On power and its tactics: a view from the sociology of science

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Abstract

This paper considers the relevance of the sociology of science for the study of power. Though there is by no means complete agreement between sociologists of science, recent work in this area has suggested that (a) scientists negotiate not only about scientific but also social reality, and (b) the distinction between macro- and micro-sociology is an impediment rather than an aid to analysis. Thus, though there are indeed differences in scale, it is argued that these should be seen as the outcome of differentially effective attempts by scientists to impose versions of scientific and social reality.

The present paper extends this argument by considering the way in which a set of pharmacological experiments was undertaken in order to generate results and control aspects of the scientific and social environment. It is suggested that the experimentalist acted like an entrepreneur, combining a variety of potentially unruly resources with the aim of simplifying these and reducing them to docile figures on a sheet of paper. The strategies and materials used in this process of control are considered and three classes of potential resources are identified: natural objects or devices, people and inscriptions. It is argued that these have certain properties that render them relatively durable and transportable and hence convenient for the purpose of long distance social control.

(1) The problem of power in the sociology of science

The problem of power, or how it is that actors and collectivities attempt to extract compliance from agents and natural objects that may resist such attempts, is surely one of the most fundamental problems of sociology. It lies behind much of classic social theory and continues to inspire the work of sociologists of widely differing perspectives. In this paper I suggest that recent work in the sociology of science is of particular relevance to the study of power. Indeed, I argue that it amounts to the outline of a general theory of social control. The purpose of the paper, then, is on the one hand to sketch out the elements of this theory, and on the other to contribute to its development.

The sociology of science, as Collins (1983:86) has recently noted, 'is a microcosm of the good, the bad and the ugly in modern sociology'. However, despite the fact that the debates that divide other areas in sociology are also to be found within the sociology of science, most sociologists of science accept certain ground rules that set them apart from many of their colleagues in other areas of sociology. The most fundamental of these are that scientific knowledge should be studied (a) with descriptive and explanatory rather than prescriptive aims, (b) as a form of culture and (c) on the assumption that it is related to and in some way a function of social life. These suggestions immediately distinguish sociologists of science from those who are committed to prescriptive, demarcatory and realist theories of science. The gap is further widened by the commitment by most sociologists of science to two methodological principles, those of impartiality and symmetry (Bloor 1976:5). These suggest, respectively, that analyses of scientific knowledge should (a) explain the origins of both true and false knowledge and (b) explain them in the same general terms. These, as critics are quick to indicate, commit its practitioners to a form of methodological relativism since the truth or falsity of the knowledge to be explained is irrelevant to analysis. However, many sociologists (and historians) of science remain committed to these principles first because much recent work in the philosophy of science suggests that knowledge is underdetermined by the natural world (Hesse 1974) and second because knowledge that is taken to be true, like that which is taken to be false, is nevertheless often visibly related to the social in one way or another.

This approach has led to a range of exemplary empirical studies in the history and sociology of science. These range from analyses of the role of background social interests for the production of (for instance) medical and statistical knowledge (Shapin 1979; Mackenzie 1978), through sensitive studies of the rhetoric of controversies and mechanisms for securing closure of debate in modern physics and parapsychology (Collins 1975; Collins and Pinch 1982), to detailed laboratory ethnographies in which the constructive character of scientific work is displayed and detailed

(Latour and Woolgar 1979; Williams and Law 1980; Knorr-Cetina 1981; Lynch 1985). Recently, however, a number of writers have expressed a degree of unease about certain aspects of such studies. This has been articulated in a number of ways of which I want to mention two.

First, some writers have noted that there is a certain oddity about a principle of methodological symmetry that applies only to the natural world but not at the same time to its social counterpart (Woolgar 1981; Lynch 1982; Callon and Law 1982; Callon 1985a; Latour 1985a). Sociologists of science, so the argument runs, make no assumptions about nature in the course of their explanations, for the structure of the natural world is precisely what it is that socially contexted scientists negotiate about. On the other hand, the principle of symmetry is not applied to the social world, for it is this that is taken to influence beliefs about the natural world, even though (and this is where the oddity emerges) it can be shown that *scientists negotiate about and attempt to impose versions not only of the natural but also the social world* (Latour 1983; Shapin 1984; Pinch 1985; Pinch and Bijker 1984; Callon 1985a).

Thus, to take one example, sociologists of science frequently offer explanations for the development of knowledge that are posed in terms of social interests imputed to actors or the groups to which they are presumed to belong. In this work it is normal practice to distinguish between scientists' talk about interests and the imputations of interests made by analysts and to argue that these are different in kind (Barnes 1981). The force of the traditional sociological view obviously depends entirely upon the notion that the professional sociologist has a more warrantable account of social interests than those whom he or she studies and that expressions by actors of their own interests or those of others must at best be seen as data for the hidden version of events that is visible only to the sociologists.[1] Naturally, this position is something to which many, perhaps most, sociologists would assent, but there is an alternative view which suggests that interests (and other social phenomena) are just as negotiable as the natural phenomena, the explanation of whose origins make up the standard fare of the sociologist of science. If this view is adopted then the role of the sociologist becomes that of discovering the methods by which actors and collectivities articulate conceptions of the natural and social worlds and attempt to impose these on others and the extent to which such attempts are met with success (Woolgar 1981; Callon and Law 1982; Latour 1983; Latour 1984;

Law 1985a). This view requires the adoption of a new methodological principle, that of *generalised agnosticism.* The latter states that the observer must maintain impartiality not only with respect to the scientific and technical arguments of scientists, but also with respect to any disagreements that there may be about the nature of social structure (Callon 1985a). In short, arguments about the latter must be taken as seriously as disputes about the former, and must not be 'censored' because they fail to accord with the views of the analyst. Since, however, the evidence also suggests that arguments about the character of natural and social reality are inextricably linked together in scientific and technical inquiry — that experts simultaneously negotiate about what kind of objects should inhabit both the natural and social worlds (Lynch 1982; Latour 1983; Shapin 1984; Callon 1985a; 1985b; Pinch 1985) — this view also necessitates the adoption of a second principle, that of *generalised symmetry.* This requires the observer to use the same terms and explanatory principles when he/she describes and accounts for the successes (or failures) of *both* the social *and* the scientific components of such disagreements (Callon 1985a). The argument, therefore, is that we need a single vocabulary when we describe the work of creating and imposing the social and the scientific, and we need a single, unified, way of talking about the successes and failures of such attempts.

The second problem which has been highlighted in recent social studies of science concerns the relationship between the macro- and the micro-social. Thus, on the one hand it has been argued that there is a tendency amongst sociologists of science to concentrate upon the micro-social, a thesis which has been in part sustained by adducing the recent popularity of laboratory ethnographies (Knorr and Mulkay 1983a:7). It has also been suggested that such studies can contribute to the analysis of the resolution of scientific controversy only with difficulty, since the latter almost always involves a 'core set' of scientists working at more than one location (Collins 1983:95). On the other hand, despite the many controversy studies that have been undertaken, it has so far proved difficult to make a link between the esoterica of technical content in contemporary physics on the one hand and the broader social content within which they occur on the other (Collins 1983:96). Again, some of those more historically oriented studies where such links have been proposed (Mackenzie 1978) have been criticised on both empirical and methodological grounds (Woolgar 1981; Yearley 1982). In particular it has been pointed out that alternative imputations of background social factors are possible on the basis of any given set of data.

Whether or not this disjunction between the micro- and macro-social should be seen as a problem is a matter for debate. Thus the defeasibility of imputations about large scale social structure, while formally correct, is not particularly damaging to explanations which depend on this since any theoretical redescription is ultimately defeasible. Again, it might be the case that there *are* no theoretically interesting links between the content of many areas of modern science on the one hand, and its broader social environment on the other (Law and Lodge 1984:220). However, faced with the latter possibility, certain writers have begun (a) to note that links between the macro- and the micro- are imputed by scientists themselves, (b) to suggest (in conformity with the principles of generalised agnosticism and symmetry) that sociologists should attend to the way such links are constructed and imposed by scientists and (c) to argue that the standard sociological distinction between macro- and micro- analysis should be seen as an impediment rather than an aid to analysis.

