LABORATORY LIFE

The Construction of Scientific Facts

Bruno Latour · Steve Woolgar

Introduction by Jonas Salk

With a new postscript and index by the authors

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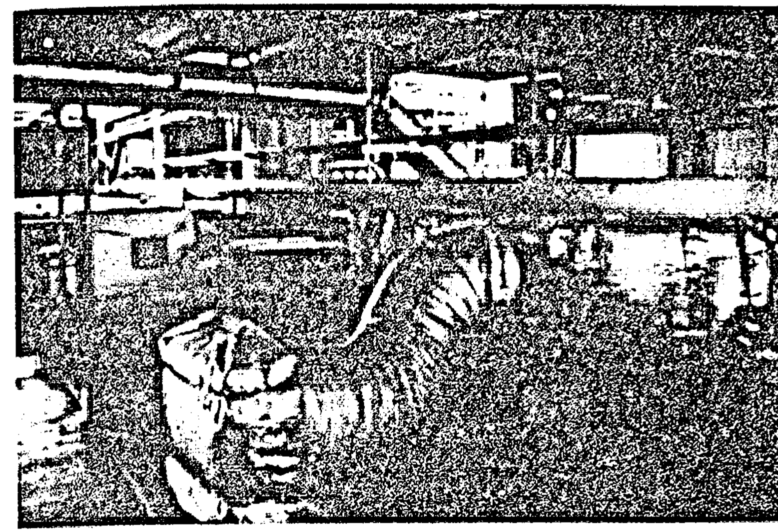

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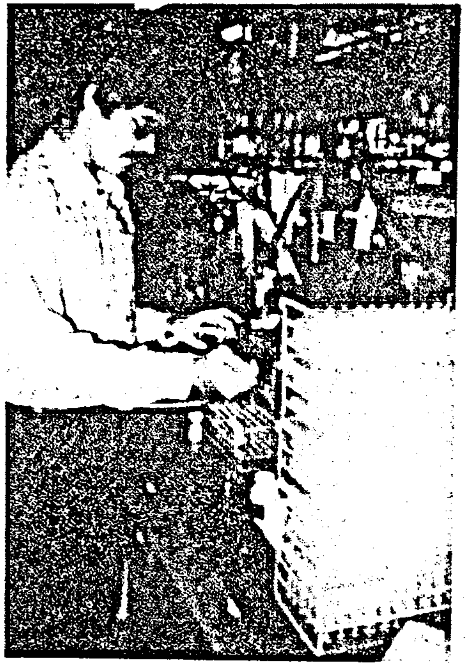

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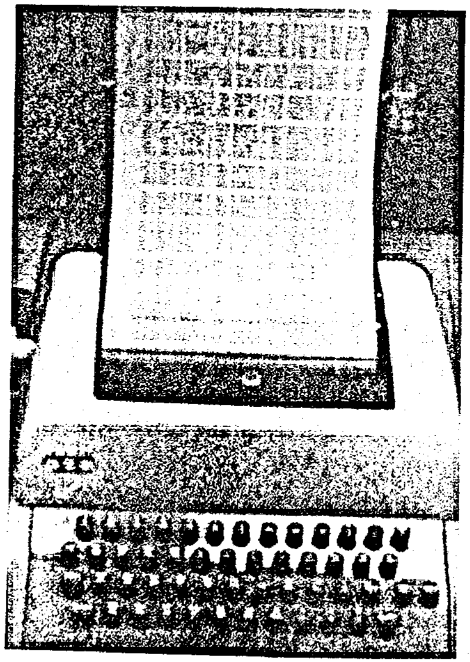

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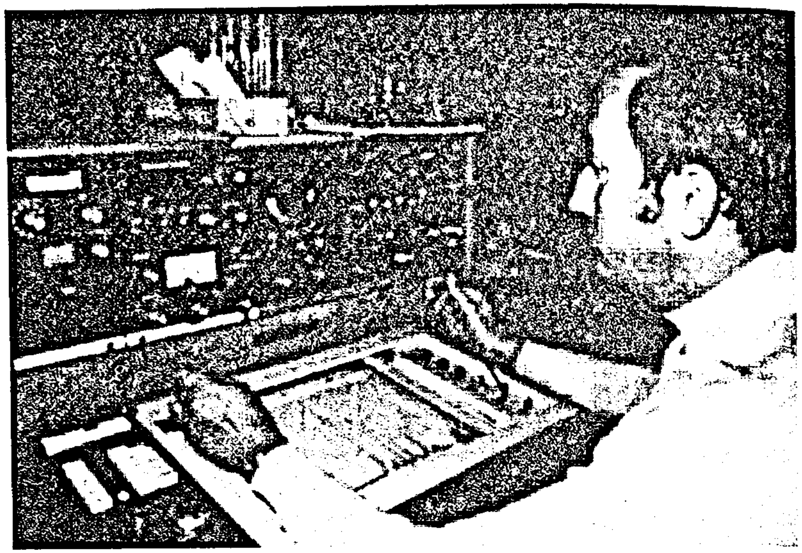

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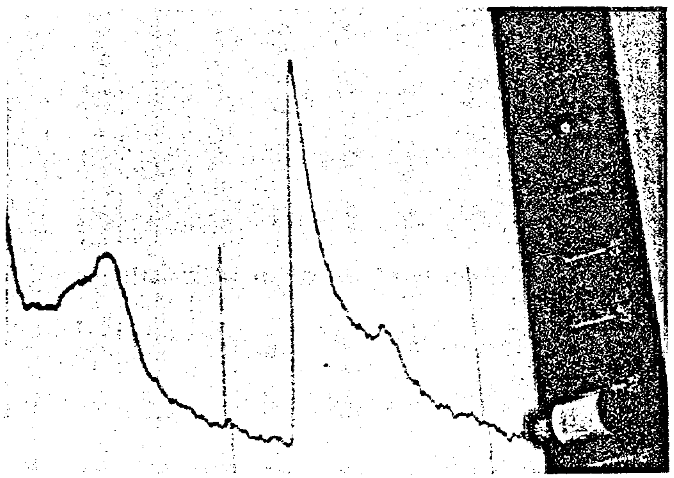

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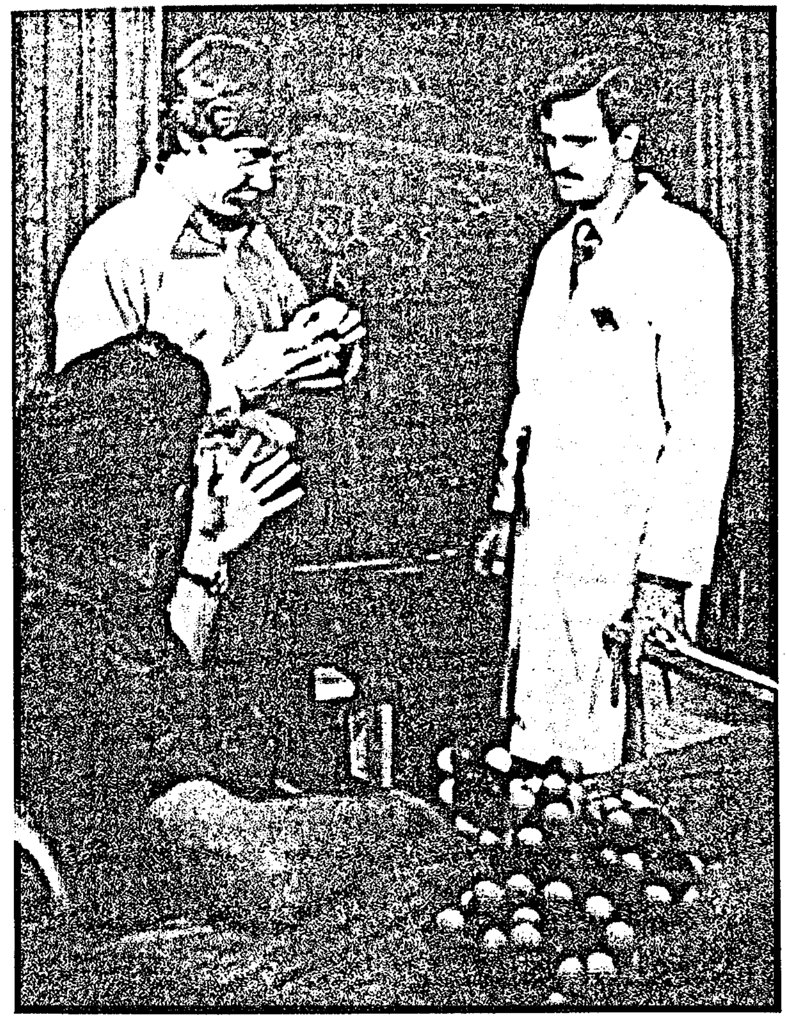

