation of a distinction between inside and outside, and second upon ensuring that whatever is inside becomes an obligatory point of passage for those on the outside. We mentioned the simple case of physical exclusion—the paradigm case was the battery intended for a user in the developing world—but also talked about the arrangements that allow those inside the barriers to turn themselves into a kind of

time machine and so model the outside. In this context physical, legal, rhetorical, bureaucratic, and technological methods were all mentioned, and doubtless there are many others. The Bessemer patent pool is another example. Creating the patent pool in 1866 was obviously a strategy designed to close the controversy between the Troy and the Ward groups. But it also led to structural constraints to future actions. Thus, as Misa shows, it posed a serious barrier to steelmaking firms, such as Andrew Carnegie's, which were trying to extend their market share in the 1870s.

The inside/outside division is heterogeneous in character. It has to do with the organization of bureaucracies (Chandler 1977), the development of methods of accountancy (ibid.; McGaw 1986; MacKenzie 1990b; Law 1991a), technologies of communication (Eisenstein 1979; Beniger 1986), techniques of representation (Bertin 1983; Latour 1990; Lynch and Woolgar 1990; Tufte 1983, Shapin and Schaffer 1985), methods of modeling (Law 1991b), mathematical tools and statistical representations (MacKenzie 1978, 1990c), developments in cartography (Wilford 1981), legal innovations (Pool 1983), and a host of other sociotechnologies. If the social is too weak to hold us all together (Callon and Latour 1981; Latour 1990), then it is certainly too weak to create obdurate negotiation spaces that are able to model and shape what goes on in the environment.

As is also obvious, such methods do not stand outside history. Rather, like all the other sets of relations that we have touched on, they are historically contingent. We hesitate to make the simple-minded argument that they are in a continuous process of development. Perhaps, like Mann (1986) and Beniger (1986), it would be better to say that they evolve discontinuously. Nevertheless, it is incontrovertible that they are subject to secular change. Such changes are not readily visible in the case studies described in this book. Nevertheless, when the new sociotechnology starts to address these changes, it will begin to obtain purchase on some of the fundamental historical and sociological questions about power, class, inequality, social change, and the formation of the modern world.

Conclusion

To conclude, we return to our point of departure. Technology is never purely technological: it is also social. The social is never purely social: it is also technological. This is something easy to say but difficult to work with. So much of our language and so many of our

practices reflect a determined, culturally ingrained propensity to treat the two as if they were quite separate from one another. The authors in this volume all wrestle with this problem. Of course, they do not come to identical conclusions. Their work has a range of contrasting implications for historiography, for social and political theory, and for the organization and management of technical change. What brings them together is an urgent sense of the need to understand the heterogeneous webs in which we are implicated.

We want to conclude with two thoughts. The first is that the academic time is right for work on the sociotechnical. We rest our case on the various approaches exemplified in this book. Of course, they are underdeveloped. Of course, they represent work in progress. Of course, they have limited applicability at present. Nevertheless, we believe that they show how theoretically informed empirical work may start to break down the disciplinary barriers and the habits of common sense and make it possible to understand the sociotechnical world in which we are caught up. This, then, is our first thought: we are witnessing the birth of a new capacity to understand, in a matter-of-fact way, how it is that people and machines work together, how they shape one another, how they hold one another in place.

Our second thought has to do with the urgency of this task. Our technologies surround us, as they have for millennia, but never before have they been so powerful. Never before have they brought so many benefits. Never before have they had such potential for destruction—in many cases a potential that has been realized. And never before has the task of understanding those technologies—how they are shaped, how they shape us—been so urgent. The work described in this book is only a first step. But with its stress on heterogeneity it *is* a first step: it says, in effect, that technical questions are never narrowly technical, just as social problems are not narrowly social. When things go wrong, it may not not make much sense to blame technologies. Neither does it necessarily make sense to blame people, nor even the economic systems in which they are caught up. Who or what should be blamed for the Nimitz Highway collapse? Or the Challenger disaster? Or the deforestation of the Himalayas? Or the greenhouse effect? If we want to make sense of these horrors—and more important, do something about them—it does not really help to look for a scapegoat. Rather, what we urgently need is a tool kit—or rather a series of tool kits—for going beyond the immediate scapegoats and starting to grapple with and understand the characteristics of heterogeneous systems.