Those who make the latter argument of course accept that there are indeed differences in scale (Callon and Latour 1981; Latour 1983; Law 1984a). Thus some scientific innovations are successful and others are not, just as some organisations are large and control aspects, at least, of the activities of many other agents, whereas others are small and correspondingly limited in the degree to which they can do this. This does not, however, mean that it is either desirable or acceptable to treat such variations in scale as if they were the result of differences in kind susceptible to different types of theoretical and methodological treatment. Amongst other drawbacks, this theoretical division of labour has the effect of reifying those who are successful while obscuring the methods by which such large-scale social control is achieved and precariously maintained. Thus for such writers as Latour, the sociology of science should treat power as an *effect* of sets of variegated and differentially successful strategies to enrol others rather than as *cause* of that success (Latour 1985a; see also Foucault 1979). Indeed, Latour presses the argument even further by suggesting that the laboratory is a peculiarly potent location for the pursuit of politics by other means (Latour 1983:168). Citing the case of Pasteur, he notes that a laboratory may be able to reorganise social life by convincing large-scale actors that it is the proper spokesman for new and powerful entities. Thus Latour argues that

Pasteur made himself the spokesman for a new class of entity, the anthrax bacillus, and in so doing profoundly altered the relationship between French farmers, veterinarians and scientific laboratories. The suggestion, then, is that under certain circumstances laboratories may make new connections between the `macro-' and the `micro-', thereby altering well-established differences in scale and reshuffling the social and scientific landscape. Furthermore, such successes typically involve the introduction and representation of novel actors (e.g. bacilli) which posit connections between entities of a kind that are neither 'political' nor 'social' as these terms are generally understood. The role of the sociologist thus becomes that of exploring these new connections rather than seeking to explain them in terms of the operation of a few predetermined categories such as that of social interest.

If the principles of generalised symmetry and agnosticism are accepted and the a *priori* distinction between macro-social and micro-social analysis is abandoned, the sociology of science therefore starts to look very different. Instead, as it has in the past, of conceiving itself as a branch of the sociology of knowledge in which background and possibly large scale social factors known primarily to the analyst are used to explain the production of knowledge, it moves, instead to a study of the ways in which actors create and attempt to impose versions of the natural and social worlds upon one another. It moves, that is, to an analysis of the processes of 'translation' (Callon 1985a; 1985b) in which actors (including collectivities) struggle to impose versions of reality on others which define (a) the number of those others, both natural and social, that may be said to exist in the world, (b) their characteristics, (c) the nature of their interrelations, (d) their respective sizes and (e) their positions with respect to the actor attempting the translation. Since there are many such actors and many different versions of reality, this process is invariably uncertain and reversible, even when rewarded with success. Therefore just as, in the conventional sociology of science, natural reality is pictured as the end product of the interactions of scientists, so, in this new approach to the sociology of science, both natural reality and social structure have to be seen as the (shifting) end product of mutual attempts at translation.

Callon (1980; 1985b) considers the methods of translation for a particular case, that of the electric vehicle in France. In the early 1970s Electricité de France (EDF), the French monopoly power utility, argued that the days of the internal combustion engine were numbered, and that an environmentally acceptable transport system would depend, in the future, upon the development and widespread adoption of personal electric vehicles. EDF mapped out a scenario in which technical and social elements were inextricably intertwined. Thus on the one hand it would be necessary to develop fuel cells. This required scientific study of the behaviour of catalysts and hydrogen ions, as well as attempts to improve the performance of existing lead acid accumulators. On the other hand it would also be necessary to enrol such organisations as Renault, the car manufacturer. EDF suggested that the latter would no longer produce petrol-driven vehicles, but rather (in a modified and reduced scheme of things) manufacture the chassis of the new electric units. Meanwhile it was also both necessary to persuade consumers that their best interests lay with the development of the electric vehicle, and convince municipalities and government departments that they should adopt policies which discriminated against the internal combustion engine.

If EDF's scenario was to be fulfilled all such elements would have to be translated. It would, in other words, be necessary to harness, give appropriate expression to, and correctly interrelate a wide range of elements including hydrogen ions, industrial companies and consumers. Only if such translations were successfully achieved could the power of such entities be put to work by EDF.

In fact Callon shows that various elements in the EDF scenario were not successfully translated. Thus the catalysts failed to behave in the expected way, and were accordingly unable to contribute to a successful fuel cell. Since, as a consequence, the fuel cell also failed to play its role, it was in turn easy for critics of the plan (which included Renault) to resist the role that was being offered them. In the end EDF's attempt to build a world of social and natural entities was unsuccessful. Thus unlike Pasteur, EDF failed in its attempts to rework the relationships between other actors and thereby harness their power. This was in part, at least, because it failed to rework the connections between the large and the small scale. Specifically it failed to establish what Callon (1985a) elsewhere calls 'obligatory points of passage' for many of those it sought to enrol. It failed, in other words, to impose on others its view that their problems would only be resolved if they *passed through* EDF, its electric vehicle and its novel fuel cell. It failed to choke off alternatives (the petrol driven car) for consumers. It failed to persuade Renault that the firm had no economic future if it stayed with the internal combustion engine. And it failed to show that propulsive force might, in future, be economically produced as a result of the interaction between catalysts and hydrogen ions. Note that, by contrast, Pasteur succeeded in turning his laboratory and its products into an obligatory point of passage for the farmers, the veterinary surgeons and the cattle of France by collapsing the distinction between the macro- and the micro- (Latour 1983; 1984).

The notion of the obligatory point of passage is thus crucial to the analysis of translation. As a first approximation we may say that the actor that is able to force others to move along particular channels and to bar access to other possibilities is one that can impose him/herself upon those others (Law 1983). It follows, then, that this new version of the sociology of science, as it seeks to analyse the techniques of power, must focus in particular upon the methods by which actors seek to open up certain possibilities whilst denying others to those round about them. It also follows that these methods may be studied independently of the scale on which they are practised. As is the case for Foucault's (1979) microphysics of power, it is not necessary (though it may sometimes be helpful) to study translation on a large scale. Analysis of the small scale may be equally instructive.

In the present paper I follow this path and describe aspects of a series of experiments conducted by a pharmaceutical chemist called Jean-Paul Remon. As in the cases of EDF and Pasteur, Jean-Paul's work may be understood as a series of (attempted) translations. Working from the large to the small-scale we start in this case with patients who suffer from heart arrhythmia. These are individuals who may perhaps be persuaded to become consumers of anti-arrhythmic drugs because the latter open up new and attractive possibilities to them. But one of the problems with such drugs (and this is a problem for both patients and manufacturers) is that it is difficult to deliver them in a form that permits slow release and is hence clinically optimal. To make such drugs more effective and attractive — to turn them into an obligatory point of passage for the patient and the manufacturer —it would be desirable to develop slow-release preparations. This is where Jean-Paul's experiments become relevant. These, which took place in the summer of 1983, were designed to explore the possibility that slow release of a particular anti-arrhythmic drug called procainamide could be achieved by linking it to a polymer called dextran which would carry the drug to the heart and there release it slowly. However, earlier *in vivo* experiments had shown that the dextran-procainamide appeared to have no clinical effect and was thus far from being an obligatory point of passage for all concerned. However, the reasons for this failure were unclear. Was the combination simply unable to operate on heart tissue, in which case there was no chance of translating manufacturers or patients? Or was it rather the case that the drug never got a chance to work on the heart because it was first absorbed by some other kind of tissue? If the latter was the case, then it might, conceivably, be possible to find some other way of delivering the drug-polymer complex. There would be some residual hope that arrhythmia might be so prevented, drug manufacturers enrolled and patients turned into consumers. The point of the experiments was to test the latter possibility. Isolated but still living rat hearts were placed in a perfusion circuit and the drug was added to the circuit. Then, after the drug had perfused round the circuit and into the heart, an arrhythmia-inducing drug called aconitine, was added. The idea was that if dextran-procainamide was active in preventing arrhythmia in heart tissue, then the onset of arrhythmia induced by aconitine would be delayed.

The immediate problem for Jean-Paul when I joined him and started to observe his work was thus to determine whether dextran-procainamide would protect rat hearts from the arrhythmic effects of aconitine. His more general problem, and the one on which I will concentrate in what follows, was to find ways of translating the various bits and pieces that made up the laboratory in such a way that they might be used to convince first other scientists and subsequently drug manufacturers (and patients) that dextran-procainamide was (or was not) able to protect the human heart from arrhythmia. Analytically, then, the focus is on the methods available to Jean-Paul for reducing the distance and altering the relationship between the large scale (scientific colleagues, drug manufacturers) and the small scale (a few rat hearts, drugs and machines). It is, in other words, about the methods for turning his experiments into an obligatory point of passage for the high and the mighty and thereby exerting distant social control. Thus, though the experiments that I describe below are limited in scope, the methods used to conduct and report them should be seen as a paradigm case of an attempt to conduct politics by scientific means, and the conclusions about the methods of social control which may be drawn from their study may be generalised.