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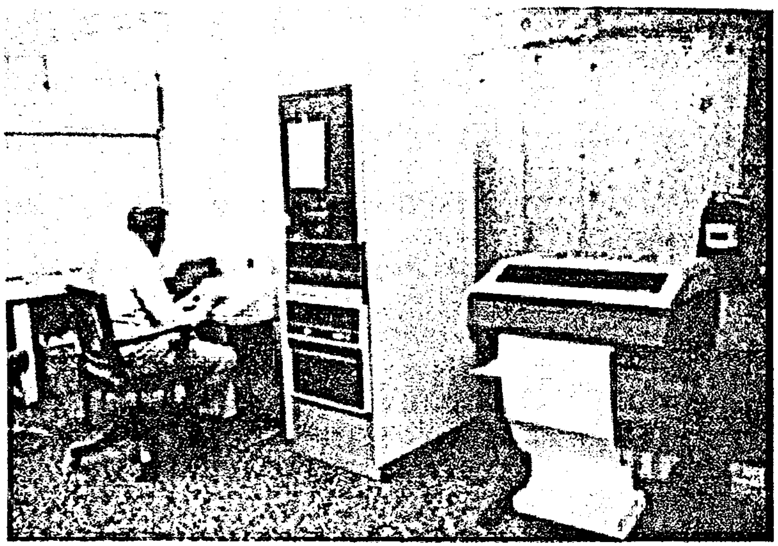

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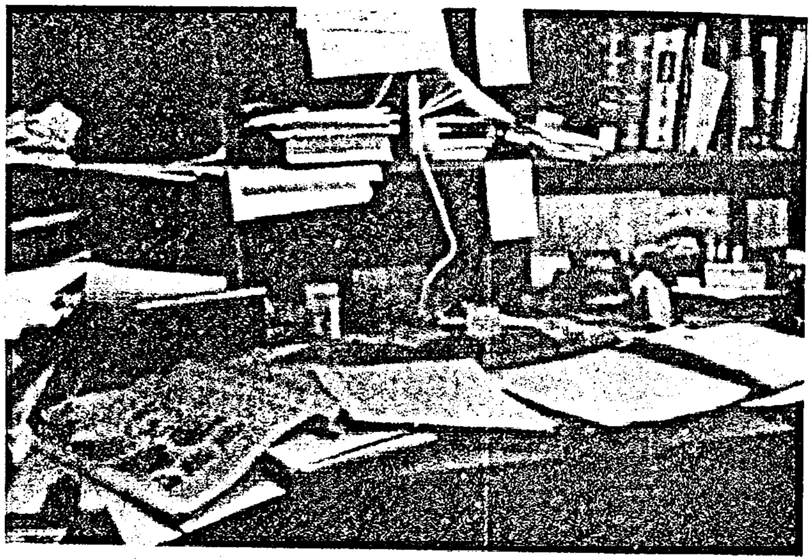

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Chapter 6

THE CREATION OF ORDER OUT OF DISORDER

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In examining the construction of facts in a laboratory, we have presented the general organisation of the setting as constituted by someone unfamiliar with science (Chapter 2); we showed how the history of some of the laboratory's achievements could be used to explain the stabilisation of a "hard" fact (Chapter 3); we then analysed some of the microprocesses by which facts are constructed, looking especially at the paradox of the term fact (Chapter 4); we then turned to the individuals in the laboratory in an attempt to make sense both of their careers and the solidity of their production (Chapter 5). In each of these chapters we defined terms which were often in contradistinction with those used by scientists, historians, epistemologists, and sociologists of science. We shall now summarise the various findings of these preceding chapters in an attempt more systematically to link the different concepts used. At the same time, we shall review some of the methodological problems encountered so far. It will not have escaped the reader's notice, for example, that a major problem arises from our contention that scientific activity comprises the construction and sustenance of fictional accounts which are sometimes transformed into stabilised objects. If this is the case, what is the status of our own constructed account of scientific activity?

In the first section of this chapter we summarise the argument so far. Instead of simply following the presentation of the preceding chapters, however, we identify six main concepts used throughout and show briefly how they are related. This leads us to the second section. Here we introduce one further notion, the concept of order from disorder, which enables us to situate our argument in the more general framework of sociology of science. Finally, in the third section, we compare our own account with those of the scientists whose activity we claim to have understood.

Creating a Laboratory: The Main Elements of Our Argument

The first concept used in our argument is that of *construction.* (Knorr, in press). Construction refers to the slow, practical craftwork by which inscriptions are superimposed and accounts backed up or dismissed. It thus underscores our contention that the difference between object and subject or the difference between facts and artefacts should not be the starting point of the study of scientific activity; rather, it is through practical operations that a statement can be transformed into an object or a fact into an artefact. In the course of Chapter 3, for instance, we followed the collective construction of a chemical structure, and showed how, after eight years of bringing inscription devices to bear on the purified brain extracts, the statement stablilised sufficiently to enable it to switch into another network. It was not simply that TRF was conditioned by social forces, rather it was constructed by and constituted through microsocial phenomena. In Chapter 4, we showed how statements are constantly modalised and demodalised in the course of conversations at the laboratory bench. Argument between scientists transforms some statements into figments of one's subjective imagination, and others into facts of nature. The constant fluctuation of statements' facticity allowed us approximately to describe the different stages in the construction of facts, as if a laboratory was a factory where facts were produced on an assembly line. The demystification of the difference between facts and artefacts was necessary for our discussion (at the end of Chapter 4) of the way in which the term fact can simultaneously mean what is fabricated and what is not fabricated. By observing artefact construction, we showed that reality was the *consequence* of the settlement of a dispute rather that its *cause.* Although obvious, this point has been overlooked by many analysts of science, who have taken the difference between fact and artefact as given and miss the process whereby laboratory scientists strive to *make* it a given.[1]

The second main concept which we have used constantly, is that of *agonistic* (Lyotard, 1975). If facts are constructed through operations designed to effect the dropping of modalities which qualify a given statement, and, more importantly, if reality is the consequence rather than the cause of this construction, this means that a scientist's activity is directed, not toward "reality," but toward these operations on statements. The sum total of these operations is the agonistic field. The notion of agonistic contrasts significantly with the view that scientists are somehow concerned with "nature." Indeed, we have avoided using nature throughout our argument, except in showing that one of its current components, namely the structure of TRF, has been created and incorporated in our view of the body. Nature is a usable concept only as a by-product of agonistic activity.[2] It does not help explain scientists' behaviour. An advantage of the notion of agonistic is that it both incorporates many characteristics of social conflict (such as disputes, forces, and alliance) and explains phenomena hitherto described in epistemological terms (such as proof, fact, and validity). Once it is realised that scientists' actions are oriented toward the agonistic field, there is little to be gained by maintaining the distinction between the "politics" of science and its "truth"; as we showed in Chapters 4 and 5, the same "political" qualities are necessary both to make a point and to out-manoeuvre a competitor.

An agonistic field is in many ways similar to any other political field of contention. Papers are launched which transform statement types. But the many positions which already make up the field influence the likelihood that a given argument will have an effect. An operation may or may not be successful depending on the number of people in the field, the unexpectedness of the point, the personality and institutional attachment of the authors, the stakes,[3] and the style of the paper. This is why scientific fields do not display the orderly pattern with which some analysts of science like to contrast the disorderly tremors of political life. The field of neuroendocrinology thus comprises a multitude of claims and many substances exist only locally. For example, MSH releasing factor exists only in Louisiana, Argentina, and one place in Canada, and in one other in France; most of the associated literature was considered meaningless by our informants.[4] The negotiations as to what counts as a proof or what constitues a good assay are no more or less disorderly than any argument between lawyers or politicians.[5]

Our use of agonistic is not meant to imply any especially wicked or dishonest character attribute of scientists. Although scientists' interaction can appear antagonistic, it is never concerned solely with psychological or personal evaluations of competitors. The solidity of the argument is always central to the dispute. But the constructed character of this solidity means that the agonistic necessarily plays a part in deciding which argument is the more persuasive. Neither agonistic nor construction have been used in our argument as a way of undermining the solidity of scientific facts; the reason for our nonrelativist use of these terms will be clear in our discussion of the third main concept used in our argument.