Notes

1. See, for instance, Amsterdamska 1990, Collins and Yearley 1991, and Callon and Latour 1991.

2. For a discussion of the metaphor of the seamless web, see Hughes 1986a.

3. Law (1991a), though sympathetic with Latour's moral and methodological position, argues that "how" and "why" questions are not mutually exclusive. Specifically, he suggests that power is indeed the product of a set of (strategy-dependent) relations, but this does not mean that it cannot be stored and used for certain purposes.

4. It could be argued that similar assumptions underlie Misa's chapter on the development of steelmaking in the United States in the nineteenth century, Bijker's study of fluorescent lighting, and Law and Callon's description of an unsuccessful military aircraft project.

5. See, for instance, Russell 1986 and Pinch and Bijker 1986.

6. See Giddens's work—for instance, Giddens 1984; and for an interesting recent commentary, see Clegg 1989. See also Law 1991a.

7. Or if not more quickly, at least more tractably. A concrete example of this is given by Latour (1987, 230–232) when he describes the scale modeling of the Dutch coast undertaken by civil engineers in the Delft Hydraulics Laboratory. Here time in the laboratory is scaled up (tides come in every twelve minutes) and size is scaled down (the whole of the Dutch river estuary is reproduced in one big laboratory hall). Law (1991c) describes a similar strategy for the case of aeroengine design. However, when other sociotechnical ensembles are used to achieve control, the scaling up and down may operate in the other direction. For example, in developing micro-automation technologies, it is often useful to build a laboratory model that is slower and larger than the final product.

8. This strategy also formed the core of Boyle's successful attempt to create a boundary between the inside and outside of science, as analyzed by Shapin and Schaffer (1985). The "material technology" of the air pump delineated the inside of science by defining the terms in which durable knowledge could be stated. Like the Dutch estuary model 300 years later, it defined the distinction between what were to be facts and what not.

9. See Collins 1981b, and Pinch and Bijker 1987.

10. For a careful analysis of structure and power as a distribution of knowledge, see Barnes 1988. Barnes does not, however, consider the role of technology in maintaining relations. For an initial attempt at this, see Law 1991a.

11. This argument may be mounted both for interests imputed by agents to themselves and for those imputed by others, including analysts. Roughly speaking, in both cases interests appear to be predictive attributes that function to link prospective structural features with the set of existing relations that constitute the actor.

12. See Staudenmaier's (1985) discussion of the notion of "constituency."

13. Misa and Bijker talk in this volume of the "amalgamation" of vested interests—a phrase with less restricted economic connotations.

14. For instance, the radical Conservative government of Mrs. Thatcher substantially altered the structure of the side bets of the British electricity supply industry.

This can be seen as a sociotechnical experiment on a huge scale. Its outcome—particularly in terms of patterns of investment in the generation of power—is unclear and is likely to remain so for some time. Some, including its critics, suggest that such investment decisions will henceforth be made on more local, short-term accounting grounds. Whether this actually happens remains to be seen. If, however, it indeed turns out to be the case, then Hughes's predictions about the holistic character of successful participation in sociotechnical systems will be incorrect—at least in this case. This is because the structure of side bets will have been radically altered. Of course, critics of the experiment might argue that Hughes is really right because the lack of holism encouraged by the introduction of local market considerations into electricity supply means that the future security of power supplies, and the overall long-term efficiency of the system, are both put at risk by the entry of a large number of players who calculate in terms of relatively short-term economic considerations. However, the jury is out, and is likely to stay out on this one until well into the next century!

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