(2) Conversation, objects and figures: mobility and control

I start with a simple case of translation: Jean-Paul's attempt to translate *me* upon my arrival at the laboratory. It is true that this formed no part of his experimental work. It is also the. case that this task was relatively easy. I was not a sceptic, for example a potentially critical colleague. Rather, I was a visiting sociologist, someone who had already expressed an interest in his work. Nevertheless, since he had agreed to let me undertake participant observation and was, in some sense, committed to explaining his work to me, Jean-Paul was faced with the problem of locating me as a knowledgeable onlooker, a competent member of his local experimental subculture. How, then, did he attempt to translate me from an outsider into someone who understood the experimental work he was undertaking and did not fail to comprehend the significance of the equipment in the laboratory? To answer this question I want to consider both the *materials* that he employed and some of the *methods* that he used to fashion these materials.

Jean-Paul's experimental set-up was in a small room on the roof of the main laboratory. The room was incredibly hot, very cramped, and filled with a complex arrangement of glassware, pumps, water jackets, gas cylinders and recording equipment, all held together by a precarious array of clamps. My first reaction was that it looked very complicated — indeed about as comprehensible as an oil refinery. Jean-Paul set about explaining it. In the course of our conversation he made extensive use of ostension — he pointed at pieces of apparatus, all the while talking to me about how the lash-up worked. Part of our conversation ran as follows:[2]

036 JPR	. . . and the aorta puts it out. You know, the heart (beating . . .)
036 JL	(. . .)
036 JPR	. . . *pumps* the solvent. .
036 JL	Yes
036 JPR	. . . up here .. .
036 JL	Yes.
036 JPR	. . .because this is the left atrium reservoir . .
037 JL	Yeah.
037 JPR	. . . this is just the solvent reservoir, and you gas it — and there is a gas connection with the cylinder . . .
037 JL	Yeah

037 JPR	. . . in the back here, you see.
037 JL	Yeah, yeah.
038 JPR	It pumps it through this one, through the . . . I mean . . . to this expansion system here .. .
038 JL	Yeah.
038 JPR	. . . then through this resistance with the (peri-spheric) resistance and coming back into the reservoir, like this.
039 JL	Yeah.
039 JPR	Now this connection, you know these pumps circulate the first pump circulates from your (real) reservoir up to the atrial reservoir with an over-
-	flow system coming back into this reservoir.
041 JL	Yeah.
041 JPR	. . . while the second pump just takes the output, the coronary output of your heart, and pumps it again in your reservoir.
041 JL	Yeah, yeah.
041 JPR	So you don't lose any fluid.
041 JL	Mmm.

Note that I seem to go along with Jean-Paul's argument but note, also, the materials that he uses to make me go along with him. First, of course, he uses words. He also, however, uses gestures (which, of course, cannot be seen from the transcript) and material objects — parts of the complex experimental set-up. In other words he has a range of materials available to him — more than would have been the case had he been at one end of a telephone trying to describe the experiment to someone at the other end. Words, objects, gestures — the whole forms a network which has (so far as one can tell from the conversation) the consequence of keeping me firmly on the trajectory that he has marked out. He is attempting to translate me by using more than the social and more than the conversational.[3]

There is only one problem with the material bits and pieces that he has mobilised. Like the words that make up our conversation they are local. Neither gestures nor complex and breakable objects travel particularly well. The proof of this is that I cannot show them to you. If we except the cases of telephone conversation and taperecording (and I accept, at least for the first of these, that this is a major exception) then the same may be said of talk. These three materials seem, then, to be plausible resources for the purposes of face-to-face translation but are not in the first instance especially suited for long distance or large scale attempts at social control.[4] Jean-Paul's attempts at translation are, accordingly, strictly local in their impact. They are local to that room on that day. We cannot imagine that he will be a successful scientist if he has to take every potential sceptic up those stairs and into that sweltering heat.

Where, then, might more mobile materials be found, materials more apposite to large scale translation? A partial answer to this question lies in the fact that although I cannot take you back to the day in July 1983 when the conversation recorded above took place, what *I can do is* to reproduce the picture that Jean-Paul drew for me two days later, at a point when I was still unclear about the exact functioning of the various parts of the apparatus (see Figure 1). This picture, scruffy though it is, has survived the vagaries of time and reproduction: as a material form it has proved much more tractable.

I do not want to suggest that by virtue of its relative mobility the picture has, so to speak, an inherent ability to translate those who look at it. Just as a conversation may fail to persuade or inform, so too may a figure. The contexts in which both talk and figures are deployed are certainly crucial to their reception. Thus, though I do not have this on tape, the construction of the figure was accompanied by the same type of commentary as the one recorded above. I found myself in a context where words and gestures were used, but also (and this is crucial) the materiality of the apparatus was simplified, altered and rendered provisionally transportable by the fact it was put onto paper.[5]

Such methods of translation — methods for transforming materials with the aim of enhancing their capacity to control — are worth considering in some detail. The form of materiality is altered in a process of translation during which the less mobile is converted into the more mobile. Let me therefore indicate some of the methods of translation used by Jean-Paul as he drew his representation of the apparatus used in the experiment, a process that simultaneously involved drawing upon and drawing away from the equipment in question:

1 Various parts of the original apparatus are simply left out of the figure altogether. All the water-bath paraphernalia designed to maintain a constant temperature is, for instance, excluded. This is because the water-bath, jackets and pipes are not a part of the

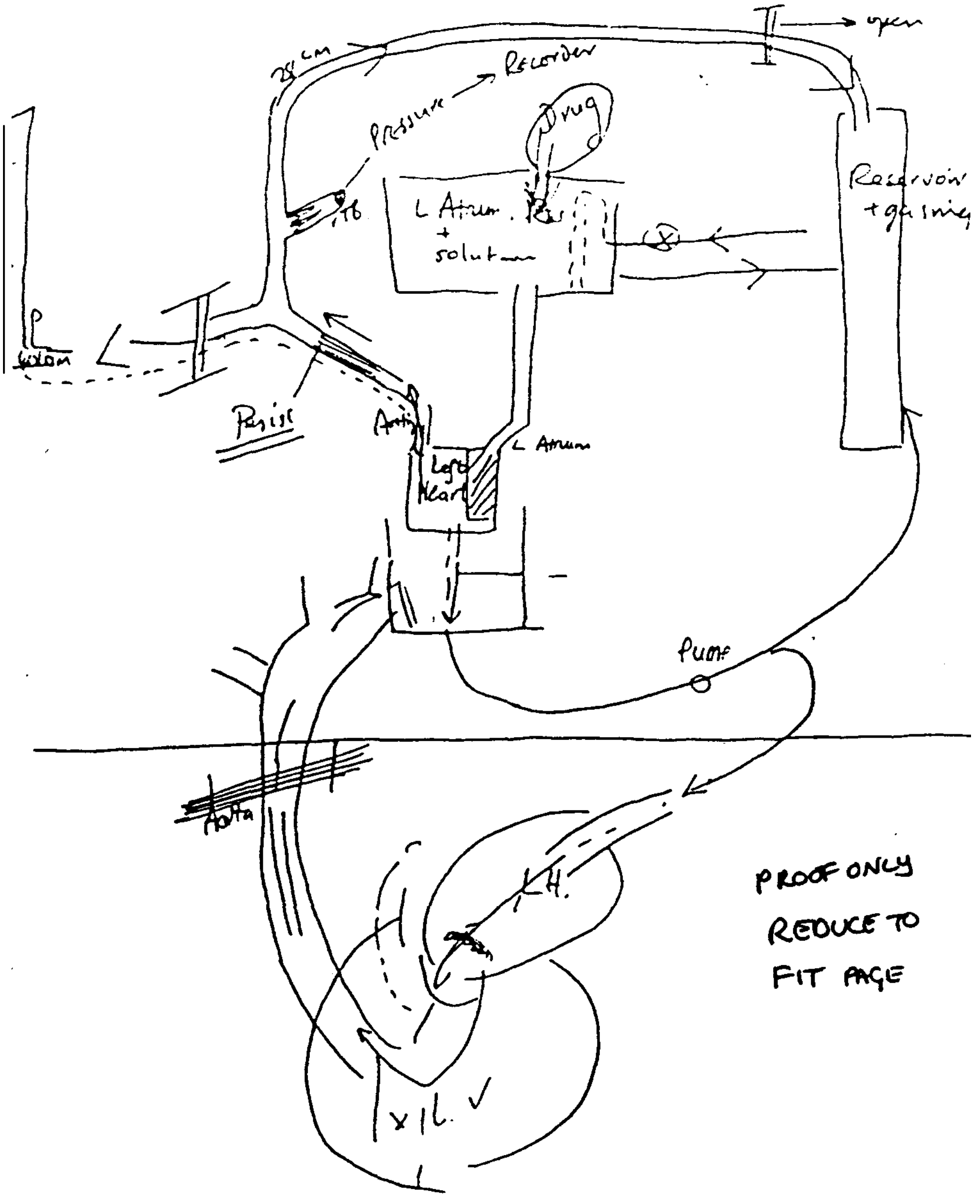

open
Recorder
Pressure
Drug
Reservoir
Left Heart
L Atrium
Aorta
Pump
PROOF ONLY
REDUCE TO
FIT PAGE