We have insisted on the importance of the material elements of the laboratory in the production of facts. For instance, in Chapter 2 we demonstrated how the very existence of the objects of study depended on the accumulation inside the laboratory walls of what Bachelard has called "phenomenotechnique." But this allows us only to describe the equipment of the group at one point in time. At some earlier point, each item of equipment had been a contentious set of arguments in a neighbouring discipline. Consequently, one cannot take for granted the difference between "material" equipment and "intellectual" components of laboratory activity: the same set of intellectual components can be shown to become incorporated as a piece of furniture a few years later. In the same way, the long and controversial construction of TRF was eventually superceded by the appearance of TRF as a noncontroversial material component in other assays. Similarly, we briefly indicated, at the end of Chapter 5, how investments made within the laboratory were eventually realised in clinical studies and in drug industries. In order to emphasise the importance of the time dimension, we shall refer to the above process as *materialisation*, or *reification* (Sartre, 1943). Once a statement stabilises in the agonistic field, it is reified and becomes part of the tacit skills or material equipment of another laboratory.[6] We shall return later to this point.

The fourth concept upon which we have drawn is that of *credibility* (Bourdieu, 1976). We used credibility to define the various investments made by scientists and the conversions between different aspects of the laboratory. Credibility facilitates the synthesis of economic notions (such as money, budget, and payoff) and epistemological notions (such as certitude, doubt, and proof). Moreover, it emphasises that information is *costly*. The cost-benefit analysis applies to the type of inscription devices to be employed, the career of scientists concerned, the decisions taken by funding agencies, as well as to the nature of the data, the form of paper, the type of journal, and to readers' possible objections. The cost itself varies according to the previous investments in terms of money, time, and energy already made.[7] The notion of credibility permits the linking of a string of concepts, such as accreditation, credentials and credit to beliefs ("credo," "credible") and to accounts ("being accountable," "counts," and "credit accounts"). This provides the observer with an homogeneous view of fact construction and blurs arbitrary divisions between economic, epistemological, and psychological factors.[8]

The fifth concept used in our argument, albeit somewhat programmatically, is that of *circumstances* (Serres, 1977). Circumstances (that which stands around) have generally been considered irrelevant to the practice of science.[9] Our argument could be summarised as an attempt to demonstrate their relevance. Our claim is not just that TRF is surrounded, influenced by, in part depends on, or is also caused by circumstances; rather, we argue that science is entirely fabricated out of circumstance; moreover, it is precisely through specific localised practices that science appears to escape all circumstances. Although this has already been demonstrated by some sociologists (for example, Collins, 1974; Knorr, 1978; Woolgar, 1976), the concept of circumstances has also been developed from a philosophical perspective by Serres (1977). Chapter 2 is an analysis of the circumstances which make stable objects possible in neuroendocrinology; Chapter 3 shows in which networks TRF is able to circulate outside the laboratory in which it was originally constructed; at the end of Chapter 4 we record how the same holds true for the extension of somatostatin. We also point out in Chapter 4 how daily conversations constantly feature local or idiosyncratic circumstances. Finally, in Chapter 5, we use the notion of positions in order to account for the circumstancial character of careers. Rather than being a structure or an ordered pattern, a field consists only of positions which influence each other in a way which is not itself orderly (see pp. 211ff). The notion of position enables us to talk about the "right" time, or the "right" assay, or in Habermas's (1971) terms, to replace the historicity in science (Knorr, 1978).

The sixth and final concept upon which we have drawn is *noise* (or, more exactly, the ratio of signal to noise), which is borrowed from information theory (Brillouin, 1962). Its application to an understanding of scientific activity is not new (Brillouin, 1964; Singh, 1966; Atlan, 1972), but our usage is very metaphorical. We have not, for example, attempted to calculate the signal to noise ratio produced by the laboratory. But we have retained the central idea that information is measured against a background of equally probable events, or as Singh (1966) puts it:

> We measure the information content of a message in any given ensemble of messages by the logarithm of the probability of its occurrence. This way of defining information has an earlier precedent in statistical mechanics where the measure of entropy is identical in form with that of information (Singh, 1966: 73).

The concept of noise fits closely with our observations of participants busily reading the written tracts of inscription devices (see Chapter 2, pp. 48ff). The notion of equally probable alternatives also allowed us to describe the final construction of TRF in Chapter 3: the import of mass spectrometry delimited the number of probable statements. In Chapter 5, the notion of demand, which allowed us to develop the idea of a market for information and to permit the operation of the credibility cycle, was based on the premise that any decrease in the noise of one participant's operation enhances the ability of another participant to decrease noise elsewhere.

The result of the *construction* of a fact is that it appears unconstructed by anyone; the result of rhetorical *persuasion* in the agnostic field is that participants are convinced that they have not been convinced; the result of *materialisation* is that people can swear that material considerations are only minor components of the "thought process"; the result of the investments of credibility, is that participants can claim that economics and beliefs are in no way related to the solidity of science; as to the *circumstances*, they simply vanish from accounts, being better left to political analysis than to an appreciation of the hard and solid world of facts! Although it is unclear whether this kind of inversion is peculiar to science,[10] it is so important that we have devoted much of our argument to specifying and describing the very moment at which inversion occurs.

Having summarised the main arguments of the preceding chapters, it is important now to show how they are related because the concepts above have been borrowed from several different fields.

Let us start with the concept of noise. For Brillouin, information is a relation of probability; the more a statement differs from what is expected, the more information it contains. It follows that a central

question for any participant advocating a statement in the agonistic field is how many alternative statements are equally probable. If a large number can easily be thought of, the original statement will be taken as meaningless and hardly distinguishable from others. If the others seem much less likely than the original statement, the latter will stand out and be taken as a meaningful contribution.[11] When a laboratory member reads a peak on an amino acid analyser, for example (Photograph 9), he first needs to ascertain whether or not he can convince himself (or others)[12] that the peak is different from the background noise. As we have seen, this depends in part on his colleagues. If his claim, "look at this peak," meets with the response, "there is no peak, it is simply noise, you might just as well say that the peak is this little blurr at the other side" (see Photograph 8), his statement has no informative value (in this context).

The sentence which threatens to dissolve all statements (and careers) takes the conditional form: "*but you might as well say* that it is . . . " and precedes a list of equally probable statements. The outcome of this formulation is often the dissolution of the statement in noise. So the objective of the game is to carry out all possible manoeuvres which might force the scientist (or colleagues) to admit that alternative statements are not equally plausible. We discussed some of the manouevres in Chapters 3 and 4. One common manoeuvre is that of *construction*. By showing colleagues, two, rather than one, peaks of an amino acid analysis, or by increasing the distance between the peak and base line, the difference between the various possible statements will also be increased. By being sufficiently convincing, people will *stop* raising objections altogether, and the statement will move toward a fact-like status. Instead of being a figment of one's imagination (subjective), it will become a "real objective thing," the existence of which is beyond doubt.[13]

The operation of information construction, then, transforms any set of equally probable statements into a set of *unequally* probable statements. At the same time, this operation draws upon the activities of persuasion (agonistic) and of writing (construction) in order to increase the signal to noise ratio.

How can inequality be introduced into a set of equally probable statements in such a way that a statement is taken to be more probable than all the alternatives? The technique most frequently used by our scientists was that of *increasing the cost* for others to raise equally probable alternatives. In Chapter 3, for example, we showed that the imposition of new standards on the field of releasing factors effectively ruined competitors' efforts. Similarly, when Burgus used mass spectrometry to make a point, he made it difficult to raise alternative possibilities because to do so would be to contest the whole of physics. Once a slide has been shown with all the lines of the spectrum corresponding to one atom of the amino acid sequence, no one is likely to stand up and object.[14] The controversy is settled. But if a slide is presented which shows the spots of a thin-layer chromatography, ten chemists will stand up and assert that "this is not a proof." The difference, in the second case, is that any chemist can easily find fault in the method used (but see the Donohue episode, p. 171).