Figure 1

main circuit and would 'confuse' the picture. The basic circuit, often masked by the complexity of the warm water circulation, is foregrounded for the viewer by the simple expedient of doing away with the latter altogether. In other words, there is *suppression.*

2 Other parts of the apparatus are *scaled down.* I am, for instance, confused by the `Langendorf column' which is a section of the array used to supply Krebs' solution to the heart before the operation on the left atrium is complete. In order to make his point, Jean-Paul reduces the Langendorf column to a small appendix on the left-hand side of the figure. Again the `essential' central circuit is highlighted at the expense of `peripherals'.

3 On the other hand, other parts of the array are *scaled up.* Central to the whole experiment is the left side of the rat heart. This is represented by a small square in the middle of the upper part of the figure. But on response to further questioning, Jean-Paul draws another depiction of the heart in the lower part of the figure. This is many times larger than the rat-heart which in real life is about the size of a five pence coin. What is important is thus magnified.

4 Note that suppression, scaling down and scaling up may all be seen as forms of *schematisation.* However this, in its most general form, involves *distorting simplification.* Consider for example, the case of the heart system. This represents a version of the heart in which much has been suppressed and selected elements (the left atrium and the left ventricle) have been scaled up. However, as a part of the latter process, they have been subjected to a distorting simplification. No one has ever seen the left atrium and the left ventricle looking like that. The figure is not — could never be — a faithful representation of what it claims to depict.[6]

5 Jean-Paul makes use of a further important form of schematisation. Relevant parts of the figure are drawn in *cross section* — for instance the left heart system. This again has the consequence that the reader is able to 'see' features that were quite invisible in the laboratory. Note that selection is involved here too. Jean-Paul does not draw a cross-section of the two mechanical pumps — only the organic one — and neither does he depict all the anatomical details of the latter.

6 Conventional *labels* are selectively added with the effect of highlighting, once again, what it is that is 'central' to the

scheme. Pumps, aortas and the like are designated and arrows are drawn in order to reveal direction of fluid flow through the circuit.

7 Finally, it hardly needs to be added that the whole is *homogeneously* depicted on a two-dimensional paper surface in black ink. Much of the heterogeneity of the original is lost, but the new juxtaposition is more easily handled, altered, annotated, added to and transported.

This figure is designed to translate the person at whom it is aimed. It involves the definition of relevant elements, their characteristics, their relationships and their relative sizes with the aim of converting a naive newcomer into a competent observer. It does this in three stages. First it simplifies and alters what there is in the laboratory (for much is suppressed and distorted). Second, it juxtaposes the tractable end products of this process into a carefully constructed network. And third, this network of elements is more mobile than those from which it was drawn. Trivial though it is, this figure thus embodies *many of the basic strategies for translation.* However, it should be noted that the first two steps in this process are also to be found to some extent in conversational translation. The example of the latter that I described above certainly involved simplification, suppression and distortion and juxtaposition. The degree to which it was possible to practise these was, however, more circumscribed: it is, after all, relatively difficult to scale up, scale down, or cross-section in talk because the medium is in some ways less tractable. Our conclusion, then, should not only be that paper is likely to be a more mobile medium than talk or complex equipment, but also that methods for schematising on paper are more powerful than their conversational counterparts. Paper must therefore be treated as an important candidate material for long-distance translation.

To say this is not, as I have already indicated, to say that any given inscription will actually translate its recipient. This is because whether the translations attempted by the text or figure are successful depends upon the context in which they are received. Thus as I have already indicated, the elements which make up Jean-Paul's figure represent a simplification and a distortion of what was to be found in the laboratory — one that may or may not be acceptable to those at the receiving end. It is clear that the figure would not pass muster in a scientific journal, and one of the editors of *The Sociological Review* in fact suggested that it would need redrawing when he first flicked through the manuscript. Nevertheless, in the correct contexts (where the naive viewer is being helped to 'see' the way in which the fluid is pumped from the left atrium into the left ventricle and thence to the aorta, or where the figure may be treated as sociological data) it passes muster.

(3) Objects and hands: durability and control

Suppression, scaling up and down, cross-sectioning, labelling — in general the processes of schematising, simplifying and juxtaposing — these are the elementary forms of translation. When they are applied to materials that are mobile then it becomes possible to think in terms of long-distance social control. Until this happens, only local control is feasible. The laboratory may be seen as a location, one of many in contemporary society, for converting the immobile into the mobile.

In order to deepen the analysis of this process I now turn to Jean-Paul's experimental work which involved some extremely tricky surgery. The 'isolation' of the heart from the rat was not too difficult, though it posed its problems, but attaching the heart to the two cannulae was very awkward. As I indicated above, the rat heart is very small. It was necessary, first, to tie the end of the aorta — a semi-transparent tube the thickness of a pencil lead — onto one of the cannulae. To do this one had to tease the aorta onto the end of the cannula with tweezers, secure it temporarily with a small clamp, and then tie it on with cotton. This was not easy. However, the second stage of the operation was even harder. This involved finding the left atrium, making a tiny incision in its wall, and teasing the other cannula into this incision. Again, it was held in place by clamps until it could be tied by cotton. Finally, the third stage in the operation involved locating a number of extremely small pulmonary veins out of which the Krebs' solution would otherwise leak, and tying each of these off in turn. The whole procedure was difficult, very small scale, and was done against considerable time pressure — as a rule of thumb, unless the heart was properly attached to the apparatus within ten minutes of being removed from the body of the rat the chances of getting it to pump became rather small.

By the end of the period during which I observed him, Jean-Paul had become very competent at this procedure. At the beginning, however, it constituted a major, indeed *the* major obstacle to the successful completion of the experiment. The operation was both tense and dramatic. Consider part of an experiment undertaken on the afternoon of 6th July.

{The heart has been taken from the rat, placed in an ice cold saline solution to slow its metabolic processes, and Jean-Paul is now on his knees, in front of the experimental array, trying to tie the aorta to the cannula} [— clamping on aorta/tieing to orifice {I mean cannula}]
(45") {silence. Finally the aorta is secured)

197 JL The pump's not on.
197 JPR Sorry?
197 JL Pump.
198 JPR (. . .)
[— feed from {subsidiary Langendorf column) started.] {This feeds Krebs' solution to heart through left aorta until the circuit is complete through the left atrium incision.} [heart doesn't {start} pumping] (6") [{Jean-Paul} on knees — saying {199} 'come on!'] {very loudly. He is trying to persuade the heart to pump). (14")
201 JPR Naaaaaah {exasperation)
(68") [— hands shaking; trying to tie on left atrium incision]. {His hands continue to shake. It is very difficult to do the very small scale surgery that is needed. It is also incredibly hot, and he is sweating.)
211 JPR (. . .) come on! {the left atrium still won't go on) (22")
214 JPR (Nah)
(3") [—{I) steady {his) hand — finally get it on {i.e. atrium incision tied onto cannula}]
214 JPR Whooff! {what a relief!}
[4.38 pm – now trying to tie off pulmonary veins] {This is tricky. There are a number of them and they are very small. They only become visible when the main circuit is turned on – then Krebs' solution spurts out. On the other hand, they can only be tied off by cotton when the circuit is off. So it is a process

of trial and error that furthermore (as with a child trying to tie shoelaces) seems to require at least three hands.)
[— moves in sump/sink] {By this, I mean he adjusts the heart so it is surrounded by the water-jacketed heart chamber). [— switch on pump] {to drain heart chamber in case of leakage).
[— leaking left atrium — tied again] {It is not only the pulmonary veins that are leaking. The operation is going badly, and the atmosphere is tense.)

221 JPR Oh, the thing loses, (why?). {Its leaking).
(10") {He starts trying to tie off the pulmonary veins again)

222 JPR Oh
(6")

223 JPR Oh {There is still leaking)
(4")

224 JPR So shaky, you know. {His hands continue to shake. The success of the experiment is now in the balance)
(3")

224 JL Is that the incision? {I'm suggesting that it's the left atrium incision that is leaking, not the pulmonary veins)

224 JPR (Yes.)
(4") {He is trying to re-tie it, but his hands are shaking.)

225 JL Do you want me to try?

225 JPR Let me try again.
(11") {He tries again, but there is still a leak.)

226 JPR (It will be dead) {The heart cannot surely be alive after such a long period of operation).

I want to make three points about this dramatic and complex experiment. The first is that it is precisely with such very local methods of translation that scientists attempt to generate a context for the creation of elements that may turn out to be more mobile (Latour and Woolgar: 1979; Knorr-Cetina: 1981). Here, then, we see Jean-Paul trying to juxtapose and combine a wide range of variegated and local material elements (tweezers, scalpels, cotton, clamps, tubes, myself, and last and not least, his hands) into a whole. His aim is, of course, to force the rat-heart to play a particular role in the experiment. Yet though the materials upon which he is working are different, he is nonetheless combining physical elements in order to translate the rat heart in much the same way as he earlier combined physical, conversational and textual elements in order to translate me into a competent observer. There he was operating on the sociologist. Here what is at stake is a properly performing rat heart, one that may be linked to a measuring instrument in order to generate an inscription. The processes involved may be summarised in this way:

Elements to be linked	Elements used to make links	Elements monitored
Aorta to cannula (Two objects)	Hand, clamp, tweezers and cotton (objects and people)	Hand, clamp, tweezers, cotton and relative positions of aorta and cannula (object and person)
Heart to action of pumping (Object)	Talk	Action of heart (object)
Left atrium to cannula (Two objects)	Hand, clamp, tweezers and cotton (Objects and person)	Hand, clamp, tweezers, cotton and relative positions of left atrium and cannula (Objects and person).
	All the above plus my hand (Objects and people)	All the above plus my hand (Objects and people)
Pulmonary veins to watertightness (Two objects)	Hands, tweezers, clamp, cotton, turning circuit on and off (Objects, people)	Leaks (Objects)
Leaking fluid to recirculation (Two objects)	Hands and heart chamber (Objects and person)	Leaks and relative location of heart and chamber (two objects)
Me to Jean-Paul	Talk	Talk

Here, then, we see the attempted translation of a local array of elements, one in which the verbal is relatively unimportant and visual depictions are altogether irrelevant. On the other hand, objects and movements of the hand are central to the array. My second point concerns these objects. Successful translation involves the capacity to select or create, characterise and juxtapose elements into a *network* that has at least some degree of durability and which, by virtue of this, is in turn able to endow its components with individual and docile durability. Indeed, in many ways this aspect of translation is even more fundamental than mobility. This is because control is quite impossible if translated objects continually drop out of role. They themselves cannot be used to translate others so the latter in turn break free. Yet, as the above transcript indicates, this is constantly happening. Hearts cease to be viable shortly after being removed from the rat because they cannot be perfused because the pulmonary veins are not, in turn, tied off. But it is not only the animate that goes haywire. The above experiment does not illustrate this, but during the course of a two day period many of the inanimate features of the experimental array also went wrong. A crucial valve was left open and the Krebs' solution poured all over the bench. More solution was lost when gassing was undertaken at an excessive pressure. The chart recorder started going backwards. The procainamide stock solution went missing. The aconitine freezer container cracked. The gas supply was interrupted when we stood on the pipe. One reel of cotton kept on breaking. The two mechanical pumps constantly failed. The gas supply ran out. The chart recorder stopped inking. It also ran out of paper in the middle of an experiment.

Put like this it sounds as if Jean-Paul's experiments were a catalogue of disasters. In fact it was not that bad. Some of the failures were minor, and some resulted from inexperience and were rapidly overcome. Nevertheless, this rather impressive list indeed suggests that objects are not durably translated unless they are properly juxtaposed with one another. This suggests, as I indicated above, that durability is better seen as a function of the interrelation of translated objects rather than as something that naturally inheres in objects themselves. Specifically, they sometimes have to be held in place by the experimentalist. It follows, then, that *the scientist is a part of the network of elements that makes up his experiment,* just as the mechanic or the truck driver form a part of the network that constitutes a working vehicle. Of course, in one way this is a commonplace. However, I think that the interest of Jean-Paul's series of experiments lies not so much in the endless list of early failures (though this certainly helps to make the point that it may be difficult to translate objects). It lies rather in the way in which he monitored the relationship between his attempts at translation and the objects which he sought to translate. This he did in such a way that, after a very few days of such drill, he was able to turn his hands into a set of tools capable of maintaining the durability of the experimental set-up. Thus two days after the experiment described above his hands no longer shook when he attempted the same operation. They were faithful and indeed accomplished resources in the conduct of the experiment. Putting the heart onto the cannulae was no more of a problem than ensuring that the right quantity of Krebs' solution had been fed into the circuit. Thus, if the pressure transducer was a reliably operating and thus unexamined 'black box' throughout, then by the end of this period of drill the operation itself was on its way to becoming one, albeit one that required human intervention.'

My third point, then, is that the *drilled body,* like words, inscriptions and devices, deserves study in its own right if we are seeking to study the methods by which and the materials on which translation is attempted. Of course actions are as ephemeral as words. They last only as long as it takes to perform them and do not, on the face of it, look like promising materials with which to attempt long-distance control. On the other hand, the neatly packaged body is nearly as durable and mobile as an inscription, and is certainly a great deal more so than the majority of devices. However, its stability as a set of reflexively organised interventions on the environment is much more questionable. On the one hand, it offers enormous possibilities. On the other hand, as with all translated elements, there is the possibility, indeed the probability, that it may betray the translator. Jean-Paul's hands betrayed him in the course of the experiment described above. How much greater are the chances of betrayal if the hands belong not to the person that sent them out but to someone else? Methods for keeping people in line, for turning them so far as possible into mobile yet durable agents of those who seek to translate them, are surely central to long distance control. As Foucault (1979) and McNeil (1983) have indicated in their very different ways, drill and drilled bodies represent one of the central technical innovations in the exercise of power.

(4) The metrication of nature: tractability and control

I have argued that translation may be analysed as the operation of a range of methods upon particular materials to create obligatory points of passage. The effect of these methods (essentially those of simplification and juxtaposition) varies in scale. Some translations are local in import: they only work upon actors who are not far removed. Others may lead or at least contribute to control at a distance by generating durable and mobile emissaries which force those who are remote to treat the translator as an obligatory point of passage. I have also suggested that certain materials — notably people and inscriptions — are, everything else being equal, more mobile and durable than others. It does appear as if many objects and in particular talk, are ill adapted to the purpose of long distance translation.[8]

This suggests that if, as Latour has argued, the laboratory is a particularly potent location for the conduct of politics by other means, then this must be because it is able to transform materials with potential for only local control into materials such as inscriptions which are, indeed, relatively mobile and durable. In fact this is what we find. The immediate object of Jean-Paul's experimental work was to produce a trace which was taken to represent the pressure-wave created by the beating heart. The production of this trace was no simple matter, and a number of attempts were met with failure. What follows is taken from the first successful experiment. The operation on the heart had been completed without significant difficulty. The chart recorder (which was the device which was supposed to produce the trace) was switched on, but started to trace an almost straight line. In order to tell this story I now turn to my notes and the corresponding chart recording (Figure 2). (Numbers in square brackets indicate a match between that point on the chart recording and my notes.)

[A] 'Come on!' {Why is it not pumping? Why is the trace dead straight?)

[B] (Jean-Paul) sucks on tube {at top of circuit to try and help it start pumping).

[C] `Ah. That's it. It needs always a little help.' {He sees that the heart has started to pulsate).