This point would clearly be tautological but for the central notion of materialisation or *reification* which we defined earlier and can now use at its best. The mass spectrometer is the reified part of a whole field of physics; it is an actual piece of furniture which incorporates the majority of an earlier body of scientific activity. The cost of disputing the generated results of this inscription device has been enormous. Indeed, this explains by Guillemin and Burgus strived from the beginning to "get at the mass spectrometer." In the case of thin layer chromatography, however, very little earlier interpretative work has been reified. Consequently, it is easy to contest any step in arguments based on a chromatograph and to propose an alternative argument. Once a large number of earlier arguments have become incorporated into a black box,[15] the cost of raising alternatives to them becomes prohibitive. It is unlikely, for example, that anyone will contest the wiring of the computer shown in Photograph 11, or the statistics on which the "t" test is based, or the name of the vessels in the pituitary.

The operation of black-boxing is made possible by the availability of credibility (Ch. 5). As we argued earlier, credibility is a part of the wider phenomenon of credit, which refers to money, authority, confidence and, also marginally, to reward. The first question raised when a statement is proposed, is how much the statement and/or its author can be credited. This question is directly analogous to the question of cost mentioned above: what sort of investments should be made so as to fabricate a statement of equal probability to that of a competitor? In a million-dollar business like the sequencing of TRF, the chances are that no alternative statement is feasible. The constraints are such that no investment could possibly match those already made. Consequently, statements which are already credited will be taken for granted. In addition, they will be used to make points

in other laboratories. This is the nature of the market defined in Chapter 5. No matter whether this taken-for-granted peptidic structure takes the form of a nonproblematic argument or of a white powder sample, the only important question is whether borrowing it (or buying it) will make it more difficult for a competitor to contest statements.

Of course, the concepts of cost, reification, and credit have to be understood in the light of our earlier argument: everything which has been accepted, *no matter for what reason*, will be reified so as to increase the cost of raising objections. For instance, the standing of one scientist might be such that when he defines a problem as important, no one feels able to object that it is a trivial question; consequently, the field may be moulded around this important question, and funds will be readily forthcoming. In the Donohue episode, chemists' preference for the enol form for the four DNA bases was stabilised and reified in textbooks, such that it was more difficult for Watson to doubt it or simply to object that the keto form was equally probable. The cost-benefit analysis will vary according to the prevailing *circumstances*, so no general rules can be established. The style of an article can make it more difficult for the reader not to believe in it; the qualification of statements can disarm readers' objections; for another audience, documentation through the use of footnotes can add conviction; competitors can even be silenced by imprisonment or fraud (Lecourt, 1976). The major rule of the game is to assess the cost of investments compared with their likely return; the game is not played according to a set of ethical rules, which a superficial examination reveals.[16]

The portrayal resulting from the above combination of concepts used throughout our argument has one central feature: the set of statements considered too costly to modify constitute what is referred to as reality. Scientific activity is not "about nature," it is a fierce fight to *construct* reality. The *laboratory* is the workplace and the set of productive forces, which makes construction possible. Every time a statement stabilises, it is reintroduced into the laboratory (in the guise of a machine, inscription device, skill, routine, prejudice, deduction, programme, and so on), and it is used to increase the difference between statements. The cost of challenging the reified statement is impossibly high. Reality is secreted.[17]

So far we have summarized the main points of our argument by showing how six of the major concepts we have used are related and, finally, by zooming in on the notion of laboratory from which we started in the second chapter. There is, however, an alternative way of describing laboratory life which draws primarily on one single concept.

Order From Disorder

The transformation of a set of equally probable statements into a set of unequally probable statements amounts to the creation of order (Brillouin, 1962; Costa de Beauregard, 1963; Atlan, 1972). Let us now provide a new account of laboratory life using the notion of order together with Brillouin's famous mythical character: Maxwell's demon. The simplest version is the following (Singh, 1966):

> A demon placed in a cold oven would be able to increase the amount of heat by allowing the swifter molecules to gather in one part of the oven and by keeping them there. In order to do this, the demon needs information about the state of the molecules, a small trap which will let them come or go depending on their quality, and an enclosure in which to prevent the sorted molecules from escaping and returning to their random state. We now know that the demon himself consumes a small amount of energy in doing his work. "It is impossible to get something for nothing, even information," as the saying goes.

This account provides an illuminating analogy with what goes on in the laboratory. We have already seen the laboratory to be an enclosure where previous work is gathered. What would happen if this enclosure was opened? Imagine that the following experiment was carried out by our observer. Entering the deserted laboratory at night, he opens one of the large refrigerators shown in Photograph 2. As we know, each sample on the racks corresponds to one stage of the purification process and is labelled with a long code number which refers back to the protocol books. Taking each sample in turn, the observer peels off the labels, throws them away and returns the naked samples to the refrigerator. Next morning, he would doubtless witness scenes of extreme confusion. No one would be able to tell which sample was which. It would take up to five, ten, and even fifteen years (the time it took to label the samples) to replace the labels—unless, of course, chemistry techniques had advanced in the mean time. As we stated earlier, any sample might equally well be any other. In other words, the disorder, or more precisely the entropy, of the laboratory would have increased: anything could be said about each and every sample. This nightmarish experiment highlights the importance of the

trapping system for any competent Maxwell's demon wishing to decrease disorder.[18]

At this point, we can perhaps do justice to the apparently strange notion of *inscription* introduced in Chapter 2. Our argument there was that writing was not so much a method of transferring information as a material operation of creating order. Let us illustrate the importance of writing by reference to an experiment undertaken by the observer during his stay in the laboratory. As we mentioned in Chapter 1, the sociologist worked as a technician during his participant observation. Fortunately for us, the observer turned out to be an extremely bad technician in a very efficient laboratory. Consequently, his deficiencies highlighted the roots of his informants' competence. One of the most difficult tasks was the dilution and addition of doses to the beakers. He had to remember in which beaker he had to put the doses, and made a note, for example, that he had to put dose 4 in beaker 12. But he found that he had forgotten to make a note of the time interval. With pipette half lifted, he found himself wondering whether he had *already* put dose 4 in beaker 12. He blushed, trying to remember whether he had made a note before or after the actual action took place; obviously, he had not made a note of when he had made a note! He panicked and pushed the piston of the pasteur pipette into beaker 12. But maybe he had now put *twice* the dose into the beaker. If so, the reading would be wrong. He crossed out the figure. The observer's lack of training meant that he continued in this fashion. Not surprisingly, the resulting points exhibited wide scatter. A day's work had been lost. It is necessary to be a technician, and an incompetent one at that, in order fully to appreciate the practical miracle (in Boltzmann's sense of the word) which gives rise to a standard curve. A wealth of invisible skills ~~underpin material~~ inscription. Every curve is surrounded by a flow of disorder, and is only saved from dissolution because everything is written or routinised in such a way that a point cannot as well be in any place of the log paper. But the unhappy observer was not party to these constraints! Instead of creating more order, he had only succeeded in creating less; and, in the meantime, he had used up animals, chemicals, time, and money.

Even insecure bureaucrats and compulsive novelists are less obsessed by inscriptions than scientists. Between scientists and chaos, there is nothing but a wall of archives, labels, protocol books, figures, and papers.[19] But this mass of documents provides the only means of creating more order and thus, like Maxwell's demon, of increasing the amount of information in one place. So it is easy to appreciate their obsession. Keeping track is the only way of seeing a pattern emerge out of disorder (Watanaba, 1969). It might be impossible to differentiate any of a thousand equally active peptides out of a soup of unpurified brain extracts. If assays designed to separate out one of these peptides were carefully carried out but not recorded, the technicians would have to start all over again; there would be no way of discriminating between statements because there would be no superimposition of traces and consequently no construction of an object. When, by contrast, a series of curves have been recorded, and it is possible to spread them out on the large library table and ponder them, then an object is in the process of construction. Objects appear because of the constant process of sorting. Thin readable traces (produced by the inscription devices) are recorded and this creates a pocket of order in which not everything is equally probable. In view of eight years' worth of documents and a million dollars' worth of equipment, the range of possible statements which can be made about the structure of TRF is restricted. The cost of selecting a statement from outside this range is prohibitive.