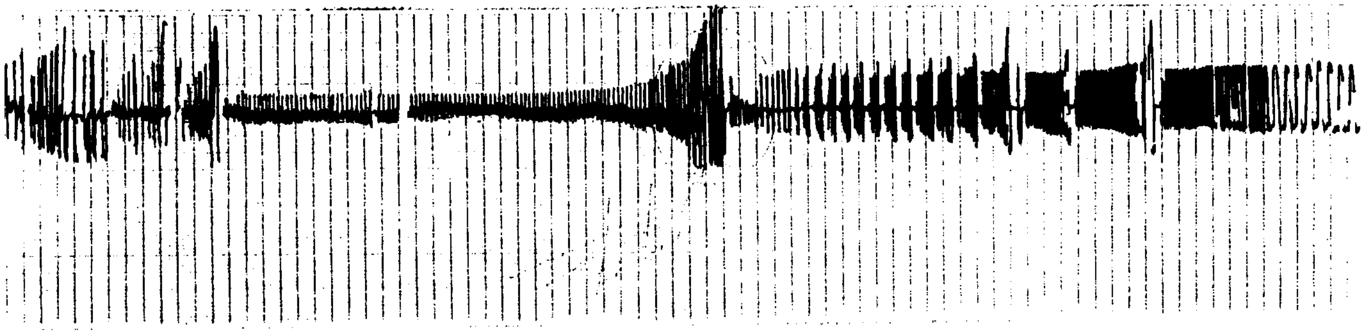
[D] {He) looks at recorder
`Now sir, what do you say?'

[E] — {He) calibrates it.

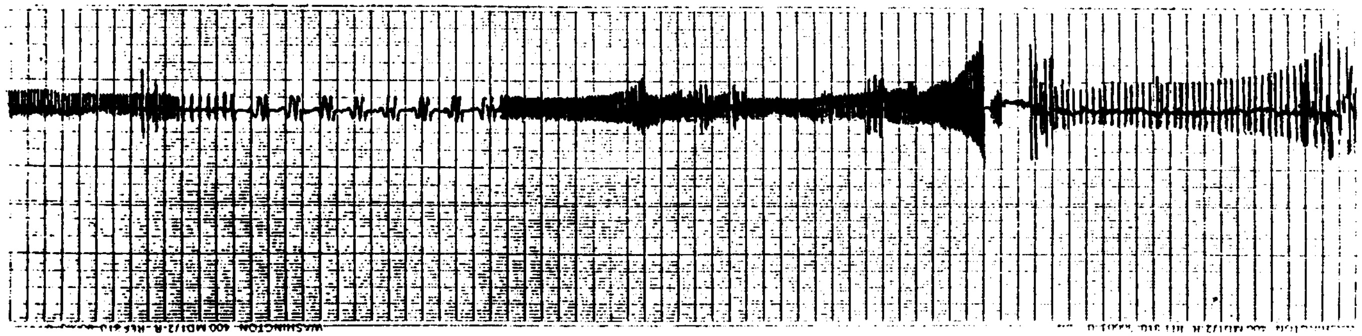
[F] — 'Perfect! Beautiful! That is perfectly great.' Slaps me on back! {The needle is tracing regular waves with an amplitude of about 3 ems)

[G] — slows down recorder {the individual waves are no longer clearly visible)
{He then waits a little while, watching the recording, and speeding it up again)

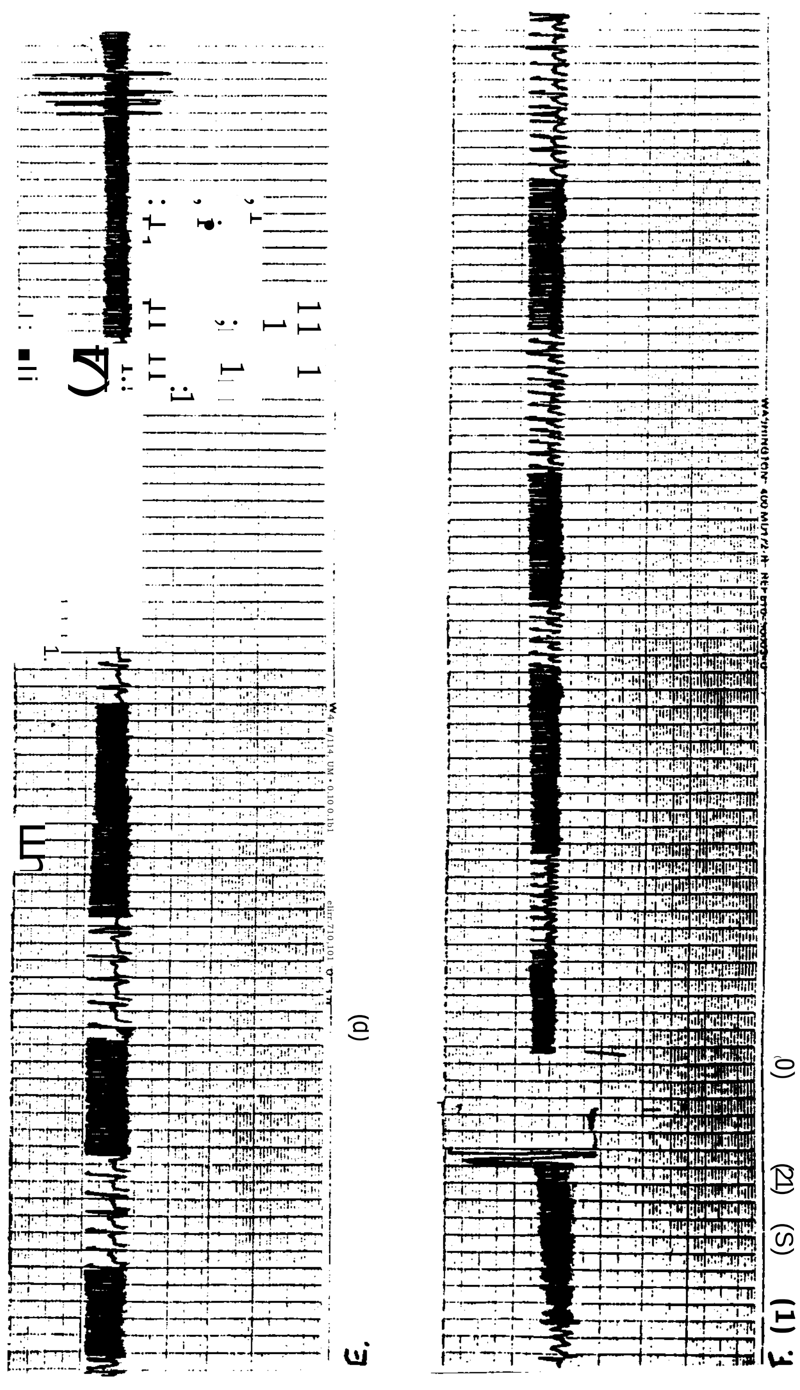
Figure 2



C. (L) (K) (J)(I) (H)



D. (N) (M)



[H] — puts in procainamide (into column reservoir)
[I] — recorder slowed down/not inking
[J] — fiddled with recorder (pushing the ink priming button)
[K] — 'arrhythmia'
[L] —`a bit of tachycardia'
— fierce oscillations
— 'Why? Is it pressing against the container wall?' {This would disturb the behaviour of the heart)
— no {there is no such problem)
{He starts discussing the reasons for tachycardia and arrhythmia)
— '1 micro grams in 1 ml. . . much too much. We'll try a dilution of 1 in ten next time. The only thing I could try to do to recover it if the receptors are not all blocked .. . You know what I'll
[M] do . . .' (looks at recorder, speeds it and
[N] then slows it)
`I just see if I can do something in adding aconitine'
[0] — puts in aconitine (and marks the point on the chart with a felt tipped pen)
`This is a perfect'
`If you had been a beautiful girl I would have kissed you'
{JL) 'Sorry. I'll go and get Sue.'
`Those are the things we say in the lab'
— aconitine apparently no effect.
`Well I am glad. This is a beautiful experiment'
[P?] JL 'Its getting faster'
JP 'Yes it is'
JL 'Faster and stronger'
{JP) 'Yes it is. And the time is short. We will see. It will give arrhythmia'
[0] fiddles {with calibration of chart recorder)
[R] JP 'It's fairly normal again' (as it speeds up)
[S] JP 'Doesn't start giving arrhythmia, does it?'
getting weaker
[T] {JP) 'arrhythmia'

Now the waves traced by the pen were no longer of regular amplitude. They increased and decreased over a period of about two seconds. But Jean-Paul was delighted. Leaving the experiment running he went out and came back a number of minutes later with a ruler. He measured the distance between the marked point [0] on the chart recorder at which the aconitine was administered and the point [T] at which arrhythmia was detected and declared this to be 80 centimetres. Working on the assumption that the chart recorder (at its slower speed) was tracking at 2.5 millimetres per second he performed a rough calculation: 800 divided by 2.5 equalled approximately 5 minutes. The experiment had been a success. The procainamide was toxic, but it nevertheless protected the heart against the effects of aconitine for five minutes.

This kind of translation, that which leads to the conversion of materials that are less into those that are more mobile and durable, is clearly of fundamental importance to long distance control. This is because it is thereby possible to create obligatory points of passage for actors that are spatially removed from the translator. Thus in the case of science, a number of authors have suggested that this may precisely be analysed as a process in which local events of one kind or another are converted into literary inscriptions (Gusfield: 1976; Latour and Woolgar: 1979; Knorr: 1981; Law and Williams: 1982; Law: 1983). Crucial to this process is what Latour and Woolgar call 'inscription devices'. An inscription device is:

> any item of apparatus or particular configuration of such items which can transform a material substance into a figure or diagram which is directly usable by one of the members [of the laboratory]. (Latour and Woolgar 1979:51)

Elsewhere these authors offer a somewhat broader definition that includes machines, pieces of apparatus and technicians (Latour and Woolgar 1979:58). In general, however, the importance of the inscription device for their analysis is that it converts local events into two-dimensional arrays on paper that are taken to stand in a direct relationship with those events. In this way it is possible to `bracket away' those events and the contingencies involved in their production and deal, instead, with more mobile and tractable inscriptions.

In one sense, therefore, anyone with a pen and a sheet of paper may be treated as an inscription device. I have already described the case where Jean-Paul drew a figure of his apparatus. I suggested that this involved a number of powerful techniques for converting features of the apparatus into traces on paper. In so far as the suppression, simplification, schematisation and sheer distortion involved in this was accepted by the viewer the materiality of an immobile set of objects was translated into the mobile form of a sheet of paper. I also noted that there were good reasons for saying that while this translation might be accepted by the visiting sociologist, it would not pass muster with the scientific community. However, the same cannot in general be the case for the kinds of inscriptions that end up as 'data' in scientific papers since the latter are often treated as unexceptionable by scientific colleagues. This is, as I have emphasised, in part a matter of context. The right drilled bodies must be located in the right places if a text is to be treated as an obligatory point of passage. This is, however, only one part of the answer. The other side of this coin is that inscription devices in science embody a set of powerful procedures for translation that are not to be found in ordinary figures and drawings. These have to do with metrication, and the consequent capacity to link inscriptions with mathematical and statistical techniques which form, so to speak, a part of the well drilled scientific body.

Let me, then, indicate some of the methods of translation embodied in Jean-Paul's use of the chart recorder and its traces:

1 As in the earlier figure, there is *suppression.* Almost everything that happened in the course of the experiment is left out. The size of the heart, the length of time taken to attach it to the cannulae, the construction of the apparatus, the way it is linked to the chart recorder — none of these are recorded. The basic `facts' of the experiment — that is, certain aspects of the pumping of the heart — are foregrounded by the simple expedient of doing away with everything else.

2 There is *scaling down.* First, the speed with which paper is fed through the recorder may be reduced. This 'squashes' time along the long horizontal axis of the chart. Second, the attenuation of the recorder may be increased. This has the effect of 'squashing' the vertical axis. The 'same' signal may thus take up less space on paper. Unlike the scaling down practised in Figure 1, however, there is a determinate relationship between the scales. Just as the experimenter intervenes to 'squash' the trace, he or she may later intervene to 'expand' important features of the reduced trace in order to render these comparable with their larger cousins.

3 Conversely, *scaling up* is also possible since the chart recorder may be speeded up. The importance of the capacity to scale up is that it permits details of the trace to be examined for salient

features when these might not otherwise be detectable. Notice that scaling up and scaling down are both features generated by the interaction of the chart-recording device, the heart, the associated experimental set up and the experimenter. All of these have to be fitted together in order to generate the traces on the chart.

4 Note that, as with the first figure, suppression, scaling down and scaling up may all be seen as forms of *schematisation.* There is, in other words, *distortion* and *simplification* on an epic scale. The chart recorder attends to one and only one feature of the behaviour of the heart. The complex quivering and pulsating that is visible to the naked eye is reduced to a simple two dimensional trace. Furthermore, once this trace has been generated, the experimenter attends to only certain of its aspects. A distinction is made between what is real and what is artefact. A further distinction is made between what is real but irrelevant and what is real and worth attending to. For the purpose of long-distance translation, certain features of a trace are much more potent than the whole heart pulsating on its cannulae.

5 Once again, *labelling* is important. Jean-Paul adds two kinds of labels to the chart. The first highlight certain features of the trace or moments in the experiment that he takes to be important. Thus, for instance, the sudden appearance of zig-zag traces of large amplitude between [L] and [M] is worthy of note: he writes on the recording that this is due to the addition of procainamide. However, not all events that lead to traces with a high amplitude are labelled. Some of them are 'uninteresting' reflecting [E] not behaviour of the heart but rather action by the experimentalist. Conversely, the point where aconitine was added [0] is labelled even though this is unremarkable in terms of the trace itself. This is because time-lapse measurements depend upon this datum. In this way certain features of the chart are highlighted and juxtaposed with features that would otherwise be invisible. A context, a set of elements, is thus built that makes it possible for other, mathematical, resources to be associated with the chart at a later stage in order that it may be converted from a chart into a number. However, the second kind of label is equally interesting. This is the title given to the entire chart. It is dated, the concentration of the procainamide is noted, and the fact that the procainamide was added via the main rather than the left atrium reservoir is also indicated. With

this 'title' the chart is linked to certain 'incontrovertible' facts that have the effect of turning it into a document that is detached from most of the circumstances of its production and which might, perhaps, be produced for later scrutiny. Thus Jean-Paul might look at it himself if he wanted to check the effect of 10 micrograms per ml. of pure procainamide on the rat heart and compare this with (say) the effect of half the dose. On the other hand, it might be treated as a set of elements with which to translate a sceptic into a believer. In this way, then, the chart begins to become durable and transportable, not only in the sense that it might be carried around but also because it is better able to resist the dissociating attacks of scientific sceptics.

6 Vital in this process of translation is the process of *homogenisation.* This is because, as in the case of the picture of the apparatus, we start with a range of heterogeneous objects and attributes that can only be translated at the particular time and place the experiment was carried out. We conclude, however, with a set of inked traces that are similar in kind and may, accordingly, be compared with one another and juxtaposed into novel relations. Therefore if the trace may be treated as a legitimate recording of the experiment then the experimenter multiplies his or her capacity to define and interrelate the objects that have been studied many times over. However, the traces generated by the chart recorder are susceptible to a much wider range of manipulation than the drawing created by Jean-Paul.

7 There is, most importantly, *metrication.* The paper is ruled into areas of equal size, and (in the absence of intervention) passes through the machine at a constant rate in a particular direction. In addition, again in the absence of intervention, the amplitude of the traces generated by the machine is in direct proportion to the strength of the pressure-wave generated by the heart. As I have already indicated, though scaling up or down will influence the size of the trace, in principle the effect of these interventions may be controlled. *Distance* and *direction* thus become vital. Such metric properties make it potentially possible to link features of the traces to a wide range of other resources which range from rulers to statistical procedures. The creation of graphs with directional and metric properties is thus a method of central importance for the translation of nature.[9]

8 As it happens, in this experiment Jean-Paul was relatively little concerned with absolute (vertical) amplitude. However, horizontal distance was very important. This was because direction and

distance, together with rulers and elementary arithmetic, were convertible to time — in other words to a figure susceptible to treatment by the entire range of statistical techniques. This conversion was possible in two ways. First, by taking account of variations in recording speed it was possible to use the scale printed on the paper to determine the time between significant events. Second the chart-recorder itself generated another trace — a horizontal line that marked the passage of each second with a small vertical trace.[10] The importance of this capacity to convert time into space cannot be too strongly emphasised. Temporal juxtaposition becomes spatial juxtaposition, and easily transportable spatial juxtaposition at that. In this way particular bits and pieces awkwardly related together at a particular place and in a particular sequence may be torn from the context of their production and taken elsewhere. *The chart recorder is a key to the translation of events in both space and time.*

9 Jean-Paul operated on the chart recording to turn it into a single number — a number that represented the time that elapsed between the introduction of aconitine into the system and the onset of arrhythmia. To do this he used a ruler. He simply measured the distance between the two events as recorded on the chart, and assumed that the paper moved 2.5 millimetres per second. This was only an approximate calculation, since it does not adequately account for the effects of speeding and slowing of the recorder. Nevertheless, a more defensible figure may be very quickly obtained and, in this way, the process of translation moves on a stage further. What started as a range of heterogeneous elements was first converted into a trace. The latter has now, in turn, been translated into a figure that may be juxtaposed and combined with other such figures using a range of statistical techniques.