Maxwell's demon provides a useful metaphor for laboratory activity because it shows both that order is *created* and that this order in no way preexists the demon's manipulations. Scientific reality is a pocket of order, created out of disorder by seizing on any signal which fits what has already been enclosed and by enclosing it, albeit *at a cost*. In order fully to explore the force of this model, however, it is necessary to examine the relation between order and disorder in more detail. Disorder is not only the noise in which statements made by inefficient technicians are dissolved; paradoxically, the laboratory is also involved in the production of disorder. By recording all events and keeping traces from all the inscription devices, the laboratory overflows with computer listings, data sheets, protocol books, diagrams, and so on. Even if it successfully resists the outside disorder, the laboratory itself generates disorder within its enclosure. The noise of thousands of brain extracts, is replaced by the noise of accumulated data. Information again seems like the elusive needle in a haystack. No patterns emerge. Participants' solution to this danger is selectively to eliminate material from the mass of accumulated data. Here is the importance of the statements, the genealogy of which we outlined in Chapter 2. The problem is not now to discern a peak from the background noise (the baseline), but to read a sentence out of the mass of gathered peaks and

curves. One particular curve is selected, cleaned up, put on a slide and shown around in conjunction with the statement: "Stress simultaneously releases ACTH and Beta Endorphine." This statement stands out of and for the mass of figures. A paper begins to be drafted, which constitutes a second-degree enclosure (an enclosure represented in Fig. 2.1 by the laboratory partitions).

Sorting, picking up and enclosing are costly operations, and they are rarely successful; any slackening can once again drown a statement in confusion. This is more so because a statement exists, not by itself, but in the agonistic field (or market, Ch. 5) made up of the laboratories striving to decrease their own noise. Is the statement going to stand out in the field or will it merely once again be drowned in the mass of literature on the subject? Perhaps it is already redundant, or simply wrong. Perhaps it will never be picked out from the noise. The laboratory's production process again seems chaotic: statements have to be pushed, forced into the light, defended against attack, oblivion, and neglect. Very few statements are seized upon by everyone in the field because their use entails an enormous economy in the manipulation of data or statements (Brillouin, 1962: Ch. 4). These statements are said to "make sense" or "to explain a lot of things" or to allow a dramatic decrease in the noise of one inscription device: "now we can obtain reliable data." Such very rare events, the sorting of facts from the background noise, are often heralded by the Nobel Prizes and a flourish of trumpets.

Maxwell's demon *creates* order. This analogy not only provides a way of summarising and relating the main concepts used in our earlier description of laboratory activity; it also helps answer the objection that we have not explained why a controversy becomes resolved, or why a statement stabilises. But this objection only has meaning in so far as it is assumed that order somehow preexists its "revelation" by science, or in some way results from something other than disorder. This basic philosophical assumption has recently been challenged, and our intention in the next part of this chapter is to show what light is shed on laboratory activity if such an assumption is modified. To do this in full would entail going beyond the usual range of argument in sociology of science, and certainly beyond the scope of this monograph. We therefore restrict our discussion to one further analogical description of the laboratory.

Figure 6.1 shows three stages of a game of "go" as related by Kawabata (1972). The game of go starts from an empty board to which

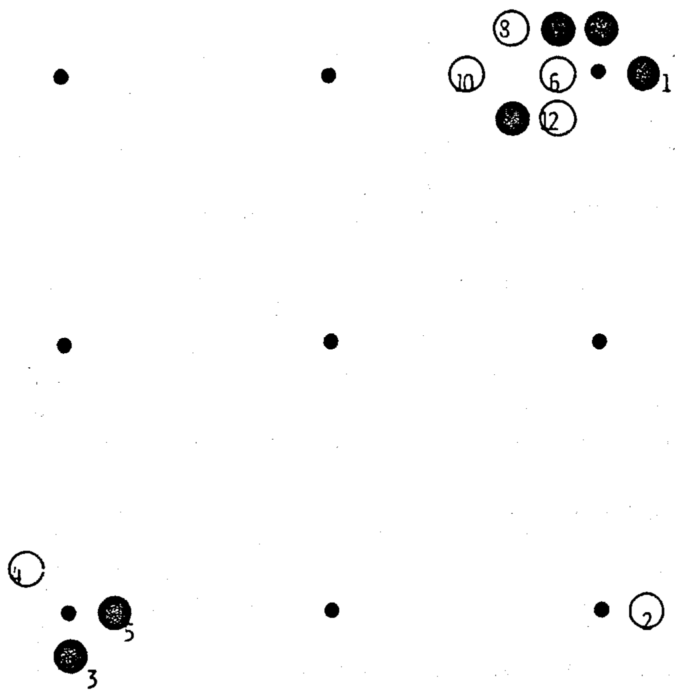

Figure 6.1a

stones are added in successive moves. The added stones do not move around the board as, for example, in chess. Consequently, the first moves are almost entirely contingent (Figure 6.1a). As the game progresses, however, it becomes less and less easy to play anywhere; as in the agonistic field, the results of earlier play transforms the set of future possible moves. Not all moves are equally possible (Figure 6.1b). Indeed, some are totally impossible (for example, white cannot play on the upper left hand corner), others are less likely, and some are almost necessary (for example, play at 64 after 63 in Fig. 6.1c). As in the agonistic field, the changing pattern is not orderly: in the lower right hand corner or in the middle of the board, it is possible to play almost anywhere; but the situation in the left hand corner is definitively settled. A territory may or may not be defended according to the

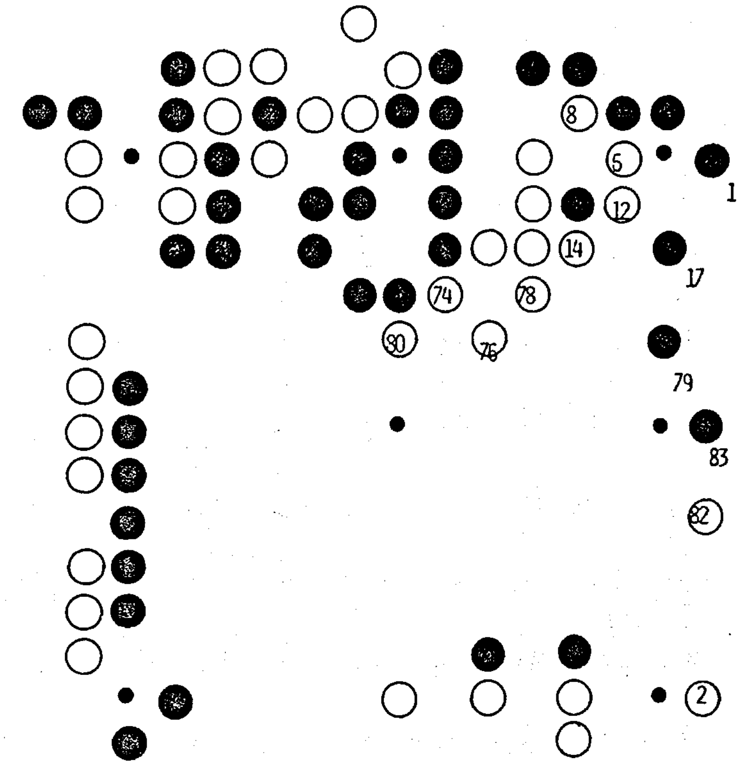

Figure 6.1b

pressures exerted by the opponent. The game ends when all territory has been appropriated (Figure 6.1c) and all disputed territories have been settled (for example, the stones at the top). From an entirely contingent beginning, the players arrive (without the use of external or preexisting order) at a final point in the game where certain moves are *necessary*. In principle, any individual move could be made anywhere; in practice, the cost of spurning what appears the necessary move is prohibitive.[20]

The relationship between order and disorder, which underpins our account of the construction of facts, is very familiar to biologists (Orgel, 1973; Monod, 1970; Jacob, 1977; Atlan, 1972). That life is an orderly pattern emerging from disorder through the sorting of random mutations, is the stock in trade of the biological representation of life.