The chart recorder, properly juxtaposed with other aspects of the experimental array, thus acts, like other inscription devices, to convert what is *less* mobile and durable into something that is *more* mobile and durable. In doing this it makes possible the creation of obligatory points of passage for those who are far removed, though whether they will feel obliged to pass by Jean-Paul's research findings is, as I have earlier noted, a function of their context and the way that they have been drilled. My claim here is not, however, that Jean-Paul's work is seen as right or important. It is that he is making use of a fundamental strategy for long-distance control: that of converting the less into the more mobile and durable. It is only by so doing that it is possible to make a link between the large and the small-scale, to collapse the distance, so to speak, between the macro- and the micro, and exert influence upon the social world from a particular place.

Note, however, an additional feature of Jean-Paul's conversion of a pulsating heart into a trace and then into a figure. This process has the effect of increasing his capacity to manipulate the elements that make up his experiment. It is, as we have seen, difficult to control the heart, the cannulae, the Krebs' solution and all the experimental bits and pieces. Once, however, a trace has been generated then the medium in which he is working becomes much more *tractable* by virtue of its homogeneity and its metrical properties. It is possible, as I have indicated, to combine together traces and figures from a range of different experiments, dispersed in time or space. It is possible to manipulate those figures to show means and standard deviations. They may be juxtaposed with other literatures and arrayed together in academic papers. The possibilities are endless, but they are endless because it is possible to work with traces in a way that is quite impossible with objects. I am saying, then, that just as bodies may be turned into docile agents in which a range of skills are juxtaposed in a novel manner, so traces may be seen as juxtapositions of elements which would not normally have been combined. In other words, traces constitute a medium that is not only mobile and durable, but also one that is eminently tractable.

(5) Conclusion

I have argued that new work in the sociology of science marks an important contribution to the sociology of power. With the abandonment of a *priori* distinctions between the macro and the micro on the one hand, and the social and the natural on the other, we find that scientists, like other actors, are concerned to articulate conceptions of the world and the roles of the actors that are in it, and impose these conceptions on one another: in short, they are concerned to *translate.* Their translations are successful in so far as they manage to impose their work as an obligatory point of passage upon those round about them. And their work is spectacularly successful — it becomes 'politics by other means' — in so far as they are able to collapse the distinction between the large and small scale by forcing macro-actors through their laboratories. The question, then, is what are the tactics and materials of which scientists make use as they attempt to create obligatory points of passage?

In this paper I have discussed a range of such materials. I started by considering words, objects and gestures. These are important for face-to-face translation, but they suffer from two major disadvantages when it comes to long distance control: they are neither particularly mobile, nor especially durable. It seems, then, that the pursuit of power, if it is to be carried beyond the face-to-face, has to find materials that possess these properties. There are two obvious candidates for this role: drilled bodies and inscriptions. Bodies are neatly packaged and relatively mobile. The problem with them, from the standpoint of social control, is that they may betray the translator. I showed the way in which Jean-Paul's hands betrayed him in the course of his experimental work, but the risk is much greater when the hands and the voice no longer belong to the translator but to one of his/her agents. Methods for keeping bodies in line, for rendering them docile, are surely, as Foucault has argued, central to long distance translation.

The second candidate, that of inscriptions, may be equally important for the collapse of the macro and the micro and the creation of obligatory points of passage. This is because paper is not only mobile and durable, but the traces which are drawn on it are also potentially tractable. It is relatively difficult to translate a heterogeneous collection of physical objects and organisms. It is relatively easy to manipulate homogeneous symbols on paper.[11] At worst, it may be necessary to do this discursively, as in the present paper. At best, as in the case of a large part of the natural sciences, it may be possible to do this mathematically. A metrical space is defined by the operation of inscription devices which convert the heterogeneous into the homogeneous.

The processes of converting materials that are less mobile, durable and tractable into materials that have these attributes to a greater degree is thus, I suggest, central to long distance translation. To say this is not, however, to say that all such conversions necessarily lead to the creation of obligatory points of passage. Many fail, as is witnessed by the apparent fact that the majority of scientific papers are never read at all. If success is to be achieved, if the point of passage is to be made obligatory, then at least two interconnected criteria have to be fulfilled. First, the conversion from materials with lesser to those with greater mobility, durability and tractability must be warrantable. And second, the materials have to be sent to the right context, a context where they are able to impose a structure on materials that are less mobile, durable and tractable than themselves. The two are interconnected, because what counts as a warrantable connection between materials is precisely a function of the context to which they are sent.[12] If science is powerful it is because it has created a network of locations where there is some agreement about warrantable connections: where the same types of docile bodies, texts and machines are all available to localise what had been delocalised.[13] And this network of agreement is, at least in part, embodied in machines (Latour and Woolgar 1979; Pinch 1985). Thus instead of the endless literary work that is the hallmark of the social sciences as they labour to translate local events into textual form, natural science is widely characterised by machines that are warranted to convert local events into traces. And if science, as an institution, is powerful then this is because it has succeeded, as Latour has argued, in scientising parts of social life. The farm, or the family medicine chest, or the public water supply, or the exhaust system of cars have become laboratories in their own right and those who work in or use them have been turned into laboratory technicians.

Science is, therefore, a particularly potent arena for the pursuit of politics by other means. The lesson to be drawn is, however, more general in character. Power may be seen as an *effect* of the creation of a network of mobile, durable yet tractable agents that have been sent out in *one-anothers' company.* A text by itself will be ignored. A person will be snubbed. A device will rust. But if the three are put together it may become, as the case of Pasteur suggests, more difficult to ignore them. Under the right circumstances the effect is that of power.[14]

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Notes

1 This point is made by Woolgar 1981; Callon and Law 1982; Lynch 1982; Latour: 1983. Note that a parallel debate has taken place in the Marxist analysis of politics: see, for instance, Hindess 1982.

2 The conversation took place in English, as recorded. In transcription, the following conventions have been used: dots in round brackets indicate talk; words in round brackets indicate uncertain transcription; dots at beginning and end of transcribed talk indicate talk continuing simultaneously with interjection from the co-conversationalist; dots within sections of talk indicate pauses of less than a second; seconds in brackets indicate a longer pause (with length) in talk; contents of square brackets indicate the content of my notes at the time of talk; contents of curly brackets indicate later comments added to make the transcript and notes clearer.

3 He is engaged in what I have elsewhere called heterogeneous engineering — the juxtaposition of various *different types* of bits and pieces in order to exercise control. See Law 1985a.

4 For a discussion of the mobilisation of laboratory findings see Callon: 1985a.

5 For similar analyses of this process see Shapin 1984 and Lynch 1985.

6 On the process of simplification in science see also Star 1983.

7 On the notion of 'black box' see Callon 1981 and Law 1984b.

8 Note that for certain objects, and in particular devices, this is not true. See Law 1985c.

9 For an attractive analysis of this point see Lynch 1985.

10 This may be seen at the bottom of the chart.

11 This point has been made by Eisenstein (1979) in her magisterial study of the role of the printing press. She also points out that the ease with which texts may be reproduced by the printing press is important. Once texts were widely disseminated it became possible for the scholar to assemble these at one location and compare them. The life of the itinerant scholar moving from library to library in Europe was thus rendered obsolete. These points have been discussed in Latour 1985b.

12 It needs to be emphasised that mobility, durability and tractability do not inhere in particular materials as natural properties of these. They rather arise out of the way in which materials are juxtaposed with others. This not only means that particular durable and mobile materials may fail to translate. It also means that new classes of materials appropriate for long distance control may come into being. For this argument see Law 1985c.

13 Collins is pointing at this, albeit in quite different language, when he talks of the importance of the 'core set' (1981) and notes that self-contained studies of

laboratories cannot explain the success of scientific theories. The point is made explicitly in Latour 1983; 1984.

14 For this point applied to the Portuguese imperialist expansion see Law 1985c.

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