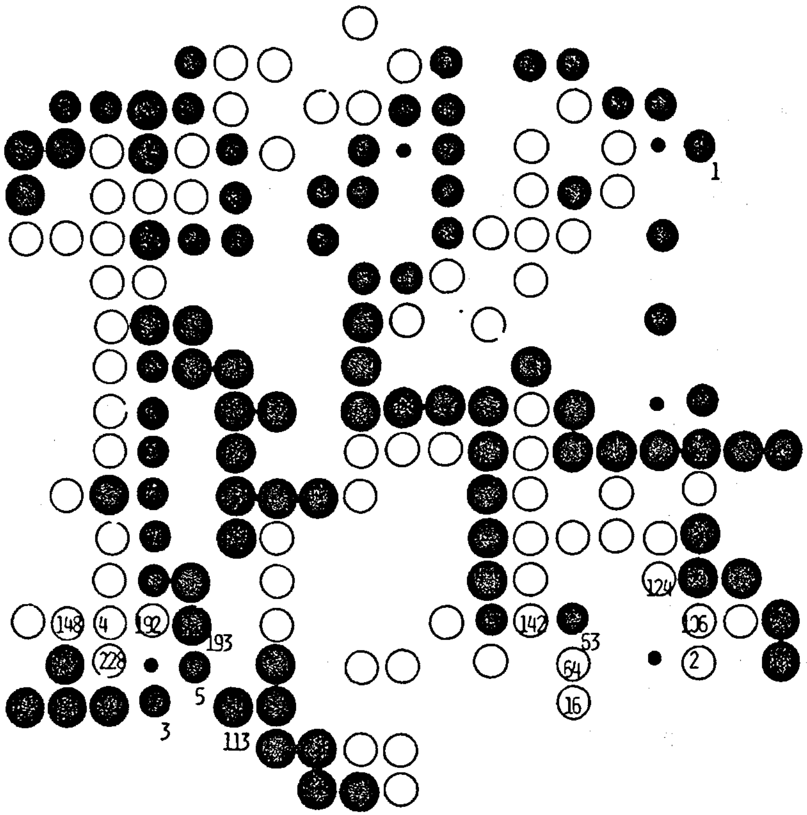

Figure 6.1c
Figure 6.1a-c is taken from the novel by Kawabata (1972). It shows three moments in the unfolding of a game of "Go." 6.1a shows the board at the 10th move; 6.1b at the 80th; and 6.1c at the end. The game of "Go" provides a model for the construction of orderly but unpredictable forms. The same stones appear in each of the three diagrams. The most important moves are signalled by numbers.

For Monod, for example, chance (disorder) and necessity (a sorting mechanism) are sufficient to account for the emergence of complex organisation. Reality is constructed out of disorder, without the use of any preexisting representation ~~of~~ life. Many of the laboratory members themselves used terms such as chance, mutation, niches, disorder, and tinkering (Jacob, 1977) to account for life itself. But sociologists of science seem extremely ~~reluctant to introduce similar concepts~~ to account for the construction of reality.[21] ~~After all, the construction~~ of reality might be no ~~more complex than the generation of~~ organisms.

The three brief analogies drawn above (Maxwell's devil, the game of Go, and Monod's notion of chance and necessity) were intended simply as a way of familiarising the reader with the slight modification of background which is well known in many other disciplines, but which seems to have escaped the attention of analysts of science.

It is part of our world view that things are ordered, that order is the rule, and that disorder should be eliminated wherever possible. Disorder always has to be eliminated from politics and ethics as well as from science. It is also part of our world view that only from disorder can an orderly pattern emerge. These assumptions have recently been challenged by several philosophers, especially Michel Serres, who, in turn, have been greatly influenced by authors such as Brillouin and Boltzmann and by new developments in biology. Their argument is that these assumptions be inverted, that disorder be considered the rule and order the exception. This argument has been familiar since life was first considered to be a neguentropic event which fed off the much larger and opposite trend towards entropy. Recently, this picture has been extended to include science itself as a marginal case of a certain type of social organism, a particular but not a peculiar case of neguentropy (Monod, 1970; Jacob, 1977; Serres, 1977a; 1977b). For our purposes, the interesting part of this argument is the contention that the construction of order relies upon the existence of disorder (Atlan, 1972; Morin, 1977). If one accepts this suggested modification, it is possible to discern a marked convergence between our approach and apparently disparate approaches to the social study of science.[22] Let us consider four such approaches.

Firstly, the history of science can be characterised as demonstrating the chain of circumstances and unexpected events leading to this or that discovery. However, this mass of events is not easily reconciled with the solidity of the final achievements. This is one reason why the context of justification is so frequently opposed to the context of discovery. With the above modification of our background assumption, this opposition is no longer necessary (Feyerabend, 1975; Knorr, 1978). To use Toulmin or Jacob's analogies, if life itself results from tinkering and chance, it is surely not necessary to imagine that we need more complex principles to account for science. The "*évènementialisation*" (Foucault, 1978) of science made by historians penetrates the core of fact construction. Secondly, sociologists have demonstrated the importance of informal communication in scientific activity. This well-documented phenomenon takes on a new meaning against the newly modified assumption: the production of new information is necessarily obtained by way of unexpected meetings, through old boy networks and by social proximity. The informal flow of information does not contradict the orderly pattern of formal communication. Instead, as we have suggested, much informal communication derives its structure from its constant referral to the substance of formal communication. Nonetheless, informal communication *is the rule*. Formal communication is the exception, as an *a posteriori* rationalisation of the real process. Thirdly, citation analysts have demonstrated the extensive waste of energy in scientific activity. Most published papers are never read, the few that are read are worth little, and the remaining 1 or 2 percent are transformed and misrepresented by those who use them. But this waste no longer appears paradoxical if we accept the hypothesis that order is an exception and disorder the rule. Few facts emerge from the substantial background noise. The circumstances of discovery and the process of informal exchange are both crucial to the productive process: they are what allows science to exist at all. Finally, growing sociological interest in the details of negotiation between scientists has revealed the unreliability of scientists' memories and the inconsistency of their accounts. Each scientist strives to get by amid a wealth of chaotic events. Every time he sets up an inscription device, he is aware of a massive background of noise and a multitude of parameters beyond his control; every time he reads *Science* or *Nature*, he is confronted by a volume of contradictory concepts, trivia, and errors; every time he participates in some controversy, he finds himself immersed in a storm of political passions. This background is everpresent, and it is only rarely that a pocket of stability emerges from it. The revelation of the diversity of accounts and inconsistency of scientific arguments should therefore come as no surprise: on the contrary, the emergency of an accepted fact is the rare event which should surprise us.

A New Fiction For Old?

We have so far in this chapter summarised the arguments of the former chapters, showed how they are related through the notion of the construction of order out of disorder and linked them to what has been done in sociology of science. We shall now summarise the methodological problems encountered in the course of our argument, looking in particular at the thorny issue of the status of our own account. What is the basis for our claim that scientists produce order from disorder? Obviously, our own account cannot escape the conditions of its own construction. From what kind of disorder does our account emerge? In which agonistic field are we to put together differences between fiction and fact?

Throughout the argument, we have stressed the importance of avoiding certain distinctions commonly adopted by analysts of scientific activity. In Chapter 1, we refused to accept the distinction between social and technical issues; in Chapter 2, we had to suspend any given distinction of nature between facts and artefacts; in Chapter 3, we demonstrated that the difference between internal and external factors was a consequence of the elaboration of facts rather than a given starting point for understanding their genesis; in Chapter 4, we argued for the suspension of a priori distinctions between common sense and scientific reasoning; even the distinction between "thought" and craftwork needed to be avoided as an explanatory resource because it appeared to be the *consequence* of scientific work in the laboratory; similarly, in Chapter 5, we argued that the notion of scientists as individuals was the consequence of the appropriation conflicts within the laboratory.

Stylistically, the replacement and avoidance of these obsolete distinctions presented severe difficulties. In allying our discussion to each of certain literary genres (for example, the "historical" discussion of Ch. 3), we found ourselves constrained by using terminology which tended to reintroduce these distinctions. For this reason, it was necessary to look carefully at our own usage of words. For example, the term social has connotations which make it difficult to avoid importing distinctions, such as that between social and technical. Similarly, the term familiar obscures the particular sense with which we wanted to apply the notion of an anthropology of science. In Chapter 3, in particular, we had to resist terminology commonly employed in historical accounts because it had the tendency of transforming constructed facts into "discovered" facts. In Chapter 4, the use of the expression "I had an idea" or the tautological use of "scientific" was sufficient to destroy the tenor of our argument. Consequently, it was necessary to dispute some of the terms used by epistemologists. By employing the word credit and by exploring its various different meanings, we circumvented some of the distinctions which usually come to mind when one uses terms such as strategy, motivations, and careers.

We have thus tried to exercise some care in discriminating between the kinds of terms and distinctions which might jeopardise our account of laboratory life. However, we have as yet to clarify what differentiates our account of laboratory life from those routinely produced by scientists. Is there any essential distinction between the nature of our own construction and that used by our subjects? Emphatically, the answer must be no. Only by rejecting the possibility of this last distinction can the arguments of this chapter cohere. The notion of creating order from disorder applies as much to the construction of our own account as to that of the laboratory scientists. How then do we know how they know?

How have we built up our account of fact production whereby laboratory scientists get by with fictions which they push as hard as they can in the agonistic field?

If we return to the situation (described in Ch. 2) where the naive observer visited the "strange" laboratory, it is clear that he constructed his preliminary accounts out of disorder. He neither knew what to observe, nor the names of the objects in front of him. In contrast to his informants, who exhibited confidence in all their actions, our observer felt distinctly uneasy. He found himself wondering where to sit, when to stand, how to present himself, and what questions to ask. A flood of gossip, anecdotes, lectures, explanations, impressions, and feelings emerged from his initial daily contact with the laboratory. Subsequently, however, he began to set up a crude inscription device to monitor these data. He found himself as observer connected up to a screen (his notebooks), the effects being recorded by means of amplification (such as his definition of assays). But these first "socioassays" were extremely noisy and chaotic. The early notebooks reveal the confusion of the first recordings: trivia, generalities, noise, and more noise.

The observer was obliged to create some stable pockets of order out of this flood of impressions. He attempted this, first by a crude imitation of the method of his informants: he plotted time on one axis of a piece of graph paper and wrote the names of the scientists on the other. Armed with a watch, he inscribed who did what and when. In this way he began to produce ordered information. In another instance, he distilled the pattern of citations received by group members from the mass of citation data in the SCI. Like any conscientious Maxwell's devil, he filtered the names he required, counted the citations and inscribed them in columns. One result was Figure 5.3: a relatively modest achievement, admittedly, but one which granted him a brief moment of contentment. On the basis of this result, he could make a statement: when his informants objected that the claim was nonsensical, he was then able to produce the figure and this had the effect of quietening his audience, at least temporarily.

In the course of a few months, our observer accumulated a sizeable body of similar figures, documents, and other notes. In terms of the analogy with "go" he began to fill his board with random moves. Consequently, as he progressed further, he realised that it was no longer possible to make just *any* statement on the basis of this accumulated material. In addition, our observer found himself able either to counter or support some of the arguments in the science studies literature. He could also transform them into artefacts or facts with the use of the objects he had begun to amass. He began to write articles and to operate in his own agonistic field. At this stage, however, his accounts were so weak that any other account seemed equally plausible. Moreover, his informants flooded him with contradictory examples and argued for alternative interpretations.

By returning to the initial stages of the study, then, we can discern an essential similarity between the methods of the observer and his informants. Even so, it is not clear who was imitating whom. Were the scientists imitating the observer, or vice versa?

As mentioned earlier, part of the observer's experience involved his participation as laboratory technician. From time to time he could don a white coat, go into the biassay room, and set up an assay for the melanotropin stimulating hormone (MSH) instead of drawing citation curves and transcribing interviews. (MSH darkens frog skin, as measured by variations of light in a reflectometer.)

The observer had his protocol book and an empty data sheet in front of him. He seized the jumping frogs, beheaded, and flayed them, and finally immersed thin sections of skin in the beakers. He placed each of the beakers over a source of light and took readings from the reflectometer, which he then wrote down. By the end of the day, he had accumulated a small stack of figures which could be fed into the computer (Photograph 11). After this he was left only with standard deviations, levels of significance, and means in the computer listing. On the basis of these he drew a curve and, taking it into his boss's office, argued about the slight differences or similarities in the curve in order to make a point.

Some similarities between the construction of the citation curve and that of the standard curve for MSH are obvious. Thus, the following features are common to both activities. Inscription devices were set up: five or ten names were singled out of the millions in the SCI (only a few pieces of skin were taken from the complexity of the frog organism); the investigator placed a premium on those effects which were recordable; the data were cleaned up so as to produce peaks which were clearly discernible from the background; and, finally, the resulting figures were used as sources of persuasion in an argument. These similarities make it difficult to maintain that there is any fundamental difference between the methods of "hard" and "soft" science.

The similarity of his two roles began to prove unnerving. Our observer sometimes felt himself completely assimilated into "his" laboratory: he was addressed as "doctor," possessed protocol books and slides, submitted papers, met colleagues at congresses and busied himself setting up new inscription devices and filling in questionnaires. On the other hand, he was painfully aware of the enormous distance between the apparent solidity of his informants' constructions and his own. In order to study half a gram of brain extract, they had at their disposal tons of material, millions of dollars, and a large group of some forty people; in order to study the laboratory, our observer was alone. At the bench, working on the MSH assay, people would constantly peer over his shoulder and criticise him ("don't hold your pipette like that"; "let me redo your dilution"; "check this reading again") or direct his attention to one of the sixty articles written about the assay.[23] While tinkering a few makeshift methods for analysing the work of the laboratory, he had few general contacts and no precedent upon which he felt he could build. The scientists had a laboratory, in which were gathered all the stable objects of their field, and free access to the object under construction; the observer had no such resources. Moreover, he had to settle in the laboratory used as a resource by the scientists and to beg information as a stranger, a foreigner, and a layman.

The difference in credibility accorded the observer's and the informants' constructions corresponds directly to the extent of prior investments. Occasionally, when members of the laboratory derided the relative weakness and fragility of the observer's data, the observer pointed out the extent of the imbalance between the resources which the two parties enjoyed. "In order to redress this imbalance, we would require about a hundred observers of this one setting, each with the same power over their subjects as you have over your animals. In other words, we should have TV monitoring in each office; we should be able to bug the phones and the desks; we should have complete freedom to

take EEGs; and we would reserve the right to chop off participants' heads when internal examination was necessary. With this kind of freedom, we could produce hard data." Inevitably, these kinds of remarks sent participants scurrying off to their assay rooms, muttering darkly about the "Big Brother" in their midst.

Gradually, the observer gained confidence in his work: he was both adding to the stockpile of inscriptions in his office and beginning to realise that there was nothing special or mysterious about the difference between his activity and that of his informants. The essential similarity was that both were engaged in craftwork; differences could be explained in terms of resources and investments, and without recourse to exotic qualities of the nature of the activity. Consequently, the observer began to feel less intimidated. When his informants were interpreting traces on the library table, for example, they really seemed little different to him; they pondered diagrams, putting some aside, evaluating the strength of others, seizing on weak analogical links, and so slowly constructed an *account*. At the same time, the observer was writing a fictional account on the basis of makeshift curves and documents. Informants and observer shared participation in the art of interpreting confused texts (texts comprising slides, diagrams, other paper, and curves) and of writing persuasive accounts.[24]

Our account of fact construction in a biology laboratory is neither *superior nor inferior* to those produced by scientists themselves. It is not superior because we do not claim to have any better access to "reality," and we do not claim to be able to escape from our description of scientific activity: the construction of order out of disorder at a cost, and without recourse to any preexisting order. In a fundamental sense, our own account is no more than *fiction*.[25] But this does not make it inferior to the activity of laboratory members: they too were busy constructing accounts to be launched in the agonistic field, and loaded with various sources of credibility in such a way that once convinced, others would incorporate them as givens, or as matters of fact, in their own construction of reality. Nor is there any difference in the sources of credibility upon which they and we can draw so as to force people to drop modalities from proposed statements. The only difference is that *they have a laboratory*. We, on the other hand, have a text, this present text. By building up an account, inventing characters (for example, the observer of Ch. 2), staging concepts, invoking sources, linking to arguments in the field of sociology, and footnoting, we have attempted to decrease sources of disorder and to make some statements more likely than others, thereby creating a pocket of order. Yet this account itself will now become part of a field of contention. How much further research, investment, redefinition of the field, and transformation of what counts as an acceptable argument are necessary to make this account more plausible than its alternatives?

NOTES

1. This point has been made frequently by Bachelard (for example, 1934; 1953). However, his interest in demonstrating the "mediations" in scientific work was never extended. His "rational materialism," as he put it, was more often than not the basis for distinguishing between science and "prescientific" ideas. His exclusive interest in "la coupure épistémologique" prevented him from undertaking sociological investigations of science, even though many of his remarks about science make better sense when set within a sociological framework.

2. From the outset, the observer was struck by the almost absurd contrast between the mass of the apparatus and the minute quantities of processed brain extract. The interaction between scientific "minds" and "nature" could not adequately account for this contrast.

3. In a different context, the importance of the stakes may vary. For example, the importance of somatostatin for the treatment of diabetes ensures that each of the group's articles is carefully checked. In the case of endorphine, by contrast, any article (no matter what the wildness of its conjectures) will initially be accepted as fact.

4. On his first day in the laboratory, the observer was greeted with a maxim which was constantly repeated to him in one or another modified form throughout his time in the field: "The truth of the matter is that 99.9% (90%) of the literature is meaningless (crap)."

5. We base this argument on several conversational exchanges which took place between lawyers and scientists. Unfortunately, we are not able to make explicit use of this material here.

6. It is crucial to our argument that anything can be reified, no matter how mythical, absurd, whimsical, or logical it might seem either before or after the event. Callon (1978), for example, has shown how technical apparatus can incorporate the outcome of totally absurd decisions. Once reified, however, these decisions take the role of premise in subsequent logical arguments. In more philosophical terms, one cannot understand science by accepting the Hegelian argument that "real is rational."

7. But for a few pages in Lacan (1966) and some indirect hints by Young (n.d), a psychoanalytic understanding of these kinds of energy investments is as yet undeveloped.

8. For example, Machlup (1962) and Rescher (1978) have attempted to understand the information market in economic terms. However, their approach extends rather than transforms the central notion of economic investment. By contrast, Bourdieu (1976) and Foucault (1978) have outlined a general framework for a political economy of truth (or of credit) which subsumes monetary economics as one particular form of investment.

9. The philosophical enterprise can be characterised as an attempt to eliminate any trace of circumstances. Thus, the task of Socrates in Plato's *Apology of Socrates* is to eliminate circumstances included in the definition of activity provided by the artist, the lawyer, and so on. Such elimination is the price which has to be paid in order to establish the existence of an "idea." Sohn Rethel (1975) has argued that such philosophical operations were essential for the development of science and economics. It could be argued, therefore, that the task of reconstructing circumstances is fundamentally hampered by the legacies of a philosophical tradition.

10. Barthes argues that this kind of transformation is typical of modern economics. It is thus possible that there is some useful similarity between Marx's (1867) notion of fetishism and the notion of scientific facts. (Both fact and fetish share a common etymological origin.) In both cases, a complex variety of processes come into play whereby participants forget that what is "out there" is the product of their own "alienated" work.

11. Brillouin uses the word likely in a counterintuitive way. It is only if a statement is unlikely that it contains information since its distance from the background of equally probable statements is very great. In ordinary language, however, we might say that people believe a statement when it is more likely than the others. The reason for this apparent contradiction is that information is nothing but a ratio of signal to noise.

12. In the course of our discussion, we have tried to minimise distinctions between convincing ourselves and convincing others. In interviews the continuous shortcuts between the two were so common ("I wanted to be sure, and I did not want W to stand up and contradict me"), that we gave up making this artificial distinction. Our experience suggests that, perhaps in the most secret part of his consciousness, a scientist argues with the whole agonistic field and anticipates every single one of his colleagues' potential objections.

13. This formulation closely matches scientists' own impression of a messy field: it is a field in which you can say *anything* or, more precisely, in which *anyone can* equally well say anything.

14. This is not to say that it is impossible in principle to contest the argument based on the use of a mass spectrometer. But the cost of modifying the basis of the theory is so high that, in practice, no one will challenge it. (The exception, perhaps is in the case of a scientific revolution.) The difference between what is possible in principle and what can be done in practice is the lynchpin of our argument. As Leibnitz put it: "Everything is possible, but not everything is compossible." The process by which the realm of compossibility is extended was explored in Chapter 3. The mass spectrometer is no more truthful than thin-layer chromotography; it is simply more powerful.

15. The term "black box" also brings to mind Whitley's (1972) argument that sociologists of science should not treat the cognitive culture of scientists as a self-contained entity immune from sociological investigation. Although we sympathise with this view, Whitley misses a crucial point. The activity of creating black boxes, of rendering items of knowledge distinct from the circumstances of their creation, is precisely what occupies scientists the majority of the time. The way in which black boxing is done in science is thus an important focus for sociological investigation. Once an item of apparatus or a set of gestures is established in the laboratory, it becomes very difficult to effect the retransformation into a sociological object. The cost of revealing

sociological factors (the cost, for example, of portraying the genesis of TRF) is a reflection of the importance of the black boxing activities of the past.

16. This is why we do not need different sets of rules by which to account for the political world and the scientific world. Similarly, we consider scientists' honesty and dishonesty from a single analytical perspective. Fraud and honesty are not fundamentally different kinds of behaviour; they are strategies whose relative value depends on the circumstances and the state of the agonistic field.

17. If reality means anything, it is that which "resists" (from the Latin "res"—thing) the pressure of a force. The argument between realists and relativists is exacerbated by the absence of an adequate definition of reality. It is possible that the following is sufficient: that which cannot be changed at will is what counts as real.

18. Although Brillouin is largely unknown among sociologists of science, he has made important contributions to a materialist analysis of science production. He regards *all* scientific activity (including the so-called "intellectual" or "cognitive" ones) as material operations which are in any way homologous to the usual object of physics. Since he provides a bridge between matter and information, he also bridges the gap—so dramatic for the study of science—between intellectual and material factors.

19. Even bench work can best be analyzed in terms of staging and writing. The samples are put into coloured racks on one side of the surgical table, and are moved slowly. The movement is monitored by a stop watch and recorded on a sheet of paper. Even at this level, possible objections are being countered by the set of precautions exercised in conducting this work (see Photograph File).

20. Many other aspects of the Go game analogy could be applied to the work of science. The main advantage of the analogy is that it provides an approximate illustration of the contingency/necessity dialectic. A further advantage is its illustration of the reification process in science. In Figure 6.1c, for example, the stone played at the fourth move lies next to another played at the 148th move. A group of white stones have been surrounded and are removed from the board.This approximates the movement of contradiction as shown in Chapter 3; whether or not a given formation is seen as contradictory (and requires elimination) will depend on the local context and on the pressures of the agonistic field. In this case, elimination will result from black's decision to play at a certain position.

21. One of the main interests of the field study is that the sociological work could be pursued hand-in-hand with the biological research of the institute. But it was clear to the observer that both his informants and his sociological colleagues were claiming to be doing science. The problems raised by this complicated relationship will be examined in detail elsewhere.

22. Our claim is not that we are advancing an original "paradigm" for the analysis of science. We simply aim to show how close our anthropological position is to other studies broadly named "sociology of science." Our impression is that the main approaches followed so far are (a) not connected to one another; (b) somewhat undecided on what is the final status of their findings. The slight, but radical, modification of background that we entertain here might provide a vantage point from which the importance of these findings can be fully appreciated.

23. This was due, in part, to the observer's isolation and lack of training and, in part, to the lack of any former anthropological studies of modern science. One particularly useful source was Auge's (1975) analysis of witchcraft in the Ivory Coast, which provides an intellectual framework for resistance to being impressed by scientific endeavour.

24. It seems that the basic prototype of scientific activity is not to be found in the realm of mathematics or logic but, as Nietzsche (1974) and Spinoza (1667) frequently pointed out, in the work of exegesis. Exegesis and hermeneutics are the tools around which the idea of scientific production has historically been forged. We claim that our empirical observations of laboratory activity fully support that audacious point of view; the notion of inscription, for example, is not to be taken lightly (Derrida, 1977).

25. "Fiction" is to be taken as having a noncommitted or "agnostic" meaning that can be applied to the whole process of fact production but to none of its stages in particular. The production of reality is what concerns us here, rather than any one produced final stage (stage 5 in the terminology of Ch. 2). Our main interest in using the word "fiction" is the connotation of literature and writing accounts. De Certeau once said (pers. com.), "There can only be a science of science-fiction." Our discussion is a first tentative step towards making clear the link between science and literature (Serres, 1977).